A

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OF

NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

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JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS:

ILLUSTRATED WITH ENGRAVINGS.

BY WILLIAM NICHOLSON.

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Obfervations in proof of the affertion, that roots afford more faccharine matter when defended from light. Theoretical explanation from the lefs quantity of oxygen being feparated in thefe circumftances. Recapitulation.

III. On the Decompofition of the Acid of Borax, or Sedative Salt. By Laurence de Crell, M.D. F.R.S. London and Edinburgh. (Concluded from page 257) - - - - - p. 292

The courfe of experiments fhew, in a general way, that the acid of borax is decompofed, and that one of its component parts is coal or carbone. But what may be the caufe why it is fo difficultly feparated, and does not, in its original combination, deflagrate with nitre, remains to be proved by future refearches.

IV. Obfervations on the Means by which the Mountains in the Cevennes are rendered fertile. By Cit. Chaptal - - p. 295.

The inhabitants of the Cevennes exert a furprifing degree of induftry and perfeverance, in building walls acrofs the ravins, and at the edges of the calcareous platforms; by means of which the vegetable earth is retained, and the whole furface of thefe naturally barren mountains rendered fo productive as to fupport two or three hundred thoufand people.

V. Account of certain Experiments and Inferences refpecting the Combuftion of the Diamond, and the Nature of its Compofition. By Cit. Guyton p. 298

Phenomena of the combuftion of the diamond in oxygen gas by folar heat. Method of filling large veffels with the gas, without the previous introduction of mercury or water. The diamond cannot maintain the temperature requifite to continue it own combuftion. It is totally combuftible, and affords pure carbonic acid, in much greater quantity than charcoal does. Theory: that diamond is pure carbone, and that plumbago, charcoal, and carbonic gas, confift of the fame bafis, oxygenated in different degrees. Interefting remarks and conclufions.

VI. On the apparently fpontaneous Combuftion of living Individuals of the Human Species. By Citizen Lair - - - p. 305

VII. Remarks on the Conflagration of the Odeon. By B. G. Sage, Profeffor and Director of the firft School of Mines p. 306

The moft remarkable circumftances attending this fire, were the probable explofion of hydrogen in the great hall, and the developement of much fulphur in various parts of the building, by the difoxygenation of the plafter of fulphate of lime.

VIII. On the chemical Action of different Metals on each other, at the common Temperature of the Atmosphere. By Cit. Fabroni - p. 308

Mr. Fabroni is disposed, from some experiments, to conclude, that the galvanic stimuli from the application of metals to the tongue, &c. are merely the consequence of chemical actions.

IX. On the Harp of Eolus. By Matthew Young, B.D. - p. 310

Experiments on a single string of the Eolian harp. Enumeration of its tones. Theory.

X. Analysis of the Chromate of Iron, of the Bastide de la Carrade. By Cit. Tassaert - - - - - p. 314

XI. Report made to the National Institute of Sciences and Arts (at Paris), on the 29th Prairial, in the seventh Year (June 17, 1799), in the Name of the Class of Physical and Mathematical Sciences, on the Measure of the Meridian of France, and the Results which have been deduced to determine the new Metrical System - - - - - - p. 316

XII. Detached Facts, or Notices on chemical Subjects—Nitric Acid—Oxide of Azote—Indigo—Nitrate of Potash—Residue of Ether—Inflammation of Oils by Nitric Acid—Ammoniac—Putrid Water at Sea. By Cit. Proust, Professor of Chemistry at Madrid - - - p. 325

Scientific News. Account of Books, &c. - - p. 329

NOVEMBER 1799.

Engravings of the following Objects: 1. A new silent Escapement for Clocks. By Mr. Goodrich 2. The Furnace of Professor Pott. 3. Apparatus for converting Iron into Steel, by means of the Diamond; and, 4. A Lamp for burning Tallow, by Mr. William Close.

I. Chemical Experiments and Observations on the Production of Sugar, and an useful Syrup from indigenous Plants. By Sigismund Frederic Hermbstadt p. 333

General observations. Facts and observations concerning the quantities of sugar obtained from the sugar-cane, the maple, Indian corn, Siberian bear's breech, or katsch, the juice of grapes, and the birch.

II. Observations on the various Kinds of Manure. By John Middleton, Esq. p. 340

Experiments on a considerable scale with peat-ashes, coal-ashes, wood-ashes, malt-dust, soot, soap-makers' waste, the sweepings of London streets, the soil of privies, and farm-yard dung, as manures.

III.

III. Defcription of a new Efcapement for Clocks. By Mr. Simon Goodrich p. 342

Efcapement confifting of a crank upon the arbor of the laft wheel of the train. Its advantages.

IV. Defcription of the Furnace of Profeffor Pott - - p. 344

V. Procefs ufed to feparate the Mucilage from Linfeed Oil; Methods of diffolving Copal for Varnifh, &c. By Mr. Timothy Sheldrake - p. 345

VI. Analyfis of the Spinel. By Profeffor Klaproth - p. 349

VII. Verbal Procefs of the Converfion of Soft Iron into Caft Steel, by means of the Diamond. By Cit. Guyton - - - p. 350

Examination of the elaftic products in the common procefs of cementation for making fteel. Procefs with the diamond. A cavity was bored in a block of foft iron, and a ftopper fitted of the fame material. Into this cavity was put a diamond, with fome filings of the iron to fill the remaining fpace, after which the ftopper was driven in. In this ftate (the diamond weighing almoft one fixteenth part of the mafs) the whole was put into a crucible, and this placed in another; and the fpace between the two crucibles filled with pure filiceous fand. The external crucible was well clofed and luted, and the whole expofed to the ftrong ignition of a blaft furnace, by which the metal was fufed completely; the diamond difappeared, and the compound was found to be fteel.

VIII. Notice of the Experiments made by order of the Minifter of the Interior of France, relative to the Finenefs of Tin or Pewter - p. 356

The method recommended is to examine the fpecific gravity. It is lefs in compounds of lead and tin that would be deduced by computation. Experimental refults.

IX. A Chemical Examination of the Bath Waters. By G. S. Gibbes, B.M. F.R.S. - - - - - p. 359

X. Conftruction of a Lamp for burning Tallow. By Mr. William Clofe p. 363

XI. Report made to the Inftitute of Sciences and Arts (at Paris), on the 29 Prairial, in the Seventh Year (June 17, 1799), in the Name of the Clafs of Phyfical and Mathematical Sciences, on the Meafure of the Meridian of France, and the Refults which have been deduced to determine the new Metrical Syftem. (Concluded from page 324) - p. 365

Method by which the unity of weight was deduced from the metre. A hollow cylinder of brafs was conftructed, and its dimenfions afcertained by meafuring 37 different lines of length, and 48 different diameters. It was weighed in air and in water at known temperatures, and from the difference of the weight duly corrected, the cubic decimetre of water, or true kilogramme, was found. The ratio between this weight and the marc of the pile of Charlemagne was alfo fettled. Standards of the metre, and the kilogramme in platina, in iron, and in brafs. Length of the fimple pendulum for feconds at Paris.

XII. On the Web of the Garden-Spider. By a Correfpondent - p. 369

Adhefive quality of the polygonal threads of the fpider's web. Obfervations.

XIII. Plan of Experiments which were made in the Garden of Plants p. 370

On

On the propagation of the ſheep with the goat. Treatment of ſheep. On various animals uſed for food, &c.

XIV. On the native Iron of Peru, and on Mercury contained in Sea-Salt. By Profeſſor Prouſt of Madrid - - - p. 374

XV. Note concerning the Earthquake in Peru. By M. Cavanilles p. 377

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DECEMBER 1799.

Engravings of the following Objects: 1. The Gazometer of Seguin; and, 2. Figures illuſtrative of the theory and fabrication of Spirit Levels.

I. An Inquiry concerning the Weight aſcribed to Heat. By Benjamin Count of Rumford, F.R.S. &c. - - - p. 381

Upwards of four thouſand grains of water was counterpoiſed againſt a like quantity of weak ardent ſpirit, in bottles of the ſame magnitude, hermetically ſealed; the temperature being 61°. The apparatus being then removed into a room at the temperature 29° the water froze and preponderated by upwards of one-tenth of a grain. The equilibrium was reſtored when the ice was fuſed again in the warm room. Suſpicions of fallacy, however, led the inventor to other reſearches. By expoſing the balance, loaded with two balls of gilt braſs, to different temperatures, it was proved that no unequal expanſion of the arms had taken place. On repeating the original experiment with mercury, inſtead of ardent ſpirit, no change of equilibrium enſued from the congelation of the water; and when again with the moſt minute attention to the equality of temperature in three maſſes, of water, of mercury, and of diluted ardent ſpirit which were reſpectively put in equilibrio at 60° and at 30° no alteration was found at either temperature. Obſervations and remarks.

II. On the Plants uſed by the Ancient People of Europe to poiſon their Arrows. By C. Ch. Coquebert - - - p. 390

III. Experiments to determine the Quantity of Tanning Principle and Gallic Acid, contained in the Bark of various Trees. By George Biggin, Eſq. p. 392

The tanning principle was ſeparated by quick infuſion, and the gallic acid by ſubſequent infuſion for a longer time. The quantities of the former were aſcertained by its precipitation with glue, and the latter was judged of from the intenſity of colour it afforded with ſulphate of iron. Reſults of trials on twenty-one ſamples of barks.

IV. On the Plumb Line and Spirit Level. By M. Chezy - p. 396

Deſcription of the plumb line. Its degree of accuracy. Spirit level deſcribed, its qualities inveſtigated. Method of grinding its inner ſurface. Annotations theoretic and practical.

V. A Chemical Examination of the Bath Waters. By George Smith Gibbes, M. D. F. R. S. (continued) - - - p. 403

Detection of a conſiderable portion of ſilex in the Bath waters.

VI. Account of the new Gazometer of C. Seguin - p. 405

This principal advantage of this inftrument confifts in the gafes being fubjected to a very regular and meafurable preffure.

VII. Inftructions for making red Crayons. By A. F. Lomet p. 406

VIII. Chemical Experiments and Obfervations on the production of Sugar, and an ufeful Syrup from indigenous Plants. By Sigifmund Frederic Hermftadt, (concluded from page 339) - - - p. 410

Facts and obfervations concerning the quantities of fugar obtained from the white beet, the runkelrübe, common beet, carrots, turnips, colewort, fkirrets, and parfnips.

IX. On the filent Efcapement of Mr. Goodrich. By a Correfpondent p. 419

X. Refults of various Experiments for determining the Quantity of Action which Men can afford by their Daily Work, according to the different Manners in which they employ their Strength. By Citizen Coulomb - p. 419

XI. New Refearches into the Affinities which the Earths exert upon each other in the humid and in the dry Way. By Citizen Guyton - p. 419

XII. Account of the Improvements made on the Farm in the Great Park of his Majefty, the King, at Windfor. By Nathaniel Kent, Efq. p. 422

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JANUARY

JANUARY 1800.

Engravings of the following Objects: 1. The Urceola, or Caout-chouc Plant. 2. The Photometer of Leslie. 3. The Hydrostatic Lamp of Keir.

I. Account of the Improvements made on the Farm in the Great Park of his Majesty, the King, at Windsor. By Nathaniel Kent, Esq. - p. 429

II. Extract of a Letter from —— Petrie, Esq. on Board the Good Hope Indiaman, at Sea, Lat. 35° 40′ S. Long. 44° E. giving an Account of a singular Accident by Lightning. Communicated by Dr. Robert Petrie p. 432

One of the seamen was struck by lightning, and rendered insensible for some minutes. The singularity of this case, was, that a portion of the penicranium on the right side of the head of the size of a dollar, mortified and sloughed off, without any previous inflammation. Remarks and observations.

III. A Botanical Description of the Urceola Elastica, or Caout-Chouc Vine of Sumatra and Pulopinang; with an Account of the Properties of its inspissated Juice compared with those of the American Caout-Chouc. By William Roxburgh, M. D. - - - - - p. 345

IV. On the different Sorts of Lime used in Agriculture. By Smithson Tennant, F.R.S. - - - - - p. 404

The author being informed that two different kinds of lime are used in agriculture, in the neighbourhood of Doncaster; one of which gives fertility, and the other is deleterious to vegetation, was induced to examine them, and found that the first was pure lime, and the latter contained two thirds magnesia. Hence he was led to investigate the general scientific and agricultural relations of these two earths, in their various, natural, and artificial compounds.

V. Account of the strange Effects produced by Respiration of the gaseous Oxide of Azote - - - - - p. 446

From a considerable number of instances here stated, it appears that this gas acts strongly on the system, producing an increase of nervous energy, and affecting the mind in a very remarkable manner, in most cases producing great exhiliration.

VI. A chemical Examination of the Bath Waters. By George Smith Gibbes. (Concluded from page 404) - - - p. 452

VII. Analysis of an Iron Ore, the Composition of which has been hitherto misunderstood. By Mr. William Henry, including a Letter on Ores of Iron, addressed to Mr. Thomas Henry, F.R.S. By Charles Hatchett, Esq. F.R.S. p. 454

VIII. An Account of some Experiments on the Fecundation of Vegetables. By Thomas Andrew Knight, Esq. - - - p. 458

IX. Defcription of an Hygrometer and Photometer. By Mr. John Leflie. Communicated by the Inventor - - p. 461

If of two fimilar thermometers, the ball of one be blacked, while the glafs of the other remains clear and tranfparent: the temperature of the former will be more elevated by light, than that of the latter; or if the ball of one thermometer be wetted, while the other remains dry, the evaporation will diminifh the temperature of the firft. The thermometers may thus be made to indicate the intenfity of light, or the abforption of humidity. The inftrument of the inventor confifts of two air thermometers, connected together by a fyphon tube, in which a fluid moves, accordingly as the fpring of the included air in one or the other ball is affected, by the before mentioned caufes.

X. Defcription of the Hydroftatic Lamp of Mr. Peter Keir - p. 467

Obfervations on the methods of fupplying lamps with oil. Fountain lamp; its excellencies and defects. The lamp of Robert Hooke, which preferved the oil accurately at the fame level. Hydroftatic lamp.

XI. Reflections on the Decompofition of the Muriate of Soda by the Oxide of Lead. By Cit. Vauquelin - - - - p. 470

Philofophical News. Accounts of Books, &c. - - p. 473

Philofophical Tranfactions. Lord Dundonald's Profpectus. Sugar from the White Beet. Count Rumford's Effays. Pearfon's Chemical Nomenclature. Mudge's Defcription of a Time-Keeper. Adams' Electricity.

FEBRUARY 1800.

Engravings of the following Objects: 1. Apparatus for determining the Action of Fluids; and, 2. A Veffel for boiling inflammable Fluids.

I. Experiments on Indigo. By a Correfpondent - p. 477

Treatment of Indigo with inflammable bodies to deprive it of oxygen. New volatile product of a metallic afpect, not magnetical, nor acted upon by muriatic acid nor alcohol, nor boiling cauftic alkali, nor boiling concentrated fulphuric acid; but rapidly foluble in nitrous acid, and precipitable of a white colour by alkalis, &c.—A fluid, having the qualities of laurel-water, produced by abftracting nitrous acid from Indigo. Applications of oxigen to Indigo, &c.

II. New Refearches into the Affinities which the Earths exert upon each other in the humid and dry Way. By Citizen Guyton. (Concluded from p. 422)

III. Concerning the Influence of the Moon on the Atmofphere of the Earth. By C. Lamark - - - - - p. 488

From a feries of obfervations, the author prefents the following principles as eftablifhed; viz. that while the moon has north declination, the winds in this climate are fouth, fouth-weft, and weft, with cloudy, damp, rainy, or ftormy weather; but that the oppofite winds, with clear fettled weather, predominate during the fouth declination of that planet. Thefe effects are modified by various circumftances, fuch as the pofitions of the moon in its orbit, its conjunctions and oppofitions, &c.

IV. Directions for making the beſt Compoſition for the Metals of Reflecting Teleſcopes, and the Method of caſting, grinding, poliſhing, and giving the great Speculum the true parabolic Figure. By the Rev. John Edwards, B.A. p. 490

V. Early Developement of the Antiphlogiſtian Theory of Combuſtion. By Robert Hooke - - - - - p. 497

Mayow's Theory was publiſhed in 1674, but Hooke's Micrographia, containing a detail of the ſame theory, was licenſed in 1664, and publiſhed in the year following, 1665.

VI. Experiments and Obſervations on Shell and Bone. By Charles Hatchett, Eſq. F.R.S. - - - - p. 500

VII. Experiments upon the Reſiſtance of Bodies moving in Fluids. By the Rev. Sam. Vince, A.M. F.R.S. Plumian Profeſſor of Aſtronomy and Natural Philoſophy in the Univerſity of Cambridge - - p. 506

VIII. An Account of a Kettle for boiling inflammable Fluids. In a Letter from Tho. P. Smith to Rob. Patterſon - - p. 514

His contrivance conſiſts of a lip, ſpout, or rim, into which the fluid ſpreads itſelf when it riſes by ebullition. By this means the expanded part becomes cooled, and checks the boiling of the reſt.

IX. Letter from Mr. Davy, Superintendant of the Pneumatic Inſtitution, to Mr. Nicholſon, on the Nitrous Oxide, or Gaſeous Oxide of Azote, on certain Facts relating to Heat and Light, and on the Diſcovery of the Decompoſition of the Carbonate and Sulphate of Ammoniac - p. 515

X. Additional Remarks on the Hygrometer and Photometer. By Mr. John Leſlie - - - - - p. 518

XI. An Account of ſome Experiments on the Fecundation of Vegetables. By Thomas Andrew Knight, Eſq. - - - p. 519

Philoſophical News, &c. - - - - p. 522

On the Soldering of Glaſs.—Simple Method of obviating the Reſiſtance of the Air againſt the Pendulums of Clocks.—Diſtillation by the Application of the cooling Mixture.

MARCH 1800.

Engravings of the following Objects: The Air Furnace of Cit. Lecour; and, 2. An Improvement in Mr. Close's Tallow Lamp.

I. Directions for making the best Composition for the Metals of Reflecting Telescopes, and the Method of casting, grinding, polishing, and giving the great Speculum the true parabolic Figure. By the Rev. John Edwards B. A. (Concluded from p. 497) - - - p. 525

II. Experiments and Observations on Shell and Bone. By Charles Hatchett, Esq. F. R. S. (Concluded from p. 506) - - p. 529

III. Some Account of the Elastic Gum Vine of Prince of Wales's Island, and of Experiments made on the milky Juice which it produces: with Hints respecting the useful Purposes to which it may be applied. By James Howison, Esq. p. 534

IV. Miscellaneous Observations relative to the Western Parts of Pennsylvania, particularly those in the Neighbourhood of Lake Erie. By Andrew Ellicott p. 539

V. Account of the Pearl Fishery in the Gulph of Manar, in March and April, 1797. By Henry J. Le Beck, Esq. - - p. 542

VI. Description of the Air Furnace of Cit. Lecour - p. 546

VII. On the Lamp for Tallow, and the Combustion of that Material. By Mr. William Close - - - p. 547

ERRATA.

Page 179, *note*, read *Journal de Physique, An.* VII. p. 114.
365, in the note, for *succeeded* read *superseded.*
476, bottom, for *Galearies* read *Galvanism.*
497, line 22, for 1675, read 1665.

A
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THE ARTS.

APRIL 1799.

ARTICLE I.

A ſtatical Inquiry into the Source of Nutrition in Succulent Vegetables. By MR. *J.* GOUGH.*

Kendall, Feb. 7, 1799.

SUCCULENT plants are commonly ſuppoſed to derive a greater proportion of nutriment from the atmoſphere, than vegetables of a leſs humid conſtitution; and this idea is ſupported, at leaſt in appearance, by a ſingularity in the economy of the plants under conſideration. If the ſempervivum tectorum, or any of our indigenous ſedums, be ſuſpended in the dry air of a chamber, they will live for a long time; whereas, other vegetables wither ſoon after their connection with the ground is interrupted. Many perſons, attending to the preceding facts, and reaſoning from analogy, have concluded, that ſempervivum and ſedum, with the reſt of the ſucculent tribe, derive this ſupply not from the earth but from the atmoſphere, becauſe they can ſubſiſt independent of the former: but natural hiſtory points out a ſecond analogy, which, although it is found in the animal kingdom, ſhould not be overlooked, on account of its ſtrict coincidence with the foregoing deviation from the prevailing habits of the inferior claſs of organized bodies. The chameleon bears hunger for months together, when in confinement: this peculiarity, added to the difficulty of obſerving it take its prey, perſuaded the ancients that the reptile in queſtion is nouriſhed by the air alone, but the induſtry of later naturaliſts †, has ſhewn this notion to be falſe, by proving it to feed on flies; and that the ſame powers of abſtinence are extended to ſerpents, ſpiders, and many more

* Communicated by the author.

† Vide Raii Synop. animalium Quadrup.

animals, whose opportunities to procure food are precarious. Now as succulent plants are intended to grow on rocks, and in other dry situations, where their supplies of humidity suffer frequent and long interruptions, in all probability they bear the same relation to the vegetable tribes, which reptiles bear to other animated beings.

The question whether of the two opinions now stated is true, will be best resolved by a set of statical experiments, made with succulent plants surrounded by dry air. Should they appear to gain weight when treated in this manner, the former notion must be adopted; but should they happen to lose part of their substance, the latter will have the preference: for the act of vegetating implies the growth of parts already formed, and the production of new organs to complete the plant. The following experiments were made, in order to decide the question under consideration; which I shall now relate without further apology.

Experiment 1*. Three plants of sedum acre, weighing $69\frac{3}{4}$ grains, were suspended in a proper manner before a window, on the 6th of September, 1790: being again examined on the 25th of the same month, their weight was reduced to 42 grains. They were then placed in water for 24 hours, and afterwards exposed to the light and air for half that period, to give the water adhering to the plants an opportunity to evaporate. At the end of this time, their weight was $63\frac{1}{2}$ grains; which is $6\frac{1}{4}$ grains less than their original weight. Perhaps the preceding deficiency is owing in part to the plants not being weighed before part of their sap was lost together with the superfluous water; but the following circumstance should also be taken into the account. If a glass of limewater be placed under a glass jar containing a plant of sedum, the lime will be precipitated in the course of three or four days; from which we may conclude with reason, that part of the carbon of the plant is converted, in all similar cases, into gas by the oxygen of the atmosphere. The same experiment being repeated with sedum reflexum, as well as sedum acre, a considerable loss of weight took place in both trials; and I think the tendency and uniformity of the result will justify the succeeding remarks and conclusions.

The sedum acre, in all probability, draws no more nutriment from the air than other plants; at the same time, the herb in question, as well as the rest of the succulent tribe, can evidently retain life for an unusual length of time, in situations incapable of affording them nutrition in the common way. The vegetative principle even continues to act in the sedum, when suspended by the roots, apparently with a view to its preservation; for the branches of the plants used for the experiment curled, by turning their tips, contrary to the general habit of vegetables, from the window: by this provident singularity, much light was avoided, and with it an unnecessary expenditure of sap. The roots in the mean time produced a number of fine filaments, as if in search of humidity; for they seemed, in this instance, to imitate trees growing on old buildings, which have been known to connect themselves with the ground, by means of roots descending along the sides of the walls. This peculiarity in the constitutions of the sedums is absolutely necessary to their existence; for these plants being intended to grow upon rocks and walls but slightly covered with earths, would be burned up and destroyed by the droughts of summer, but their many succulent leaves obviate the disaster, by performing the

* The substance of this experiment is inserted in Wither. Bot. Arrangement, edit. 3d. Vide art. Sedum.

office

office of so many receptacles of sap sufficient to answer the demands of these trying seasons; and which are recruited in a greater or less degree by every shower. These magazines of food are emptied in the flowering season, probably to feed the expanding capsules; for by the time the seeds are ripe, the plants are exhausted, and suffer what may be called a vegetable death, by a natural process.

Experiment 2. The sempervivum tectorum, which resembles the sedums in habit, also agrees with them in the property in question: for an offset of sempervivum, of the weight of 250 grains, being left in a window from the 29th of April to the third of June, lost 116 grains, and appeared to be in a very exhausted condition, but its faculty to vegetate remained unimpaired; for the same offset was afterwards placed for an hour every other day in a glass of rain-water to the end of the month, by which it was so much recruited, as to produce a new shoot, and the joint weight of parent and offspring amounted to 170 grains. Notwithstanding the conclusive result of the two last experiments, it may, perhaps, be alleged, that the dry air of a chamber does not give succulent plants that opportunity to procure water which the atmosphere affords them during fair weather, especially in the nights, when the dews are formed: but this objection seems to be overturned by the succeeding paragraph.

Experiment 3. I suspended different offsets of sempervivum tectorum in glass-bells, having their mouths placed in water; which did not rise in the vessels far enough to touch the plant. The air which surrounded the offsets was kept so moist by this sort of confinement that the insides of the jars appeared constantly covered with vapour: at the same time care was taken not to expose the apparatus to the direct rays of the sun. A plant treated in this manner in June, lost a sixth part of its weight in a fortnight. Another plant, subjected to a similar trial during the two last weeks of September, lost but $\frac{1}{18}$ part of its weight. The loss of substance which is here remarked, will be best explained by the following fact.—Dew does not form with equal facility on all kinds of bodies; especially in a temperature of 50°, or higher: for instance, glass collects dew from air, which is not in a state to impart the same to varnished wood or polished metals; and were we to form a scale of these affinities by experiment, green vegetables would take their respective stations, somewhere between glass and some one of the metals; because, when the three substances in question are exposed to the open air, glass is covered with dew in the first place; and the vapour fixes on clean metallic surfaces, after the leaves of living shrubs. On this account, the water contained by the air used in the last experiment, attached itself to the sides of the jars, in preference to the offsets of sempervivum; which being deprived of dew, could not repair the loss they suffered by evaporation for want of aqueous particles in a condensed form. The uniform result of the preceding experiments seems to prove, beyond a possibility of doubt, that the succulent vegetables of Europe receive their portion of nutrition by the common vehicle of water conveyed to them in the common way by the earth, or deposited on their leaves in the form of rain; and I have reason to suppose that plants of the same description, natives of tropical countries, are regulated by the same principle of economy; for a plant of aloe perfoliata, which was suspended for 52 days, about the conclusion of autumn, in a room with a northern aspect, lost something more than one-fourth of its weight in this dry situation; but no attempts were made to repair the loss by the methods pursued in the first experiment.

II.

Description of a Pocket Ribband-machine, for charging a small coated Phial with Electricity. By *J. Till. Ulloustone.*

FIG. 1 and 2, Pl. I, are representations of this instrument of its real size; A B C D, fig. 1 and 2, are two flat mahogany boards, each three inches long, two inches broad, and three-eighths of an inch thick. In the holes E and F, fig. 2, which are two inches from each other, and equidistant from the sides and ends of the boards, are glued fast two cylindrical pins of box-wood, each one-fourth of an inch in diameter; these pins project about one inch and a quarter, perpendicularly, from the opposite surface of the board, and have their ends formed into male screws about one-half of an inch long. Two cylindrical wooden pins of one-eighth of an inch in diameter, but without screws, are fixed at each end of the same board, and in the same transverse lines with the other pins, but near the sides, as at G H I J. They stand parallel to the other pins on the same surface of the board, and are about a quarter of an inch shorter. These pins pass through six holes in the board, fig 1, when both boards are applied together. Two flat nuts of boxwood, K and L, fig. 1, of five-eighths of an inch in diameter each, are screwed upon the middle pins to press the boards together. Each board has one of its faces covered with a cushion, consisting of six or eight pieces of flannel slightly stitched together, with perforations for the pins to pass through. The cushions are covered with a mole-skin laid with the fur outwards, and the hair of each inclining to the edges of the boards, in the direction of the arrow in fig. 1; holes being also made through the skins to admit the pins. When the boards are screwed pretty close together by means of the nuts, with the cushions between them, the redundant edge or border of each skin is glued to the edge of its respective board, and they thus completely form two elastic cushions. A piece of brass wire, about one tenth of an inch in diameter, is bent into the form M N O, fig. 2, and fixed upon the inner side of this board, which is faced with the fur, and in the situation represented in the drawing, by means of two screws which pass through pieces of flat brass, soldered on the wire. To the parts M S O T of this wire, a tin tube is soldered, open at both ends, and fitted to receive the lower part of a very thin ounce or ounce and half phial or jar, coated on both sides with tin-foil, excepting $1\frac{3}{4}$ of an inch of its top. This phial, when placed firm in the tin tube, has its wire parallel to, and $1\frac{1}{2}$ inch from, the side of one of the boards, and likewise in the same plane with that produced by the contact of the two cushions. The wire proceeding from the inside coating of the phial is fixed firmly in its neck, by means of a narrow silk ribband dipped in melted electric cement, and rolled lightly round the same wire to a proper degree of thickness. The space U V X W, fig. 1, is without tin, and is rather more than half the circumference of the tube. Thus the charged phial may be removed from the machine, without the hazard of discharging, by pushing it forward a little with the thumb applied against its bottom, and then taking hold of it by its coating. R is a curved wire, with a ball to it, which by moving on E as a centre, and standing at any angle, it is raised to answer the end of an electrometer and discharger of the phial. The figure of an arrow is made on the

the board, fig. 1, with its point in the fame direction to which the fur of its cufhion inclines, in order to fhew the proper application which is alfo fecured by the pins. The ends of a filk ribband, two inches broad, and a yard or 32 inches long, are neatly fewed together with filk, and the ribband afterwards fteeped in weak feed-lac varnifh. Inclofe the ribband betwixt the cufhions, the arrow pointing towards the wire of the jar. The machine being expofed to the moderate warmth of a fire for ten or twelve minutes, efpecially in damp weather, and the cufhions made to prefs gently againft the ribband, fet the balls a quarter of an inch from each other, then hold the inftrument by the boards in the left hand, and with the right hand, at a pretty good diftance from the wire in the phial, draw the ribband gently, keeping the filk under the wire of the jar, and in contact with it. At fix revolutions of the ribband, the phial will generally difcharge itfelf at the above-mentioned diftance of the balls. Twelve revolutions of the ribband, when the excitation is powerful, and the balls feparated at a confiderable diftance, will produce a charge of the jar, which few perfons would choofe to receive a fecond time. Mole-fkin, with the hair on, excites filk, or the refinous plate of an electrophorus, better than the fur of any indigenous animal the writer of this has tried: this he difcovered by accident. The machine may be carried conveniently in the pocket in an oval cafe made of pafteboard.

This inftrument is not to be confidered folely as a philofophical plaything; it may be ufed when a common electrical machine is not at hand, as an auxiliary in recovering perfons apparently dead by drowning, and other kinds of fuffocation; and when the unfortunate objects are at fome diftance, it will be found not a little convenient, on account of its portable fize. It may likewife be very advantageoufly applied in fome cafes of menftrual obftruction, and in feveral morbid affections where fmall fhocks are indicated. In fufpended animation from fubmerfion, fhocks of a proper degree of ftrength may be given conveniently by this inftrument in the following manner, paffing each charge through the breaft, at the moment the lungs are expanded by an affiftant. Apply the knob of the charged phial, held by its coating in one hand to the region of the heart of the patient, at the fame time that a finger of the other hand is in contact with the fpine. To avoid the fhock which the operator receives in this cafe, the electrical circuit may be completed by means of a portable conductor formed of a gold, filver, or copper thread, neatly inclofed in a filk ribband, except its ends. When the phial is about to be difcharged, perhaps it may be right not to fuffer any part of the patient's body to come in contact with this conductor, but the fpine; and this may be effected by means of an affiftant, and one hand of the operator.

III.

On the Corundum-ftone from Afia. By the Right Honourable CHARLES GREVILLE, *F.R.S.*

(Concluded from page 544, vol. II.)

WE frequently fee fmall rhombs traced on the furface of the planes, on the ends of the hexaedral prifm, fig. 10, Pl. XXIV, vol. II. This, no doubt, is occafioned alfo by the interfection of the laminæ, on the planes of the primitive rhomboidal parallelopiped. But thefe rhombs,

rhombs, formed by the reunion of lines that join in angles of 60° and 120°, inſtead of 86° and 94° (like thoſe we have ſeen traced on the faces which correſpond with thoſe of the rhomboidal parallelopiped), form angles of 60° and 120°. It would, therefore, be an error to conſider them as indications of the form of the elements of cryſtallization, as we are tempted to do from a ſimple inſpection of the cryſtal. Theſe ſame lines form equilateral triangles with one another, as may be ſeen in fig. 10.

The cauſe of theſe ſmall equilateral triangles, which ſometimes project a little over the planes on the end of the priſm, muſt now be obvious. If, during the ſuperpoſition of the cryſtalline laminæ on all the planes of the rhomboidal parallelopiped, it has happened from any cauſe whatever, that the laminæ depoſited on the three faces of the ſame ſummit, have not fallen exactly on thoſe which preceded them, or that they have experienced ſome deviation, or have not had the ſame decreaſe, as all the others, at the angle of 86°, theſe triangles muſt neceſſarily occur; in the ſame manner it muſt be obvious, why theſe ſmall equilateral triangular projections are frequently placed on one of the ſides of the cryſtal.

The primitive form of the corundum cryſtal is therefore a rhomboidal parallelopiped, whoſe ſolid angle at the ſummit is 84° 31′, and that formed by the reunion of the baſes is 95° 29′.

The cryſtalline laminæ are rhombs of 86° and 94°: theſe, in my opinion, are double cryſtalline molecules; the ſingle molecules I apprehend to be iſoſceles triangles, of 86° at the angle of the ſummit, and of 47° at thoſe of the baſe*.

Although the rhomboidal parallelopiped of 86° and 94° is the primitive form of the corundum cryſtal, yet it is rare to meet with that ſubſtance under this perfect and determined form; and in moſt mineral ſubſtances, it is more rare to meet with their primitive cryſtals, than their different modifications. Amongſt Mr. Greville's numerous ſpecimens of corundum, I have met with only one which has this primitive form, and it is doubtful whether even this may not be a fragment.

The corundum cryſtal preſents another modification, under which the regular hexaedral priſm, inſtead of having three alternate ſolid angles, at each of its ends (on which ſolid angles are placed iſoſceles triangular planes, forming a ſolid angle of 122° 34′ with the planes at the extremities upon which they are inclined), has alſo its angles ſupplied by iſoſceles triangular planes; but theſe planes, inſtead of 122° 34′, form ſolid angles of 160° 42,′ with the ſaid planes on the extremities. (See fig. 11 and 12.) Theſe new planes, which conſtitute a new modification of the primitive form of corundum, are the reſult of a different order, in the decreaſe of the laminæ; which, in the primitive form, are depoſited on the planes of its primitive rhomboid by ſingle rows of cryſtalline molecules, and increaſe the planes which ter-

* I am at preſent preparing a work, in which I ſhall, if circumſtances permit me to finiſh it, give the reſult of my obſervations, and my own opinion on this intereſting part of mineralogy. I ſhall only obſerve here, that although double molecules, ſquare and rhomboidal, are frequently formed in the proceſs of cryſtallization, yet the real form of the cryſtalline molecules ſeems to be triangular. By obſerving the progreſs of the rhomboidal parallelopiped, in its paſſage to the form of an hexaedral priſm (fig 4 and 5), and by conſidering the priſm terminated, it ſeems evident, that the laſt laminæ which had been depoſited, after the progreſſive decreaſe in the rows of cryſtalline molecules to one ſingle molecule, muſt neceſſarily have been triangular. B.

minate the hexaedron: whereas, in this ſecond modification, the decreaſe of molecules is by two rows, which gives a more obtuſe inclination, and forms new planes. The ſurface is uſually ſtriated, parallel to the ſides of the planes which terminate this cryſtal; an appearance always announcing imperfection in the cryſtallization, ariſing either from a change in the order of decreaſe or increaſe, or from a leſs perfect union of the cryſtalline laminæ. A ſection would ſhew gradual riſings or ſteps, as appears in fig. 14, which is a ſection of fig. 13, in the line A D B. Theſe ſtriæ are not to be confounded with thoſe in numberleſs ſubſtances, as in tourmalines, ſchorls, &c. which ariſe from the longitudinal union of numberleſs diſtinct cryſtals. The cryſtal reſulting from this new mode of decreaſe in the cryſtalline laminæ, will repreſent one or other of the varieties ſhewn in fig. 11, 12, and 13, according to the period when ſuch decreaſe has begun in the proceſs of the cryſtallization; and, if it has begun very late, the new faces will only be ſmall, nay, almoſt imperceptible, iſoſceles triangles, forming ſolid angles of 160° 42′ with the planes of the extremities of the priſm, as in fig. 5; the meaſure of the angles however muſt be excepted.

If this irregular mode of decreaſe had begun with the firſt cryſtalline laminæ, which were depoſited on the primitive rhomboidal parallelopiped, the hexaedral priſm reſulting therefrom would have been terminated by two very obtuſe triedral pyramids, whoſe planes would have been rhombs; and they would have been placed in a contrary direction to each other, as may be ſeen in fig. 12, by the dotted lines. I have not met with this variety, but its exiſtence may be ſuppoſed.

It happens ſometimes, that the cryſtallization has not been ſo perfect as to deſtroy every appearance of the faces of the primitive rhomboidal parallelopiped; in this caſe there remains, on the ſolid angle of 112°, formed by the junction of the new faces with the edges of the priſm, a ſmall iſoſceles triangle, as in fig. 13, which correſponds to thoſe in fig. 5, of the preceding modification.

The cryſtals which explained the ſecond modification, form alſo a part of Mr. Greville's collection: one in particular, is highly worthy of notice; it is the moſt perfect cryſtal I have ever ſeen of this ſubſtance. The ſurface of the faces of the priſm, although rough, is infinitely leſs ſo than that of the others, and much more brilliant. The planes on the ends have the uſual poliſh of cryſtals; its colour is a pale red, and its tranſparency may be compared to that of wax.

This ſubſtance preſents a third modification, in which the hexaedral priſm diminiſhes in diameter, as is apparent by comparing the diameter of its two ends; in ſome, it appears like a regular hexaedral pyramid truncated (fig. 15). The cryſtals of this modification are uſually irregular, and ſeldom admit of a certain meaſure of their angles; but among the numerous ſpecimens, in Mr. Greville's collection, I have been able to aſcertain, in the greater part, that the hexagonal plane at the top, forms angles of about 120°, with the planes of the pyramid; and the hexagonal plane at the baſe, forms angles of about 78° with the planes of the pyramid. In other inſtances, the form of the pyramid varies greatly; in ſome the angle at the upper plane was 110°, and the angle at the baſe about 70°; in others, the angle at the upper plane was about 100°, and the one at the lower plane about 80°.

In

In theſe three varieties, the cryſtalline laminæ can be ſeparated, as in the hexagonal priſm, at the three ſolid alternate angles of each end, but in a contrary direction to each other. The planes which appear when the laminæ are detached regularly, form ſolid angles of 22° 34′ with the planes of the extremity: this arrangement is analogous to that of the hexaedral priſm. The difference of form ariſes from the cryſtalline laminæ depoſited on the planes of the primitive rhomboid, decreaſing by more than one row of molecules, on the planes of one of the triedral pyramids of the rhomboid, and by leſs than one row, on the planes of its other pyramid. This general obſervation, on the manner in which this primitive cryſtal of corundum paſſes to the different varieties juſt mentioned, is the only one I have eſtabliſhed with any degree of certainty at preſent. Specimens with perfect cryſtals, whoſe angles may be meaſured with accuracy, will probably arrive from India, and give further demonſtration, as to theſe and other varieties of modifications of corundum. We may conceive, that if in this modification the cryſtallization had ceaſed before the intire formation of the cryſtal, there would have remained ſmall iſoſceles triangular planes, on three of the alternate ſolid angles, formed by the junction of the planes on the ends, with the edges of the truncated pyramid. Theſe iſoſceles triangular planes reſemble thoſe we have ſeen in the firſt modification (fig. 4 and 5), and form, in the ſame manner, ſolid angles of 122° 34′ with the planes on the ends of the priſm (fig. 16.)

Finally, if during the formation of the cryſtal, in this modification, it ſhould happen that the laminæ depoſited on the three planes of the rhomboidal parallelopiped, on the ſide where they undergo a greater decreaſe, do not undergo the decreaſe of one row of molecules at the acute angle of the ſummit, the cryſtal will be a real hexaedral pyramid (fig. 17), whoſe acute angle at the ſummit, meaſured on the ſides, will be nearly 24°, in one of the varieties; 40° for the moſt obtuſe; and 20° for the moſt acute variety: the angle of their triangular planes, in the firſt inſtance, 13° 41′; in the ſecond, 22° 20′; and 11° 28′ in the third. I have not ſeen any perfect pyramids; but in many the hexagonal plane terminating the pyramid is ſo ſmall, that it renders its total ſuppreſſion probable.

This decreaſe neceſſarily produces a ſingle pyramid, as above mentioned; nevertheleſs, there are inſtances of cryſtals of corundum, belonging to the variety where the terminal planes make, with the planes of the pyramid, a ſolid angle of about 100°, in which two pyramids of the ſame dimenſions, having their ſummit replaced by a ſmall hexagonal plane, are placed baſe to baſe.

I have alſo obſerved among the cryſtals of the obtuſe variety above mentioned, in Mr. Greville's collection, an inſtance of the decreaſe taking place by ſeveral rows, on one three-ſided pyramid of the primitive rhomboid, and by ſingle rows on the other. Conſequently, the cryſtal is a ſhort regular hexaedral priſm, terminating on one end only by an hexaedral pyramid; the planes of which, as well as of the priſm, are alternately broad and narrow, and almoſt perfect, its apex being replaced by a very ſmall plane.

I ſhall conclude, by mentioning a variety of corundum, deſcribed by the Abbé Hauy, in the Journal des Mines, No. 28; in which the edges of the terminal planes of the hexaedral priſm are replaced by planes which form an angle of 116° 31′ with the terminal planes; but in the numerous collection of Mr. Greville I have not ſeen this variety. One cryſtal had an ap-

pearance

pearance of ſuch planes; but, on examination, it was clearly accidental. The authority of the Abbé Hauy in cryſtallography, is ſo great, that the exiſtence of ſuch modification ought not to be denied without further examination; though I cannot, in this inſtance, adopt it: he derives this variety, which he calls ſubpyramidal, from a decreaſe of three rows of molecules, at the angles of the baſe of the two pyramids of the primitive rhomboid; and he ſeems to attribute the ſame formation to the pyramidal variety with double pyramid, which he ſuppoſes may exiſt.

The primitive cryſtals, and the firſt and ſecond modifications of corundum, are from the peninſula of India. The third modification, or the pyramidal variety is from China; nothing approaching this form being among the ſpecimens which Mr. Greville received from the peninſula of India.

The preceding obſervations, and particularly the laſt-mentioned modification of corundum, compared with the beſt deſcription of the ſapphire, ſuggeſt the further examination of the degree of connection, if not of identity, of theſe oriental ſtones.

In both, the hexaedral pyramids are uſually incomplete in their apex, and they vary in acuteneſs. I have ſtated the degree in which the ſolid angles of the pyramid (taken as complete) vary, in corundum, to be from 20° to 40°.

Romé de L'Iſle ſtates, that the ſapphire varies from 20° to 30°. The Abbé Hauy (Journal de Phyſique, Aug. 1793) mentions two varieties of the ſapphire, one meaſuring at the ſolid angle of the pyramid 40° 6′, the other 57° 24′. I never ſaw a ſapphire with ſo obtuſe an angle as the laſt, but many whoſe angle at the top, if the pyramid had been complete, would have been the ſame as that of the corundum. Beſides the analogy between the cryſtals of corundum and the ſapphire, by the union of two hexaedral pyramids at their baſe, it alſo exiſts by the meaſure of their angles; and both ſubſtances are ſubject to the ſame irregularity, ſometimes appearing as a ſingle hexaedral pyramid, and ſometimes as an hexaedral priſm: moreover, the ſapphire ſometimes has on its ſolid angles, alternately, the ſame triangular planes, (fig. 5.), and alſo the prominent triangles on the planes of the extremities (fig. 10.), which often appear in the cryſtals of corundum. The Abbé Hauy, in the Journal de Phyſique, Auguſt 1793, names this variety, *orientale enneagone* which is repreſented in the annexed plate (fig. 18.), and ſays, that the ſmall triangular planes make with the terminal planes an angle of 122°. 18′; and, in the deſcription of the ſame triangular planes in the corundum (fig. 16.), it appears that theſe planes are the remains of the planes of the primitive rhomboid, and form, with the terminal planes, an angle of 122° 34′.

Perhaps the rhomboidal cryſtal, which Romé de L'Iſle had given as one of the forms of the ſapphire, ſhould be reſtored to it. He had examined it at M. Jacquemin's, jeweller to the crown (Criſtallographie, 1 edit. p. 221), and he ſuppreſſed it in his ſecond edition, but often expreſſed to me his regret in having made the alteration. I have before me a letter from that celebrated naturaliſt, dated September, 1784,* in which he incloſed, for my opinion, a copy of a letter he had received from Mr. Werner, with models of ſome cryſtals; among them two called by him rubies; one a rhomboid, of which the angles of the ſummit are ſubſtituted by planes (fig. 19.), the other is preciſely the ſame as fig. 3, 4, and 5, of the annexed plate.

* A letter to the ſame effect was written to Mr. la Metherie, and publiſhed in the Journal de Phyſique, May, 1787.

The

The following is a tranſlation of Romé de L'Iſle's words: "The firſt of theſe rubies has "exactly the ſame form as I have repreſented in plate IV. fig. 60, of my Criſtallographie, "viz. a rhomboidal parallelopiped, truncated at each of its obtuſe angles, by an equilateral "triangular plane."

"You will have a correct idea of the other cryſtal, if you ſuppoſe the cryſtal, repreſented in "Plate IV. fig. 87, truncated at each of the ſummits of its pyramids, by an equilateral triangu-"lar Plane, as in the preceding modification, but deeper, and in ſo great a degree, that the "three rhombic planes of each pyramid diſappear with the exception of three iſoſceles trian-"gles: this modification differs from the firſt only by the hexaedral priſm, and the deeper "truncature at the ſummits of the pyramids."

It is therefore clear, that if the primitive rhomboid of corundum decreaſed only at the ſuperior angles of its laminæ, it would exhibit exactly the firſt of theſe varieties of Mr. Werner's ruby, as in the annexed fig. 19.

As to the ſecond variety of Mr. Werner's ruby, it is equally clear, if in fig. 87, referred to by Romé de L'Iſle (repreſented by the annexed fig. 20.), no more of the pyramid was left than the three ſmall triangles *b*, *a*, *c*, there would be preciſely one of the forms of corundum before deſcribed, to which the annexed figure 5 belongs.

It may, perhaps, be objected, that the laminæ appear to be parallel to the terminal planes, in the ſapphire, and inclined in the corundum. There are cryſtals of corundum, in which, very frequently, the laminæ appear parallel to the terminal plane; I was at firſt, and for ſome time, deceived by that appearance. In other corundum cryſtals, the laminæ appear to be parallel to the priſmatic planes; and to conclude the inſtances of analogy, the ſuperpoſition of rhomboidal laminæ is ſometimes obſervable in oriental rubies and ſapphires. It was by this appearance, Mr. Greville was led to try the effect of cutting the forementioned ſtones *en cabochon*; whereby a ſimilar effect of triple reflection, which formed ſtars of ſix rays from a common centre, was produced in the oriental ruby, in the ſapphire, and in the corundum.

It is to be lamented that Mr. Werner did not ſend, with his models, the ſpecific gravity of each of the rubies, and the meaſures of their angles: we ſhould then have had data to decide whether the rubies, ſent by Mr. Werner, were, as I ſuppoſe them to be, oriental rubies, or ſapphires; and, with equal certainty, whether the parallelopiped rhomboid correſponds preciſely with that of the corundum: by this the perfect identity or analogy, between the corundum and the ſapphire, would have been no longer doubtful.

Table

Table of the specific Gravity of the Corundum, Sapphire, Topaz, Ruby, and Diamond, on different authorities.

Corundum.

Hatchett and Greville	2,768 *	Matrix of corundum. Coast
H. and G.	2,785 *	Lump of corundum. Coast
Klaproth	3,075	
Klaproth	3,710	
Blumenbach	3,808	
Brisson	3,873	
Hatchett and Greville	3,876 *	Corone. Bengal
Lichtenberg	3,908	
Grofs	3,935	
Hatchett and Greville	3,950 *	Cryftal of corundum. Coaft
H. and G.	3,954 *	Ditto, with vitreous crofs fracture Coaft
H. and G.	3,959 *	Ruby-coloured. Coaft
H. and G.	3,959 *	Chatoyant. China
H. and G.	3,962 *	Cryftal. China
Klaproth	4,180	

Sapphire.

Briffon	3,130	Brafilian; probably a topaz
Bergman	3,650 †	
Quift	3,800 †	
Bergman	3,940 †	
Klaproth	3,950 †	
Bergman	3,974 †	
Briffon	3,991 †	White oriental
Briffon	3,994 †	
Blumenbach	3,994 †	Blue
Hatchett and Greville	4,000 †*	Greyifh ftar-ftone
Werner	4,000 †	
Blumenbach	4,010 †	
Hatchett and Greville	4,035 †	Blue ftar-ftone
Briffon	4,076 †	From Puy-en-Velay
Blumenbach	4,083 †	Crimfon
Hatchett and Greville	4,083 †*	Pale blue cryftal
Mufchenbroek	4,090 †	
Blumenbach	4,100 †	Yellow
La Metherie	4,200 †	
Quift	4,200 †	

Topaz.

La Metherie - - -	2,690	Siberia
Bergman - - - -	3,460	
Werner - - -	3,464	Light blue. Brazil
Quiſt - - -	3,500	
Werner - -	3,521	Eibenſtocher
Briſſon - - - - -	3,531	Red. Brazil
Briſſon - - - -	3,536	Dark yellow. Brazil
Werner - - -	3,540	Dark yellow. Brazil
Briſſon - - -	3,548	Oriental
Werner - - -	3,556	Schneckenſteiner
Briſſon - - - -	3,564	Schneckenſteiner
Briſſon - - -	4,010 †	Oriental
Bergman - - -	4,560 †	

Ruby.

Bergman - - -	3,180	
Muſchenbroeck - - -	3,180	
Quiſt - - -	3,400	Spinel
Blumenbach - - -	3,454	Ceylon
Quiſt - - -	3,500	Brazil
Briſſon - - - -	3,531	Brazil
Klaproth - - - -	3,570	
Hatchett and Greville - -	3,571 *	Octoedral cryſtal
H. and G. - - -	3,625 *	Macle of Octoedral cryſtal
Blumenbach - - -	3,645	
Blumenbach - - -	3,760	
Briſſon - - - - -	3,760	
Hatchett and Greville - -	4,166 *	Salam Ruby. Star-ſtone. Coaſt
Quiſt - - -	4,200 †	
Bergman - - - -	4,240 †	

Diamond.

Hatchett and Greville - -	3,356	Perfect cryſtal
Wallerius - - -	3,400	
Hatchett and Greville - -	3,471	Aggregate cryſtal
Cronſtedt - - - -	3,500	
Muſchenbroeck - -	3,518	
La Metherie - - -	3,520	
Briſſon - - -	3,521	
Werner - - - - -	3,600	

The Mark * diſtinguiſhes the ſpecimens in my collection, to which I have referred in the foregoing paper.

The

The mark † distinguishes the stones, which, from their specific gravity, I think belong to the genus of corundum.

The generic name corundum, I am in the habit of giving to those sorts which have a sparry, or a granulated fracture. When corundum has a vitreous cross fracture, I call it sapphire; and distinguish its varieties by their colours, white, red, blue, yellow, green; and by the accidental reflection of light from their laminæ: when in one direction, I call the sapphire chatoyant; when the reflection is compounded of rays which intersect each other, and appear to diverge from a common centre, I call them star-stones, as red, blue, or greyish star-stones, or star sapphires.

IV.

Experimental Researches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various Hydraulic Phenomena. By Citizen J. B. VENTURI, Professor of Experimental Philosophy at Modena, Member of the Italian Society, of the Institute of Bologna, the Agrarian Society of Turin, &c.

(Concluded from page 494, vol. II.)

PROPOSITION XI.

If the water of a reservoir, which flows through an horizontal aperture, be influenced by any foreign motion, it will form an hollow whirl above the orifice itself.

CITIZEN Bossut, has given a very good description of this kind of eddy*. It is of a different nature from those considered in the foregoing proposition; but the causes of both are, in some respects, similar, for which reason I propose to attend to them more particularly in this place.

Let D Q fig. 18 (plate XXII, vol. II), represent an horizontal plane near the orifice E F, through which the fluid of the reservoir M N flows. A fluid particle D, situated in this plane, has a motion D B, inclined to the axis A B. This motion may be decomposed into two, D C, C B; let us suppose that plane D Q to descend parallel to itself along the axis, with the motion C B; the motion D C of the particle D on the plane D Q, remains to be examined. This motion impresses upon all the particles, situated in the plane D Q, a centripetal force, towards the centre C.

Let any other horizontal motion whatever, not coincident in direction with D C, be impressed upon the same particles: Under the government of these two forces, the particles will describe round the centre C areas proportional to the times, and by the equilibrium of these motions, they may assume an horizontal circular rotation.

Let us imagine, that during this horizontal circulation, the particle D, in its approach toward the centre C, as in a spiral, shall describe circular orbits, of which the diameter is successively diminished; let us call the velocity of rotation of the particle D $= v$; its distance from the centre $= r$; the time of one revolution $= t$; and since the areas must be as the times, we shall

* Hydrodyn. No. 432.

have

have nearly $v=\frac{1}{r}$; $t=r^2$; and the centrifugal force of the particle D will be $=\frac{1}{r^3}$. When we attentively obſerve the particles which revolve at the ſurface of the funnel, at M N, we ſee that the effect which really takes place in nature, is nearly $t=r^2$. Since, therefore, the centrifugal force in approaching the centre C increaſes as $\frac{1}{r^3}$, it will become equal to forming an equilibrium againſt the upper preſſure S D, which produces the centripetal force D C. A cavity, K R T H P v, will therefore be formed, round which the whirling fluid will ſupport itſelf by the centrifugal force of its rotation.

Let D Q P R repreſent a circular fluid zone, the particles of which turn round the cavity R P, according to the law here indicated. Let the gravity of a fluid particle be $=g$; C R $=a$; R D$=b$; D X$=z$; X Z$=dz$; and the velocity of the particle D$=v$. If the centrifugal force of the particle D were equal to its gravity, its velocity, by the thorems of Huygens, would be equal to that of a body falling by gravity alone, through the ſpace $\frac{a+b}{2}$. And ſince an heavy body falls in one ſecond through the ſpace of 181 inches$=$S, the velocity of the particle D on the ſame ſuppoſition would be $=\sqrt{(2\ S\ (a+b))}$. The centrifugal force in the circle is as v^2; the centrifugal force of D will therefore really be $=\frac{-v^2 g}{2\ S\ (a+b)}$. And ſince the centrifugal force is $=\frac{1}{r^3}$; taking $\frac{1}{(a+b)^3} : \frac{1}{(a+b-z)^3} = \frac{-v^2 g}{2\ S\ (a+b)}$: a fourth term, we ſhall have the centrifugal force of the element of D X in X$=\frac{v^2 g\ (a+b)^2\ d\ z}{2\ S\ (a+b-z)^3}$; and that of the filament D X$=$A$+\frac{v^2 g\ (a+b)^2}{4\ S\ (a+b-z)^2}$. When $z=o$ the integral is $=o$; whence A $=\frac{-v^2 g}{4\ S}$. Taking $z=b$, the centrifugal force of the filament D R will be$=\frac{bgv^2}{4\ a^2\ S}$ $(2\ a+b)$. The quantity $b\ g$ is the gravity itſelf of the filament D R. The gravity of this filament is therefore to its centrifugal force $=v^2\ (2\ a+b) : 4\ a^2$ S.

When the fluid zone, D R P Q, is nearer the aperture E F, the preſſure S D increaſes; whence the centrifugal force of the zone muſt alſo be increaſed by diminiſhing the radius of the cavity R C: hence we may determine the nature of the curve which forms the perpendicular ſection of the cavity K R T. For greater ſimplicity, let us ſuppoſe that the ſides of the veſſel have the ſame form M D as that of the cavity itſelf, ſo that D R$=b$ may be conſtant. Let A C$=x$; and C R$=y$. Let us ſubſtitute y inſtead of a in the preceding formula. And ſince the gravity of the filament D R, is to the gravity of the filament S D$=b : x$, we ſhall have by compoſition of ratios, the centrifugal force of the filament D R, to the preſſure S D$=$ $b^2\ v\ (2\ y+b)$; $4\ x\ y^2$ S. Theſe quantities muſt be equal, in order to afford an equilibrium. We have therefore $x\ y^2 \frac{b\ v^2\ y}{2\ S} - \frac{b^2\ v^2}{4\ S} = o$ for the equation of the curve K R T. This is the ſixty-fourth ſpecies in the enumeration of lines at the third order, by Sir Iſaac Newton. Its convexity is turned towards the axis; it has two a ſymptotes, one of which is the axis A Y, and the other is in M N, ſuppoſing the two points M N to be infinitely diſtant.

If the aſſumed poſitions in this theory do not abſolutely coincide with nature, they approach its effects very nearly. It is not only poſſible, but there does exiſt in nature, a whirling ſtream, of which the cavity turns its convex part to the axis, and in which $t = r^2$ very nearly, as is ſhewn by experiment.

Experiment xxvi. Let the orifice E F be opened, and any motion whatever be impreſſed on the fluid, independent of that which its gravity, and the preſſure of the circumambient particles, tend to produce; the turning immediately begins, and is ſeen to be more rapid in thoſe parts of the fluid which are neareſt the bottom. The cauſe of this is, that the motion D B is more convergent and perceptible in thoſe parts which are neareſt the orifice E F*. The centripetal force D C, produces its effect there rather than at the upper parts. Theſe laſt afterwards fall into the cavity which begins to be formed below, by which means they alſo acquire a centripetal force, and the funnel or cavity opens to a much greater height, than that in which the convergence of fluid filaments is obſerved towards the orifice E F, in water which is leſs agitated.

Experiment xxvii. Place a floating body at the ſurface of the fluid, of ſufficient magnitude to prevent the formation of the cavity. If the fluid be much agitated, the cavity will take place at the lower part, and air will introduce itſelf through the opening E F. Whence it follows, that the preſſure of the atmoſphere on the upper ſurface of the fluid is not the cauſe of the cavity, which aſſumes the ſhape of a funnel. The air does not enter but becauſe it finds an empty ſpace formed by a centrifugal force.

Experiment xxviii. When the fluid remains in a ſtate of tranquillity without eddies, the veſſel empties itſelf in forty ſeconds; but when the circular motion takes place, the evacuation is accompliſhed in fifty ſeconds, more or leſs. It cannot, therefore, be ſaid in general terms, that the whirling ſtream abſorbs and draws down bodies through the opening E F with more force than if no ſuch circulation took place.

Experiment xxix. Pour a ſtratum of oil upon the water of the veſſel. As ſoon as the funnel forms itſelf, the oil ruſhes down, and iſſues out before the greateſt part of the lower water, upon which it reſted. The portions of oil partake leſs of the rotation of the lower water; having leſs denſity, they likewiſe recede leſs from the axis than the water; in conſequence of which, as they occupy the interior part of the funnel, and are unſupported, they flow out firſt.

Experiment xxx. Every other ſmall body which floats on the water in the veſſel, acts in the ſame manner as the oil, provided its dimenſions be very ſmall. If the volume of the body be ſomewhat greater, while it approaches the cavity, to fall therein, its extremity, which is neareſt the axis, comes into a place where the circulation is more rapid. This rapidity of motion impreſſed at one extremity of the floating body, is tranſported by the laws of mechanics, to its centre of gravity, which is more remote from the axis, in a ſituation where the circular motion is ſlower, conſequently the body recedes from the edge of the cavity into which it was about to fall. It returns a ſhort time afterwards, is again repelled, and theſe alternate motions continue as long as the circumſtances which produced them. Laſtly, if the body which floats

* Bernouilli, Hydrod. ſect. 4, §. 3. Boſſut, art. 427.

at

at the furface of the liquor after the funnel has been formed, be of f ficient fize to cover the whole cavity, it deftroys the funnel in the upper part, and fometimes alfo in the lower. The reafon is, that the body itfelf cannot turn round its centre but according to the law $v=r$; it therefore deftroys by friction the law $v=\frac{1}{r}$ in the parts of the fluid in contact with it, and confequently it deftroys the funnel itfelf.

PROPOSITION XII.

The lateral communication of motion takes place in the air as well as in water.

The ftream of air which moves in the midft of a body of air at reft, produces undulations and eddies round its current in the fame manner as in water. Thefe may be obferved in the fmoke which rifes from a furnace, and produces a remarkable afpect, when it iffues like a dark tree from an agitated volcano. They may likewife be feen in the particles which float in an obfcure chamber, when a ray of the fun fhines in, and the obferver blows through them.

If a general wind comes, for example, from the fouth, it frequently happens that the north fide of a mountain is at the fame time ftruck by a north wind. This partial and local wind is nothing but the eddy produced by the mountain itfelf acting as an obftacle, againft the principal wind, from the fouth. It is probably from the fame reafon, that the wind fometimes acts in the contrary direction on the fails of a veffel, when they are too obliquely prefented to its ftream.

The vapour of water which iffues from the eolipile carries the furrounding air with it, and drives it againft the burning coals oppofite to the ftream of aqueous vapour. It muft not, therefore, be concluded that the aqueous vapour is itfelf in this cafe decompofed to maintain the combuftion of the charcoal.

It is known that the flues of chimnies affift the rifing of fmoke by their figure; concerning which, we have drawn fome inductions, in the feventh propofition.

In organ pipes, the air which iffues out of the fide opening (lumiere) rubs laterally againft the extremity of the column of the air included in the pipe. It rubs it on one fide in the longitudinal direction, and is, as it were, an elaftic file, acting upon an elaftic furface. Though the column of air be fluid, its parts are, however, fo far intermixed together, that the tremulous motion excited at the place of friction, is foon communicated laterally through the whole thicknefs of the column, which receives vibrations of fuch a kind, that they are an equilibrium with each other, and with the velocity of the ftream which affords the friction. For this effect, it is requifite that the column fhould divide itfelf at different points or nodes diftributed through the length of the tube*. It is by repeated actions that the wind which iffues from the fide aperture impreffes at length upon the whole column contained in the pipe, a movement of vibration greater than that which the laws of impulfe, and of the lateral communication, would permit it to make by a fingle impulfe. In the hautboy, and other fimilar inftruments, having a mouth-piece, or reed, the caufe which excites the tremulous motions, does not act fideways on the air contained in the pipe, but ftrikes the column directly in the middle: for which reafon, it communicates its vibrations with fo much the more effect to the whole mafs.

* Memoires de l'Acad. an. 1762, page 431.

In

In like circumftances the force of found, which is propagated in the atmofphere, depends on the magnitude of the fection of the air which is at the extremity of the pipe, and the amplitude of the vibrations of this fection. It is this furface which ftrikes the atmofphere, and communicates the pulfations*. For this reafon, conical divergent pipes afford a ftronger found than thofe which are cylindrical; and thefe laft afford a ftronger found than pipes which are conically convergent. The firft caufe of the found which acts at the mouth end of the pipe would never, of itfelf, excite fuch ftrong pulfations in the atmofphere, as it does excite by the lateral communication in the air contained in a divergent conical pipe.

The explanation of this phenomenon may be underftood by obferving, 1ft, That if a number of elaftic bodies be difpofed in progreffion, the firft will imprefs upon the laft, by the intermedium of the others, more velocity than would be communicated by the immediate ftroke. 2. The vibrations excited in the pipe, have a certain permanence which permits them to receive an increafe of force by the united effect of fucceffive impulfions; whereas, in the open atmofphere every pulfation is tranfient and alone.

Is not the augmentation of found in the fpeaking trumpet, in part owing to the fame caufe of the lateral communication of motion, rather than to the mere reflection of the fonorous lines from the fides of the tube itfelf?

I call thofe *refonant* vibrations, which take place in a tube when found is excited; and I call thofe *propagated* vibrations, or pulfations, which tranfmit the found through the atmofphere. I have already pointed out a difference, which appears to me to take place between thefe two kinds of vibrations; namely, that the firft have a certain permanence and connection with each other, fo that the latter excites, fupports, and reinforces the former; whereas, the pulfations which fucceed each other in the atmofphere by the repeated action of the refonant body are fingle, and independant of each other.

But the following is a much more remarkable difference between thefe two kinds of vibrations. When at the extremity of a pipe A B C, a refonant vibration is made in the fection of air, B C, fig. 2. Plate VIII, vol. II. experience fhews that this vibration becomes the centre of pulfations propagated all round in P S Q. For on whatever fide the obferver is placed, whether at P or at Q, he will hear the found of the pipe A B C nearly as much as at S. But when there is no pipe, and the vibration at C B is a fimple pulfation propagated through the open air from A to B, in this cafe, the pulfation is not propagated laterally and completely to P and Q like the refonant vibration; but is contained almoft entirely in the limits B Z and P Y, with a divergence of between 15 and 20 degrees. This fact has been difputed by various philofophers; but it cannot be queftioned, fince it is well known that we do not hear the echo, or reflected found, from a plain furface, unlefs we place ourfelves in the line of reflection, or very near it. If the pulfation of the echo were propagated all round, before the reflecting furface, diverging from thence as centre, ought we not to hear the echo in every fituation whatever before that reflecting furface? We muft therefore admit, with regard to fonorous pulfations propagated in the atmofphere, certain exceptions, and even limits, with regard to the lateral communication of motion which we have pointed out in the firft propofition, and in the fifth, with regard to water.

* It is known that the material of which a pipe is made does not perceptibly affect the found.

Addition respecting the contracted Vein.

Much has been written concerning the convergent directions assumed by the particles of a fluid contained in a vessel, previous to their being emitted through an aperture in the side of the vessel itself, and concerning the form of the contracted vein which is thus produced. The reflections and experiments, which I shall proceed to give, may afford some farther explanation in this respect.

I shall begin by defending the fundamental doctrine of hydraulics against the opinion of a learned man, distinguished by his labours and his zeal for the advancement of science: Lorgna, the founder of the Italian society. He pretends * that the contracted vein is nothing else but the continuation of the Newtonian cataract, and that the celerity of the fluid, issuing from an orifice in a thin plate, is much less than that of a body which falls from the height of the charge.

Let M D, fig. 22, Pl. XXII, vol. II, represent the axis of the vein which issues from B. The radius of the circular orifice $BC = BD = 1$; $MB = a$. Lorgna pretends that $0{,}472\ a = HB$, is the height which would produce, in an heavy body, the velocity of efflux in B C; he supports this proposition by computations deduced from the mutual action of the particles of the fluid contained in the vessel. But after having seen the failure of the efforts of the greatest geometers on this subject, we ought to mistrust all these demonstrations founded on mechanical principle, very true in themselves, but of which the application to an infinity of bodies, which move and are pressed in every direction, becomes extremely difficult, if not impossible. Let us see whether the theory of Lorgna agrees with experiment. Supposing the velocity of the fluid at B, arising from the elvation $HB = 0{,}472\ a$, the velocity of the same fluid in D will be increased in the ratio of $\sqrt{HB} : \sqrt{HD}$; and the vein in D will be contracted in the same ratio: whence $DE = \sqrt{}^{4}\left(\frac{0{,}472\ a}{1+0{,}472\ a}\right)$; which is the formula of the hyperbolic conoid of Newton. If this be the sole cause of the contraction, the dimensions of D E ought, very nearly, to agree with this figure when examined by experiment. But they, in reality, differ from it very much, as may be seen in the following table.

Authors of the experiments.	Value of D E found by actual measurement.	Value of D E calculated by the preceding formula.
Poleni (de Castellis § 35.)	0,79	0,97
Michelotti (Sperim. Idraul. tom. I. exper. 46; tom. II. exper. 4)	0,80	0,99
Bossut (Hydrodyn. art. 437 exper. 5)	0,818	0,99
Myself, with 35 inches charge and an horizontal circular orifice of 18 lines in diameter	0,798	0,984

* (1) Mem. della societa Italiana, vol. IV.

It

It is evident that the contraction of the vein, as found by experiment, is incomparably greater than can be produced by the acceleration of gravity, even in deſcending ſtreams. But what can we ſay of horizontal and aſcending jets, in which, aſſuredly, the acceleration of gravity does not take place, but in which, nevertheleſs, the contraction is obſerved nearly in the ſame manner as in deſcending currents? The contraction of the ſtream is, therefore, very different from the Newtonian hyperboloid.

Deſirous of proving that the vein does not poſſeſs the whole velocity ariſing from the height of the fluid above the centre of the orifice, Lorgna relates the experiments of Kraft*, which are not applicable to the queſtion, becauſe they were made with cylindrical pipes; and we have ſeen, that ſuch pipes always deſtroy part of the velocity of the fluid; conſequently, we cannot eſtabliſh any rule from them which ſhall apply to orifices through thin plates†. He wiſhes not to determine the velocity of aſcending jets by the height to which they riſe, becauſe he is apprehenſive that the preceding part of the ſtream or jet is urged, and ſupported by the ſucceeding part nearly to the height of the charge. Nevertheleſs, if we interrupt the jet all at once, the laſt portions of water fly to the ſame height as thoſe which preceded them, without having any continued column of the fluid below to follow and ſupport them: theſe laſt portions muſt conſequently have received, at their paſſage through the orifice, all the velocity which was neceſſary to raiſe them nearly to the ſurface of the fluid in the reſervoir.

Let us confine ourſelves, if it be thought proper, to horizontal jets; the experiment, which I have related as a term of compariſon, appears to me to be deciſive. Under the charge of 32,5 inches the vertical line PM, fig. 1, Pl. VIII, vol. II, being 54 inches, the horizontal line MN was always 81,5 inches, which was only 2 inches leſs than it would have been if the jet had preſerved in the direction of the horizon, all the velocity which a heavy body acquires in falling from the height of 32,5 inches. The diameter of the contracted vein was 14,3 lines very nearly. Since the quantity of 81,5 inches in MN ſuppoſes in the contracted vein a velocity of 149,5 inches per ſecond; this number multiplied by the area of the contracted vein itſelf, gives the expenditure of 4 cubic feet in 41 ſeconds of time, which is alſo the reſult of experiment. We have, therefore, three meaſures determined by experiment, which agree and mutually confirm each other; namely, the quantity MN, the contraction of the ſtream, and the time of expenditure. And ſince the quantities obſerved by Boſſut, Michelotti, and Poleni, give nearly the ſame reſults, it can no longer be doubted; 1, That the contraction of the ſtream is nearly 0,64 of the orifice; 2, That the velocity of the contracted view is nearly the ſame as that of a heavy body which may have fallen through the height of the charge.

Theſe two experimental principles are true in all caſes where the orifice is conſiderably ſmall in proportion to the ſection of the reſervoir, where that orifice is made through a thin plate, and the internal afflux of the fluid filaments is made in an uniform manner round the orifice itſelf. But what would be the conſequence if this internal afflux ſhould be modified in a manner different from what uſually happens? The following experiments were made with the in-

* Acta Petrop. vol. 8.

† Torricelli took notice of this difference at page 168, of his works, "Quotieſcumque autem aqua per tubum "latentem decurrens per anguſtias tranſire debuerit, falſa omnia reperies."

tention of aſcertaining ſome of the moſt remarkable effects of theſe particular modifications in the direction of the fluid filaments, which preſs each other in order to paſs through the orifice.

Experiment xxxi. In the orifice A C B D, fig. 21, Pl. XXII, vol. II, the two ſides A B are parallel to the horizon; the extremities C D are rounded: the width of this aperture is leſs than two lines, its length 18 lines, and the charge 32,5 inches. The ſection of the ſtream, which iſſues from this orifice, firſt aſſumes the form E F; after which, the two extremities E F, approaching nearer and nearer to ſwell the middle part of the ſection of the ſtream, at 4,5 inches diſtance from the orifice, acquires the quadrangular form G H. The ſtream afterwards extends itſelf in the perpendicular direction in the form of a large fan K L.

I have repeated the experiment by placing the longitudinal axis of the orifice E D vertically. In this caſe the ſame phenomena were produced; E F becoming vertical, and K L horizontal, both preſerving their form.

The fluid filaments which, iſſuing out of the orifice, paſs near the two oppoſite borders A, B, are very near each other, being convergent, tend to unite at a very ſhort diſtance from the orifice itſelf. The filaments C, D, are more remote, and, perhaps, leſs convergent; they cannot unite but at a greater diſtance than the two former. In this caſe, therefore, there are movements which tend to form two contractions, the one nearer, and the other more remote from the orifice. Theſe two contractions counterbalance each other in part; their mutual oppoſition carries the effect G H to a diſtance five times greater than that of the contracted vein of a circular orifice, having a diameter of the ſame breadth as that of this orifice.

In this experiment we ſee the cauſe of a phenomenon, which has been obſerved in ſome particular caſes by Poleni and others, without giving the explanation. In every orifice of a right-lined figure through a thin plate, the angles of the contracted vein anſwer to the ſides of the orifice, and the contrary. When the quadrangular orifice has the ſituation M N O P, the greateſt contraction of the ſtream is made at a greater diſtance than in a circular aperture; it aſſumes the form and ſituation Q R S T. The reaſon is, that the oppoſite angles, M, P, are morere mote from each other than the ſides I, V, whence the ſame thing happens as in the long orifice A C B D. In the ſame manner, the triangular orifice in the ſituation X produces a contraction of the form and in the ſituation Z, &c.

Experiment xxxii. The orifice being the horizontal cleft C D, fig. 21, the place G H, or moſt contracted point of the ſtream, was found to be diſtant from the orifice as in the following table:

Height of the charge above the orifice C D. Inches.	Diſtance of the greateſt contraction G H. Lines.
32, 5	53
18	48
10	40
6	36

The long orifice C D exhibits to us, under an enlarged dimenſion, the diſtance of the contracted vein from the orifice. By this means, the foregoing table ſhews us, in a very ſenſible manner,

manner, that the contraction of the ftream takes place at a greater diftance under ftrong charges, than in thofe which have but little elevation.

Experiment xxxiii. To the centre of the circular orifice A B, fig. 23, formed in a thin plate, I difpofed, within the refervoir, the cone of metal D G E, with a cylindrical part C F G D, in fuch a manner that the cone was moveable along its own axis I V, and its fummit, E, could be protruded more or lefs through the orifice A B, approaching or receding from the point V. The meafures in lines were A B=18; I E=24; D G=27; C D=8. This apparatus was applied to the orifice P, fig. 1, Plate VIII; the change being 32,5 inches. The refults were as in the following table.

Quantity E X, by which the fummit of the cone projects beyond the line A B.	Diftance of the contracted vein.	Diftance of M N.	Time of expenditure of 4 cubic feet.
Lines.	Lines.	Inches.	
11,1	9,1	76	85″
6,6	12,3	77,5	53
0	14	78,5	43
The cone removed.	14,3	81,5	41

I intend to repeat and vary this experiment, in order to difcover the caufe of the fingular phenomena which it prefents.

Experiment xxxiv. The orifice being a femi-circle, Plate XXII, fig. 24, vol. II, having the diameter A B, 11,2 lines, I applied within the veffel a plane Q A B, perpendicular to the plate in which the orifice was made. The line A B was perpendicular to the horizon, and the charge 32,5 inches. The jet deviated in the horizontal direction in P F G, departing from the axis C E towards the fide on which the plane Q P was placed. The angle F C E was 9° 5′, and the angle F C G was 36°. The vertical fection of the jet had the form K L, fo that the largeft part of the jet was M F. The four cubical feet of water iffued out in 206 feconds.

The refults of this experiment are analogous to thofe of the experiments xxxi and xxxiii.

Experiment xxxv. Citizen Borda, in a very interefting memoir*, relates a peculiar phenomenon, of which he has given a very fimple demonftration, from the principle of the equality of preffure, which fluids exert in every direction. It is that, if the extremity of a cylindrical tube be pufhed into the interior part of the refervoir, the contraction of the vein is greater, and the expenditure lefs, than if the fame tube be applied to the fide of the veffel. I have repeated this experiment, and obferved a fimilar refult when the tube was cylindrical from one end to the other, like that made ufe of by the author, and when the water was made to flow out in a full ftream. I afterwards gave to the interior extremity of the pipe the form A C, fig. 4, Plate VIII, vol. II, of the common contracted veins; in this cafe, there was no longer any remark-

* Memoires de l'Acad. 1766.

able

able difference between the two expenditures, in the two ſituations of the tube. For when the end AC was puſhed into the interior part of the reſervoir, the full tube afforded, in 81 ſeconds, the ſame quantity of water as was furniſhed in 80 ſeconds, when it was applied to the ſide of the veſſel. It may be preſumed, that if the part A C had more perfectly poſſeſſed the form of the contracted vein, this ſlight difference of one ſecond would have diſappeared*.

IV.

On the Mechanical Fabrication of Lint, and the Manufacture of Hats. By N. L.

To Mr. NICHOLSON.

SIR,

ON the ſubject of dreſſing lint, by engines, the reſult of my inquiries is the following:—It was attempted by a Mr. John Swan, of Whitehaven, Cumberland: the objection made againſt it was, that it broke the lint: could any of your correſpondents, in that part of the country, obtain a drawing or model of the machine; as it may be very likely ſome alteration in it might accompliſh the purpoſe? On the other ſubject of inquiry—Hats, a Mr. Saxton, 45 and 46, Queen-ſtreet, Southwark, is concerned with a houſe in the country, who makes hats with engines, but *where* he may poſſibly inform, I do not know myſelf. Meſſrs. Davis, Welts, and Martin, late of Stockport, now ſomewhere in Kent, make uſe of engines for plaiting hats. There is alſo, I underſtand, another company at Stockport, names unknown, who uſe engines to make ſoldiers' hats: how far might all theſe be improved, ſo as to complete the proceſs? Hats are divided into three ſorts, felts, thoſe made of plain wool, cordies, cordebacks, writ cordies, thoſe covered or otherwiſe with codies wool (wool ſtript from the ſkins of thoſe lambs that die in yeaning, called by the Scotch, mort wool); and the third ſort is called ſtuff, Caroline, or caſtor-hats, that is, hats covered with beaver, or beaver and hares' wool mixed: how did the ſecond ſort, cordies, obtain that name? The reaſon, I have heard, is, they were firſt made in a town of France, whoſe name comes near that this kind of hat has obtained: if ſo, what town is it? Why are the third ſort called *Carolines?* Theſe queſtions may be thought, at firſt, trifling; but when the hiſtory of a manufactory, from its origin, is ſought for, it ſometimes happens that the various names of the article, in queſtion, lead us to the diſcovery. Maſters, in general, are divided about the uſe of galls in their dying: it would be well, if a regular plan could be adopted for the proportion of ingredients, whether of wool or ſtuff; for this we muſt look up to the chemiſts of the preſent day. Allow me to aſk them a queſtion, in the reſolving of which, the dye-houſes in the country, are much intereſted. By what mixture of ingredients, do the London dye-houſes obtain a black colour ſo much ſuperior to thoſe of the country? Dr.

* Here concludes the intereſting reſearches of Citizen Venturi on fluids. At the end of his publication he has given a ſhort ſummary of the contents, which, on account of the length of this concluding part, I ſhall defer to the next number. I take this opportunity of announcing, that Mr. Taylor, bookſeller in Holborn, has applied to me for permiſſion to reprint the tranſlation from this Journal, and that it will accordingly appear in the form of an octavo pamphlet.

Aikin,

Aikin, in his hiftory of Manchefter, fays, that in that part of the country, the hatters boil the felt hats, after bowing and bafoning in a mixture of "ingredients of native growth." I wifh the doctor would inform us, what he means by that phrafe? as the cordies about Manchefter, are better *got up*, than thofe in many other places, and it may be owing to fome vegetable put into the pan during boiling.

I am, Sir,

Your's, &c.

N. L.

V.

Report and Obfervations on the Art of Hat-making. W. N.

BY a private letter of recommendation from the refpectable author of the foregoing communication, I vifited the manufactory of Meffrs. Collinfons', hatters, in Gravel-lane, Southwark; and conclude it will be interefting to many of my readers, to fee an account of the procefs.

The materials for making hats, are rabbits' fur, cut off from the fkin, after the hairs have been plucked out, together with wool, and beaver. The two former are mixed in various proportions, and of different qualities, according to the value of the article intended to be made; and the latter is univerfally, as I take it, ufed for facing the finer articles, and never for the body, or main ftuff. Experience has fhewn that thefe materials cannot be evenly, and well felted together, unlefs all the fibres be firft feparated, or put into the fame ftate with regard to each other. This is the object of the firft procefs, called bowing. The material, without any previous preparation *, is laid upon a platform of wood, or of wire, fomewhat more than four feet fquare†, called a hurdle, which is fixed againft the wall of the work-fhop, and is enlightened by a fmall window, and feparated by two fide partitions from other hurdles, which occupy the reft of the fpace along the wall. The hurdle, if of wood, is made of deal planks, not quite three inches wide, difpofed parallel to the wall, and at the diftance of one-fortieth, or one-fiftieth of an inch from each other, for the purpofe of fuffering the duft, and other impurities of the ftuff, to pafs through; a purpofe ftill more effectually anfwered by the hurdle of wire.

The workman is provided with a bow, a bow-pin, a bafket, and feveral cloths. The bow is a pole of yellow deal-wood, between feven and eight feet long, to which are fixed two bridges, fomewhat like that which receives the hair in the bow of the violin. Over thefe is ftretched a catgut, about one-twelfth part of an inch in thicknefs. The bow-pin is a ftick with a knob, and is ufed for plucking the bow-ftring. The bafket is a fquare piece of ozier work, confifting of open ftrait bars with no croffing or interweaving. Its length acrofs the

* Some writers mention a partial wetting of the fur while on the fkin, by lightly fmearing it with a folution of nitrate of mercury to give it a curl. Meffrs. Collinfons do not ufe it, nor any other preparation.

† I give the numerical eftimates, not from meafure, but by memory.

bars

bars may be about two feet, and its breadth eighteen inches. The ſides into which the bars are fixed, are ſlightly bended into a circular curve, ſo that the baſket may be ſet upright on one of theſe edges near the right hand end of the hurdle, where it uſually ſtands. The cloths are linen, and dyed of a dark olive brown. Beſides theſe implements, the workman is alſo provided with brown paper.

The *bowing* commences by ſhovelling the material towards the right hand partition with the baſket, upon which, the workman holding the bow horizontally in his left hand, and the bow-pin in his right, lightly places the bow-ſtring, and gives it a pluck with the pin. The ſtring, in its return, ſtrikes part of the fur, and cauſes it to riſe, and fly partly acroſs the hurdle in a light open form. By repeated ſtrokes, the whole is thus ſubjected to the bow, and this beating is repeated till all the original clots or maſſes of the filaments are perfectly opened and obliterated. The quantity thus treated at once, is called a *batt*, and never exceeds half the quantity required to make one hat.

When the batt is ſufficiently bowed, it is ready for *hardening*, which term denotes the firſt commencement of felting. The prepared material being evenly diſpoſed on the hurdle, is firſt preſſed down by the convex ſide of the baſket, then covered with a cloth, and preſſed ſucceſſively in its various parts by the hands of the workman. The preſſure is gentle, and the hands are very ſlightly moved back and forwards at the ſame time through a ſpace of, perhaps, a quarter of an inch, to favour the hardening or entangling of the fibres*. In a very ſhort time, indeed, the ſtuff acquires ſufficient firmneſs to bear careful handling. The cloth is then taken off, and a ſheet of paper, with its corners doubled in, ſo as to give it a triangular outline, is laid upon the batt, which laſt is folded over the paper as it lies, and its edges, meeting one over the other, form a conical cap. The joining is ſoon made good by preſſure with the hands on the cloth. Another batt, ready hardened, is in the next place laid on the hurdle, and the cap here mentioned placed upon it with the joining downwards. This laſt batt being alſo folded up, will conſequently have its place of junction diametrically oppoſite that of the inner felt, which it muſt therefore greatly tend to ſtrengthen. The principal part of the hat is thus put together, and now requires to be worked with the hands a conſiderable time upon the hurdle, the cloth being alſo occaſionally ſprinkled with clear water. During the whole of this operation, which is called *baſoning*, the article becomes firmer and firmer, and contracts in its dimenſions. It may eaſily be underſtood, that the chief uſe of the paper is to prevent the ſides from felting together.

The baſoning is followed by a ſtill more effectual continuation of the felting, called *working*. This is done in another ſhop, at an apparatus called a *battery*, conſiſting of a *kettle* (containing water ſlightly acidulated with ſulphuric acid, to which, for beaver hats, a quantity of the grounds of beer is added, or elſe plain water for rinſing out), and eight *planks* of wood joined together in the form of a fruſtum of a pyramid, and meeting in the kettle at the middle. The outer or upper edge of each plank is about two feet broad, and riſes a little more than two feet and a half above the ground; and the ſlope towards the kettle is conſiderably rapid, ſo that the whole battery is little more than ſix feet in diameter. The quantity of ſul-

* For the cauſes and mechaniſm of felting, ſee the note in Philoſ. Journal, I. 400.

phuric

phuric acid added to the liquor is not ſufficient to give a ſour taſte, but only renders it rough to the tongue. In this liquor, heated rather higher than unpractiſed hands could bear, the article is dipped from time to time, and then worked on the planks with a roller, and alſo by folding or rolling it up, and opening it again; in all which, a certain degree of care is at firſt neceſſary to prevent the ſides from felting together; of which, in the more advanced ſtages of the operation, there is no danger. The imperfections of the work now preſent themſelves to the eye of the workman, who picks out knots and other hard ſubſtances with a bodkin, and adds more felt upon all ſuch parts as require ſtrengthening. This added felt is patted down with a wet bruſh, and ſoon incorporates with the reſt. The beaver is laid on towards the concluſion of this kind of working. I could not diſtinctly learn why the beer grounds were uſed with beaver-hats. Some workmen ſaid that, by rendering the liquor more tenacious, the hat was enabled to hold a greater quantity of it for a longer time; but others ſaid that the mere acid and water would not adhere to the beaver facing, but would roll off immediately when the article was laid on the plank. It is probable that the manufacturers who now follow the eſtabliſhed practice, may not have tried what are the inconveniences this addition is calculated to remove.

The acid, no doubt, gives a roughneſs to the ſurface of the hair which facilitates the mechanical action of felting. Mr. Collinſon informed me that they uſe nitrous acid in a proceſs called carrotting. In this operation, the material is put into a mixture of the nitrous and ſulphuric acids in water, and kept in the digeſting heat of a ſtove all night. The hair acquires a ruddy or yellow colour, and loſes part of its ſtrength. I did not ſee any part of this proceſs, nor of the material ſo treated; neither did I gather any further information reſpecting its utility, than that, for ſome kinds of work, the carrotted ſtuff is better.

It muſt be remembered that our hat ſtill poſſeſſes the form of a cone, and that the whole of the ſeveral actions it has undergone, have only converted it into a ſoft flexible felt, capable of being extended, though with ſome difficulty, in every direction. The next thing to be done is to give it the form required by the wearer. For this purpoſe, the workman turns up the edge or rim to the depth of about an inch and a half, and then returns the point back again through the centre or axis of the cap, ſo far as not to take out this fold, but to produce another inner fold of the ſame depth. The point being returned back again in the ſame manner, produces a third fold; and thus the workman proceeds, until the whole has acquired the appearance of a flat circular piece, conſiſting of a number of concentric undulations or folds, with the point in the centre. This is laid upon the plank, where the workman keeping the piece wet with the liquor, pulls out the point with his fingers, and preſſes it down with his hand, at the ſame time turning it round on its centre in contact with the plank, till he has, by this means, rubbed out a flat portion equal to the intended crown of the hat. In the next place he takes a block, to the crown of which he applies the flat central portion of the felt, and by forcing a ſtring down the ſides of the block, he cauſes the next part to aſſume the figure of the crown, which he continues to wet and work, until it has properly diſpoſed itſelf round the block. The rim now appears like a flounced or puckered appendage round the edge of the crown; but the block being ſet upright on the plank, the requiſite figure is ſoon given by working, rubbing, and extending this part. Water only is uſed in this operation of

 faſhioning

fashioning or blocking, at the conclusion of which it is pressed out by the blunt edge of a copper implement for that purpose.

Previous to the dying, the nap of the hat is raised or loosened out with a wire brush, or carding instrument, as I understood; but I did not see this done. The fibres are too rotten after the dying to bear this operation. The dying materials are logwood, and a mixture of the sulphates of iron and of copper, known in the market by the names of green copperas and blue vitriol. As the time of Mr. Collinson was limited, and my attention was more particularly directed to the mechanical processes, I did not go into the dye-house, but have no doubt that the hats are boiled with the logwood, and afterwards immersed in the saline solution. I particularly asked whether galls were used, and was answered in the negative. From the candid and obliging manner of Mr. Collinson, I am convinced that his information was correct, and that if he had any secret to reserve, he would not have hesitated in telling me so.

The dyed hats are, in the next place, taken to the stiffening shop. One workman, assisted by a boy, does this part of the business. He has two vessels, or boilers, the one containing the grounds of strong beer, which costs seven shillings per barrel, and is, as I presume, used in this and other stages of the manufactory, as the cheapest mucilage which can be procured; and the other vessel containing melted glue, a little thinner than it is used by carpenters. I particularly asked whether this last solution contained any other ingredient besides glue, and was assured that it did not. The beer grounds are applied in the inside of the crown to prevent the glue from coming through to the face, and also, as I suppose, to give the requisite firmness at a less expence than could be produced by glue alone. If the glue were to pass through the hat in different places, it might, I imagine, be more difficult to produce an even gloss upon the face in the subsequent finishing. The glue stiffening is applied after the beer grounds are dried, and then only upon the lower face of the flap, and the inside of the crown. For this purpose, the hat is put into another hat, called a stiffening hat, the crown of which is notched, or slit open in various directions. These are then placed in a hole in a deal board, which supports the flap, and the glue is applied with a brush.

The dry hat, after this operation, is very rigid, and its figure irregular. The last dressing is given by the application of moisture and heat, and the use of the brush, and a hot iron, somewhat in the shape of that used by taylors, but shorter and broader on the face. The hat being softened by exposure to steam, is drawn upon a block, to which it is securely applied by the former method of forcing a string down from the crown to the commencement of the rim. The judgment of the workman is employed in moistening, brushing, and ironing the hat, in order to give and preserve the proper figure. When the rim of the hat is not intended to be of an equal width throughout, I conclude that it is cut by means of a wooden, or, perhaps, metallic pattern; but as no such hats are now in fashion, I saw only the tool for cutting them round. The contrivance is very ingenious and simple. A number of notches are made in one edge of a flat piece of wood for the purpose of inserting the point of a knife, and from one side or edge of this piece of wood there proceeds a strait handle, which lies parallel to the notched side, forming an angle somewhat like that of a carpenter's square. When the legs of this angle are applied to the outside of the crown, and the board lies flat on the rim of the hat, the notched edge will lie nearly in the direction of the radius, or line pointing to

to the centre of the hat. A knife being therefore inserted in one of the notches, it is easy to draw it round by leaning the tool against the crown, and it will cut the border very regular and true. This cut is made before the hat is quite finished, and is not carried intirely through, so that one of the last operations consists in tearing off the redundant part, which by that means leaves an edging of beaver round the external face of the flap. When the hat is completely finished, the crown is tied up in gauze paper, which is neatly ironed down. It is then ready for the subsequent operations of lining, &c.

The art of invention, as Leibnitz has long ago remarked, does not consist of lucky thoughts and intuitive conceptions, but is a regular operation grounded on a science which may be taught, and of which the rules are most assuredly investigated by every man who afterwards succeeds in his researches. The inventor of commercial objects ought, as I apprehend, first to ascertain whether the process, he means to improve, is capable of rewarding his exertions; and, secondly, what may be the physical and moral difficulties that obstruct his purpose. With a view to these respective departments of inquiry, I asked Mr. Collinson what he conceived might be the proportion of the charge of raw materials, rent, and other expences, compared with that which is applied to the mere fabrication of the article. He seemed to think, that this last might amount to one-third of the whole cost, and that the bowing and basoning might be one-sixth. Now if we take, as an extreme supposition, that the inventor of a machine could accomplish the whole operation at a charge altogether inconsiderable, and that the profits of the manufacturer and shopkeeper may be respectively 20 per cent, we shall find, by a simple calculation, that such an inventor, if he were to be content with the same profit, would be able to under-sell other hatters by about 23 per cent, or if his article were as good or better than theirs, he would have no occasion to lower his price, but would realize an extra profit of 28 per cent as a manufacturer. If his machinery and working processes were to call for half the present disbursement, the difference would only be $11\frac{1}{2}$ per cent in the market; and if he could only do the bowing and basoning, the sale price would be only affected to the amount of about 5 per cent, or six, considered as manufacturers profit; a difference scarcely more than may reasonably be supposed to take place in various manufactories, from the mere differences in skill and diligence between one manufacturer and another. It may, therefore, become a serious question (according to the simplicity, efficacy, and profit of a new plan, which may require a manufacturer to interrupt and lay aside his old practices), whether he should not, in prudence, endeavour rather to improve the state of his affairs by the safe and ordinary methods of industry, punctuality, and economy, than to unsettle his mind, and risk his fortune and tranquillity in this new pursuit. And still more will it become a mere speculative mechanic to consider how far he ought to pursue a scheme, for the reward of which he must either enter, most probably, with borrowed capital, into a business of which he knows nothing, or depend on the knowledge, the candour, and the integrity of a manufacturer for his gains, or the repayment of his labour and expence.

These are the motives for caution which ought certainly to present themselves in the first contemplation of an undertaking of this kind. But, on the other hand, it is too well known in this active manufacturing country, that improvements, if made and conducted with skill and in-

 telligence,

telligence, are productive of wealth, and a very honourable degree of independence and public eftimation. In the folution of a problem of this kind, experiments muft neceffarily be made; and as thefe are attended with expence, and require to be frequently repeated, in order to avoid deception, the inventor will naturally be led to inveftigate and examine the general ftock of facts, which, in other branches of manufacture, or departments in natural philofophy, may bear any relation to his own purfuit. Hence it appears to be of the higheft importance that he fhould poffefs an extenfive acquaintance with incidents and events of this kind. In the purfuit before us, for example, he fhould learn all the methods by which wool, fur, and vegetable filaments are manufactured. The proceffes of beating, bowing, carding, felting, fulling, fpinning, weaving, knitting, and knotting, will pafs in review before him. He will confider which may be moft applicable to his purpofe, and what new modifications or fucceffive applications he may make of thefe refpective operations, of the principles on which they are founded, and of the materials, as well as the mechanifm ufed for carrying them into effect. The certainty of thefe operations, which have been practifed for ages, will give ftrength and firmnefs to his new combinations, and if he poffefs enough of this kind of knowledge, he will, probably, enjoy the fatisfaction of feeing his attempts anfwer his expectations, inftead of being repeatedly mortified and diftreffed by lofs of time in alterations. In the prefent inftance, on which I have meditated but little, I am unwilling to fpeculate in the way of conjecture. Yet I am inclined to fuggeft the enquiry, whether carding, which is rapidly and mechanically done, be inferior to bowing, which does not promife much facility for mechanical operation? Whether a fucceffion of batts or cardings might be thrown round a fluted cone, which rapidly revolving, in contact with three or more cylinders, might perform the hardening, and even the working, with much more precifion and fpeed than they are now done by hand? Whether blocking or fhaping be not an operation extremely well calculated for the operation of one or more machines? Whether loofe weaving and fubfequent felting might not produce a lighter, cheaper, and ftronger article? And how far the mechanical felting, which is not confined merely to the hairs of animals, might be applied to this art? Thefe, and many other queftions, might be put and illuftrated by incidents in the private hiftory of manufactures which have come to my knowledge: but fuch a detail could not be of any advantage, unlefs it were more amply extended than the nature of the prefent publication can allow.

The moral impediments to the fuccefs of inventions, exclufive of thofe which arife from co-partnerfhips, are principally fuch as may be excited by rival manufacturers, or may arife from the difficulty of introducing an article which muft pafs through many hands in its progrefs from the manufacturer to the public. The manufacture of hats is, as I underftand, carried on with capitals much differing in magnitude, and the manufacturer may either fell to the vender, or vend the article himfelf, though I do not find that this is ufually the cafe. The poffeffor of a new invention for making hats might, therefore, ftand alone between the feller of the raw material, and the confumer of the finifhed article, without being neceffarily dependent on a warehoufe-man or retailer. In this refpect, therefore, the purfuit appears to be liable to no objection.

VII.

VII.

A popular Account of Experiments which have been made or attempted for the Purpoſe of obtaining an invariable Meaſure of Length, from the Difference between the Lengths of the ſame Pendulum, when adjuſted to meaſure different known Portions of Time. (*W. N.*)

ABOUT the beginning of the preſent century, and for a conſiderable time afterwards, the attention of ſcientific men, namely Huyghens, Sir Chriſtopher Wren, and others, was directed to the uſeful application of the pendulum, as an univerſal meaſure capable of being obtained from the menſuration of time, by any number of independent obſervers, who ſhould carefully follow the ſame proceſſes of obſervation. Every one knows that the vibrations of a pendulum will be ſlower the greater its length; and that by the repeated meaſurement of the ſame portion of time, the deviations from any given fraction of a day, as for example a ſecond, will be ſhown with extreme accuracy. Thus in the pendulum which ſhall vibrate ſeconds, the difference of leſs than one-thouſandth of an inch in the length will be ſhewn by a variation of one ſecond per day; and if the pendulum, intended to be uſed as a meaſure, were adjuſted ſo as to keep time without varying one ſecond in the week, it ſhould ſeem to follow, that our general or univerſal meaſure would be obtainable to leſs than the ſeven-thouſandth part of an inch, or the two hundred-thouſandth part of the whole. But many difficulties preſent themſelves upon a nearer practical view of the ſubject. 1. If the expanſion and contraction from change of temperature be not provided for, we ſtill have a cauſe of error, which, in a ſteel rod, would amount to a ſecond a day for every four degrees of the thermometer*; and it ſcarcely can happen in any common ſituation, that the thermometer will not vary many degrees in the 24 hours. 2. The wheel-work applied to keep account and to overcome the effect of the aſſiſtance of the air will in moſt, and, perhaps, in every caſe alter the meaſure of the vibration. 3. If the point of ſuſpenſion have any ſpring or mobility, the vibration will be ſlower than correſponds with the apparent length of the pendulum. Without entering into minute deductions reſpecting this variation and its quantity, according to different aſſumed laws, this effect may be eaſily ſhewn by hanging any weight to a ſtring, and holding it in the hand while it vibrates. The vibration will be much quicker if the hand be held ſtill, than if it be ſuffered to follow the motion of the pendulous body. 4. When a body moves in a right line, the whole of its motion may be conſidered as if it reſided in the centre of gravity; but if it move in a curve, one part of the body will move ſwifter than the other, and the centre of motion, or place at which an obſtacle would ſtop it without occaſioning rotation, will be nearer to the outer ſide of the extreme curve than the centre of gravity itſelf. This centre in pendulums is called the centre of oſcillation. It is uſual in public lectures to illuſtrate this doctrine by means of a ſtrait ſtick. If the ſtick be held by one end, and the obſerver ſtrikes an obſtacle downwards with the middle or centre of gravity of the ſtick, his

* Philoſ. Journal, I. 58.

hand

hand will be jarred by a blow upwards, owing to the tendency of the outer half of the ſtick to proceed downwards by virtue of its greater velocity. If the obſtacle be ſtruck with the outer end of the ſtick, the jar will be in the contrary direction, becauſe the whole, or the greateſt part of the force, is exerted within the obſtacle; but if the blow be given at the diſtance of one-third part of the outer extremity, it will be more effectual than in other caſes, and the greater velocity of the external portion compenſating for the greater maſs of the part neareſt the hand, no blow will be felt, becauſe there is no tendency to rotation. The ſtick therefore moves, and if hung up would vibrate as if its whole maſs were collected at the diſtance of two-thirds of its length from the point of ſuſpenſion. And it is accordingly found, that a ſmall ball hung by a thread of that length will vibrate in equal times with the ſtick. Mathematicians, particularly Huyghens, have determined the places of the centres of oſcillation of bodies, variouſly figured and ſuſpended. It is enough for our preſent purpoſe to have ſhewn, that the effectual length of a pendulum cannot be determined without admitting the conſideration of the figure and magnitude of the pendulous body as an element in the proceſs; or to ſtate the effect more immediately, we may obſerve, that if two balls were ſeparately ſuſpended, and ſet to vibrate through equal arcs, having the diſtance between their centres and points of ſuſpenſion reſpectively equal, the larger ball would vibrate ſloweſt, becauſe its centre of oſcillation would be furtheſt from its centre of gravity. Hence it muſt follow, that our two obſervers, who are ſuppoſed to be in ſearch of one and the ſame meaſure, would, independently of the other difficult requiſites, find it neceſſary to uſe balls either equal or proportioned to each other in a known ratio, a condition which is preciſely the difficulty this method is meant to obviate. It will, moreover, be required, in order that the determinations may hold good, that the material, whether metal or any other ſubſtance, ſhould be of uniform denſity throughout, as well as that it ſhould poſſeſs great truth of figure; both which conditions ſcarcely require any remark with regard to their practical difficulties. 5. The force of gravitation is different according to the latitude of the place of obſervation, and as far as obſervations have yet been carried, this variation is not governed by a perfectly regular law. It is not the ſame in the northern and ſouthern hemiſpheres, and is probably affected by the vicinity of mountains, and the poſition of the plane of vibration with regard to them. Theſe cauſes, which do not affect the utility of an inſtrument for meaſuring time, are of great conſequence in the deduction of our linear meaſure. 6. Whether there be any notable irregularity in the rotation of the earth, from which our meaſures of time are derived, may be queſtioned. It is probable, from the evidence of aſtronomical clocks, ſome of which have performed with wonderful accuracy, that there is not; and this evidence is ſtill more confirmed by the correſpondence of the rotations of Jupiter and Mars with reſpect to that of the earth, concerning which, I have heard of no irregularity, excepting in a paper of Dr. Herſchel, in the Philoſophical Tranſactions for 1784. 7. The mechanical determination of the centre of ſuſpenſion is not altogether an eaſy taſk.

From theſe, and, perhaps, other difficulties, the admeaſurement of length by the pendulum had been, for a conſiderable number of years, diſregarded, when in the year 1774, the ſociety in London, for the encouragement of arts, manufactures, and commerce, offered a reward of one hundred guineas, for a mode of aſcertaining invariable ſtandards for weights and meaſures, communicable at all times and to all nations, which offer being continued for three ſucceeding

years,

years, was at length productive of competition on the part of five candidates, one of whom, Mr. John Hatton, watchmaker, of London, propofed in March, 1779, to obtain a meafure by applying a moveable point of fufpenfion to one and the fame pendulum, in order to afcertain the difference of length between two portions fucceffively made to vibrate, and meafure different known parts of time. This attempt, whether from its imperfection in principle, or the difficulties found by its author in carrying it into effect, was not confidered by the Society as intitled to their reward; but as an encouragement for the farther profecution of the fubject, they prefented the inventor with thirty guineas. After this time, the purfuit remained unattended to for fome years, till Mr. Hatton's plan was taken up by the late John Whitehurft, F.R.S. an ingenious mechanic and worthy man, but poffeffed of very little fcience, at whofe houfe I faw the machine going in the year 1786, which he afterwards defcribed in a pamphlet on that exprefs object*. He was, at that time, of opinion, that his pendulum, which confifted of a ball of lead fufpended by the fine flatted fteel wire, called pendulum wire (of which the length was determined by an adjuftable clip), would give a meafure by the difference of its lengths, as before explained, which would not be affected by the dimenfions of the weight itfelf; and his weight was accordingly a very rude-fafhioned round piece of metal. It was not without fome difficulty that I perfuaded him to the contrary, which is a point concerning which men of information cannot have two opinions; but afterwards, when he became convinced, he propofed that the ball fhould be made of a diameter anfwering to fome aliquot part of the meafure expected to be obtained, or deduced from crude experiments, and afterwards corrected from the actual refult; which corrected ball being ufed to obtain a fecond refult, fhould be again corrected, and this procefs of approximation continued till the refults were not found to differ by any perceptible quantity. I have reafon to believe, however, that he never ufed this method, nor even took the trouble to give any precifion of figure to his ball, with which he made the experiments recorded in his pamphlet.

After the death of Mr. Whitehurft, his apparatus came, by purchafe, into the hands of Dr. George Fordyce, F.R.S. who confiderably improved it, and gave a defcription of the fame to the royal fociety†. The moft important improvement confifted in a contrivance to prevent the effect of variations of temperature upon the pendulum, which the reader will perceive to be highly valuable for the mechanical fkill difplayed in the execution, and in principle fomewhat refembling the fixed apparatus defcribed at page 61 of our firft volume. As the fimilitude of the two apparatus of Whitehurft and Fordyce muft render many particulars of the defcription of the fame in both, I have given no copy of the drawings of the former; but fhall fpeak of both conjunctively.

Fig. 3, Plate II, will afford a more perfect notion of the apparatus than may, perhaps, have been conveyed by the verbal defcriptions already given. O reprefents the pendulous ball fuf-

* An attempt towards obtaining invariable meafures of length, capacity, and weight, from the menfuration of time, independent of the mechanical operations requifite to afcertain the centre of ofcillation, or the true length of pendulums. By John Whitehurft, F.R.S. 4to. 34 pages, with three plates. London, 1787.

† *Account of a new pendulum*, Phil. Tranf. 1794; from which our drawing is copied of a fmaller fize, with fome amendments of the perfpective.

pended

pended from Q by the fine flat fteel wire O N. In Mr. Whitehurft's machine, the ball weighed 12251 grains, or upwards of two pounds troy, and the wire, which was flat fteel *tempered*, was of fuch a thicknefs, that 80 inches in length weighed nearly three grains. The piece W I affords a notch through which the wire paffes, and which limits the length of the effective pendulum. This piece is fixed to the frame-plate of a train of wheels, fimilar to that of an eight-day clock, which, by means of a crutch of communication applied to the pendulum-wire, ferves to maintain the vibration, and keep account of the time. The efcapement is the dead beat of Graham *, with the addition of an arm fpringing from the axis of the pallets upwards, and carrying an adjuftable weight; the ufe of which is to fet the pallets themfelves to the fame vibration as the pendulum is intended to perform: a condition which Mr. Whitehurft, in common with other artifts, fuppofed to be of value to prevent the maintaining power from influencing the meafure of time, though this, in fact, depends more particularly on the ftructure and figure of the pallets †.

The principal fupport to which Mr. Whitehurft's apparatus was attached, confifted ‡ of two planks of deal, two inches thick, nine inches wide, and nearly fix feet long, which being framed edge-wife together, left a groove to admit certain bolts, or fliding parts, from the movement S S, by the help of which it can be raifed or lowered, or fixed at any defired elevation. In the actual experiment, therefore, the movement is to be flided up and fixed near the top of the frame, and the pendulum then adjufted to the longer vibration by means of the fcrew A, which raifes or lowers the cock E G N. This, in Mr. Whitehurft's procefs, was one forty-fecond part of a minute. There is a brafs rule or piece 5 feet 2 inches long, and one quarter of an inch thick, fixed at its lower end in one of the planks in a vertical pofition to receive and preferve the meafure. On this brafs piece, at the temperature of 60°, a fine ftroke was then drawn by means of a proper tool applied againft the edge of the plate of the movement. In the next place he flided the movement about five feet lower, and, by means of a fcrew applied to the bottom of the frame-plate, the piece I W was adjufted to the wire till the vibrations became 84 in the minute. At this period, another ftroke was drawn as before on the brafs rule, at the fame temperature of 60°, which is fuppofed to have been kept up during the whole of the operation from firft to laft. This fuppofition of an equal or fteady temperature of the air in an apartment, can hardly be admitted, except with regard to the mean temperature in a clofe fubterraneous place, which is moft fteadily kept during fummer, becaufe the warmer air from above has then no tendency to defcend.

Mr. Whitehurft's machine was fixed in the corner of a firft-floor room, in Bolt-Court, Fleet-ftreet, which, I think, was wainfcotted. The meafure obtained as the difference of the two lengths aforefaid, vibrating through femi-arcs of 3° 20′, was 59,892 § inches, and the length of feconds pendulum for London, thence refulting, proves 39,1196 inches, inftead

* Philof. Journal, II. 52. † Ibid, p. 49, 50.

‡ Dr. Fordyce retained this part in his improved machine. Philof. Tranf. 1794, p. 16.

§ Sir G. Shuckburgh Evelyn, who afterwards meafured it, found it about 0,0015 inches longer at 64°, which is nearly equivalent to the expanfion of brafs from 60° to 64°. See Philof. Journal I. 58, and Phil. Tranf. 1798, p. 135.

of

of the commonly ſuppoſed meaſure of 39,2 inches*. He thinks the ſhortneſs of his meaſure a proof, that the maintaining power did not affect the pendulum. But, on this it may be remarked, that this power may either accelerate or retard the time of vibration, according to circumſtances, and that there were other modifying events that would tend to ſhorten his pendulum, ſuch as the unſteadineſs of his houſe, his ſlight frame, and the mode of fixing, as well as the looſe connection of the piece, I W, fig. 3, with that frame, the extent of his arc, and the ſlight ſpring of his wire at the place of flexure.

Notwithſtanding all the objections which may be brought againſt this method of meaſuring the difference of lengths of two pendulums, inſtead of the direct length of one, it obviouſly poſſeſſes conſiderable advantages over this laſt method; and though the experiment requires uncommon precautions before unexceptionable reſults can be expected, yet, upon the general conſideration of the limits of error in a meaſure obtained by this proceſs, and that which might be had by deduction from the aſtronomical obſervations of angles, it appears more than probable, that Hatton's proceſs would prove the moſt accurate. This was, no doubt, a leading motive with Dr. Fordyce to improve the machine of Whitehurſt, and is the chief reaſon why I have thought it might be acceptable to the reader, to poſſeſs an account of facts which are only to be met with in books of confined circulation.

One of the moſt conſiderable, if not the greateſt impediment to accuracy, in Mr. Whitehurſt's experiments, muſt have conſiſted in the variations of temperature to which his apartment was undoubtedly ſubjected. Dr. Fordyce amended the conſtruction of the clock ſo far, that theſe variations became of no conſequence during its adjuſtments for time and the ſubſequent trials of rate. The wire N O, fig. 3, muſt lengthen by heat and contract by cold; in conſequence of which, the ball O muſt riſe and fall, and the length of the effective pendulum between I and V will be more changeable than that of a ſimple pendulum. The remedy of Dr. George Fordyce was founded on the conſideration, that if the piece G G were ſupported upon a metallic rod or other piece of ſuch a length as that the expanſion of this piece upwards, by any increaſe of temperature, ſhould be preciſely ſuch as to counteract the expanſion of the wire and preſerve the length of the effective pendulum unvaried—the frame-plate, S S, would, when the adjuſtment was completed, occupy that ſtation, with regard to the braſs bar, when at 60 degrees, or any other ſtandard temperature, as it would have occupied if that temperature had been permanent during the whole of the adjuſtment; and, conſequently, that the marks drawn on the braſs while at that temperature, according to the previous directions, would not be affected in their truth from changes of this kind. In fig. 2, the maſs A A A, repreſents the wooden ſupport of the clock: P, repreſents the head of the ſcrew, which is denoted by the ſame letter in fig. 3. The ſcrew, and conſequently the piece E G N of fig. 3, is ſupported entirely by the extremity of the lever N H P. This lever, the unflexibility of which is ſecured by the triangular framing M L K, reſts upon a braſs tube I I, and is provided with a counterpoize, O, equal in weight to that of the pendulum, and ſerving to ſecure the tube I I from any ſtrain or flexure that might ariſe from the

† George Graham's numerous experiments, looſely quoted, without reference, by Deſaguliers, in his *Courſe of Lectures*, I. 437, gave the meaſure between 39,133 and 39,125 inches by compariſon with the ſtandard yards at Guildhall and at the Exchequer.

weight being all on one ſide. The evident advantages of the metallic ſupport I I being made in the form of a tube are, that it will be more rigid than the ſame maſs any otherwiſe diſpoſed, and that it will more ſpeedily, on account of its ſurface, acquire the temperature of the ſurrounding air. B B is a braſs ſupport to which the tube C C is firmly fixed or ſoldered. This laſt tube is ſlit longitudinally, and fits upon the tube I I, ſo as perfectly to ſteady it, but, at the ſame time, to leave it at liberty to ſlide up and down with conſiderably facility. A ſtronger piece E D F ſurrounds the tube C C, and when the thumb-ſcrew G is tightened, it cauſes C to embrace I firmly, and confine it from ſliding at all. Imagine the tube I I to be continued downwards; and at ſome diſtance lower upon the wooden frame of ſupport there is ſcrewed the piece A D, fig. 1. This piece, at its outer face, affords a dove-tail groove for a ſliding part, G G, to move in. The ſliding part carries a piece which forms two half holes at K, which embrace the tube I I, and hold it firmly when the thumb-ſcrews L L are ſcrewed up: the adjuſting ſcrew H H is previouſly uſed to ſet the ſliding piece to any deſired poſition. If we now connect the whole together, and ſuppoſe the tube I to be graſped by the piece K while it remains unconfined at D, in fig. 2, the whole pendulum will be raiſed and lowered by the expanſions and contractions of the tube I I, which is above K. The centre of ſuſpenſion V, fig. 3, will alſo be liable to ſome change of place from thermometical variations of the wooden frame, and the ball of the pendulum will be lowered by the expanſion of the ſteel-wire from the ſame changes as cauſed them to riſe from the expanſion of the tube I I. Whatever may be the metal of the tube, it is eaſy to conceive, that if it be very long, its effect will predominate over that of the expanſion of the wire, and, on the contrary, that if it be ſhorter than a certain length, the contrary effect will take place. That the tube ſhall accurately compenſate for the changes in the pendulum is a matter of experiment or trial; and as braſs expands more than moſt other metals in common uſe, it is clear, that a ſhorter tube or braſs will anſwer the purpoſe better than one of any of the other common metals. To make the adjuſtment, nothing more is required than, in the firſt place, to ſcrew the tube faſt by the nut G, fig. 2, and afterwards looſen the piece K, fig. 1. The ſliding piece G G may then be moved upwards or downwards, by means of the ſcrew H H, accordingly as the obſerved effect of temperature upon the rate may have indicated the neceſſity of ſhortening or lengthening the acting part of the tube. The ſcrews L L, fig. 1, may then be again ſcrewed up, and the ſcrew G, fig. 2, looſened.

It is rather unfortunate, that neither the publication of Mr. Whitehurſt, nor that of Dr. Fordyce, contain any regiſter of the going of this clock. The former had no tranſit inſtrument, but carried his time from his neighbour, Mr. Mudge. The latter, who had miſlaid the particulars of his obſervations, kept his inſtrument going at the vibration of ſeconds for about nine months, during which, the thermometer had fallen as low as 15° of Fahrenheit, and riſen as high as 84°. The greateſt difference from its rate of going was, in the words of the Doctor; "Counting on according to the rate of its going, during the whole time, it never exceeded the ſum half a ſecond, nor was ever leſs than half a ſecond, whether it was taken from day to day, month to month, or from any one to any other period during the obſervation," which, I think, is at leaſt as accurate, if not more ſo, than the performance of any other time-piece.

It is ſtill more unfortunate, that Sir George Shuckburgh Evelyn, in his late reſearches to aſcertain

ascertain a ſtandard of weight and meaſure, did not find any wire which would ſupport the weight of the pendulum in this machine during any conſiderable period of time, while it was in his poſſeſſion in 1796, though he uſed wire nearly twice as think as that of Mr. Whitehurſt. From this incident, I ſhould have ſuſpected that Mr. Whitehurſt himſelf might have annealed his wire to a certain point, as he ſaid it was *tempered*, if it did not appear from a paſſage in Dr. Fordyce's paper, that he himſelf uſed it from the bobbin. For, he ſays, that the curvature was not entirely unfolded for ſome months, as he concludes from the clock having loſt in its rate during that time.

Upon the whole, then, though the apparatus for making experiments with pendulums is brought to a conſiderable degree of perfection, yet it appears that the fundamental experiments ſtill remain to be made, in order to afford a ſtandard meaſure; the old reſult of Whitehurſt being the only one we are, at preſent, in poſſeſſion of.

VIII.

On the Vibration of the Wings of a Fly. By S. R.

To Mr. NICHOLSON.

SIR,

THE connection of the different departments of ſcience with each other is often ſhewn in reſearches where it might leaſt be ſuſpected, and it frequently happens, that ſubjects of no apparent or immediate utility may give birth to reflections which afford an innocent and intereſting ſource of entertainment, and often lead to more valuable conſequences. Some time, in the courſe of laſt ſummer, my thoughts were accidentally directed to the ſolution of the problem of the frequency with which flies and other ſmall creatures move their wings. If we were to examine the facts upon the general principles of mechanics, taking for our data the ſize and inclination of the wing, and the velocity of flight, we ſhould certainly obtain a very rapid ſeries of ſtrokes; but there are two other methods of direct experiment, the firſt deduced from the doctrine of ſound, and the latter from optical principles, which give an immediate reſult. As the number of vibrations performed by a ſonorous body in a ſecond, to produce a given note are known, and no inſect has any other voice than that which is afforded by the immediate vibration of its wings or other mechanical action, we may eſtimate the frequency of vibration in the bee, the fly, and the gnat, from the reſpective notes with which their tones are in uniſon. But, into this enquiry I ſhall not enter, becauſe the other optical method has anſwered my purpoſe.

M. D'Arcy *, Dr. Watſon †, and others, have ſhewn that the impreſſion of light upon the eye endures for a time after the object has ceaſed to exert its action, and this truth is familiarly illuſtrated by the common experiment of whirling the end of a lighted ſtick; the ignited extremity of which will appear to form a circle, if the motion be quick enough to bring the end round to its firſt place, before the original impreſſion has ceaſed. When a common fly moves horizontally towards the ſun, and the obſerver views him in a line at right angles to his flight

* Memoirs of the Paris Academy, 1765. † On time.

(both which events are continually preſenting themſelves), the ſtrokes of his wings may be obſerved from the reflections of the light, which exhibit a row of bright ſpecks or ſtars. When the fly moves at the rate of about five feet per ſecond, the intervals are about ten in the inch; but in very quick flight, to avoid danger, he can advance near three quarters of an inch per ſtroke. Now $5 \times 12 \times 10 = 600$ ſtrokes per ſecond, in the firſt caſe. I could not eſtimate the velocity in the ſecond caſe; but ſuppoſe it might be ſix or ſeven times as much, or about 30 feet per ſecond*. No ſound was produced in either caſe; whence, I ſuppoſe, the ſound of the wing, when the fly is caught, or the ſhrill tone of an gnat when it ſticks itſelf in the melted tallow of a candle, are produced by much ſtronger and quicker actions.

If you ſhould think theſe obſervations may be acceptable to your readers, they are at your ſervice; but if not, excuſe the trouble I give you, and ſuppreſs them.

I remain, Sir,

Your obliged reader,

S. R.

IX.

An Account of ſeveral Veins of Sulphate of Strontian or Strontites, found in the Neighbourhood of Briſtol, with an Analyſis of the different Varieties. By WILLIAM CLAYFIELD†.

THE firſt ſpecimen of ſulphate of ſtrontian was ſhewn to me by Mr. Tobin, about three years ſince. At that time it was generally believed to be merely a variety of ſulphate of barites. It had been found at Redland, a ſhort time before, in a vein of conſiderable thickneſs. The greater part of this vein has received a red tinge from the iron ſtone on which it lies, and exhibits but ſlight traces of any regular cryſtallization. In ſome few ſituations, however, it is entirely free from colour, and appears to be compoſed of a confuſed maſs of bevilled tables, looſely adhering together. Its ſpecific gravity varies from 3,51, to 3,87. Walking along the beach at Auſt-paſſage, in June, 1797, I met with a ſimilar ſubſtance, and ſoon diſcovered ſeveral detached veins in different parts of the cliff. The ſtrata in which theſe veins are found are nearly horizontal, conſiſting of limeſtone of different degrees of hardneſs, and argillaceous ſandſtone, intermixed with clay and gypſum. The whole cliff, as well as the ſurrounding country, have evidently been produced by aqueous depoſition, ſince which period, the level of the water in the main channel having been conſiderably lowered, the Severn current has acquired ſufficient force to deepen the bed of the river by plowing up the ſtrata which had been previouſly formed.

A ſecond depoſition of the ſoil towards the mouth of the river, by forming diagonal fiſſures in the cliff, has occaſioned an inequality of five or ſix feet in the level of the ſtrata.

Theſe fiſſures are moſtly filled up with veins of ſtrontian, from three to twelve inches in thickneſs, conſiſting of an aſſemblage of ſemi-tranſparent cryſtals, flanked up on each ſide by a thin layer of a fibrous fracture; both of a delicately blue tinge. This laſt variety was obſerved

* A race-horſe, at firſt ſtarting, will clear 90 feet per ſecond, making about four, or, perhaps, five leaps each ſecond.

† From "Contributions to phyſical and medical Knowledge, &c. &c." collected by Thomas Beddoes, M. D. See the next article.

about

about three months since, by Mr. Deriabin (inspector of the Russian mines), who was immediately struck with its resemblance to a substance of the same nature found in Pensylvania.

The crystallization of the middle vein is either that of bevilled tables, or rhomboidal cubes of nearly an inch in diameter; the transparency of the latter exceeds that of every other species; the specific gravity of the cubes varied from 3,88 to 3,96, while that of the fibrous was about 3,91.

Wishing to obtain some muriate of barites, about nine months since, I reduced a portion of the spar to the state of a sulphure, and dissolved the earth in marine acid; the great solubility of the salt, with its needle-formed crystals, soon indicated the presence of strontian.

Several trials which were then made with it, fully confirmed the result of the first experiment. Shortly after this, Dr. Beddoes informed me of his having met with a paper of Klaproth's, containing an analysis of the American sulphate.

Since the first discovery of this rare production, Mr. Bright has furnished me with specimens of another variety from the neighbourhood of Ham-green, where it is found breaking through the soil in such large masses that it has been made use of in mending the roads. The crystallization of the latter, like that of the Redland, consists of bevilled tables; it does not, however, partake either of its tinge, or semi-transparency: its specific gravity is between 3,60 and 3,68.

The present state of the arts furnishes continual instances of the refuse of one manufacture forming the basis of a second. While this continues to take place, it is evident that every mere production must claim a full investigation. The peculiar properties of this earth renders it probable, that its affinities may shortly be made to furnish us with those productions from the raw materials of our own island, which we can now only obtain with considerable difficulty from other countries.

The following analysis was undertaken at the solicitation of some chemical friends; more leisure would doubtless have contributed to greater accuracy. What is now stated is the mean result of several experiments, the differences of which have rarely amounted to more than two or three grains. Considering the fibrous variety as the most deserving attention, it was the first subjected to analysis.

To find whether it contained any portion of water, or other volatile material, 500 grains were exposed to a red heat under a muffle: the loss, amounting to no more than four grains, proves than the quantity of water, if any, must have been very trifling.

1. 200 grains of the powdered spar, in its original state, were digested with a solution of carbonate of pot-ash (obtained by deflagrating nitre and tartar); the powder, when dried in a red heat, weighed 163,5 grains.

2. A solution of this powder in diluted marine acid extricated 47 grains of carbonic acid, leaving about one grain undissolved: this was afterwards taken up by the alternate application of carbonate of pot-ash and marine acid. From this it appears that the whole quantity of earth must have been very nearly 116,5 grains.

3. The solution, No. 2, was then fully charged with caustic ammoniacal gas, which produced scarcely any traces of precipitation; the addition of carbonate of ammonia immediately threw down a precipitate, which, dried as before, weighed nearly 160 grains, the difference

in

in the weight of the precipitate, and that of the 162,5 grains taken up by the marine acid, arofe from decanting the folution into different veffels.

4. To detect any fmall portion of barites which might be contained in the precipitate, a quantity of marine acid was poured on it, fufficient to diffolve only a few grains of the earth. Had any barites been prefent, it would have been taken up in preference to the ftrontian, from its fuperior affinity for the acid; the folution, however, after digeftion for feveral hours, ftill cryftallized in needles, and afforded a copious precipitate to baritic lime-water.

5. 164 grains of the precipitated carbonate of ftrontian were thrown into marine acid, which extricated, as before, about 47 grains of carbonic acid, leaving nearly one grain undiffolved: the addition of fulphuric acid to the folution reproduced 200,8 grains of fulphate of ftrontian.

6. The folution of pot-afh, No. 1, was then taken, and the whole of the carbonic acid remaining in it difengaged by an excefs of marine acid: the addition of muriate of barites afforded a precipitate, which, after drying in a red heat, weighed nearly 249 grains: had the whole of the 200 grains been decompofed, the folution would have furnifhed nearly 250 grains of fulphate of barites.

Klaproth and Dr. Withering having eftimated the quantity of fulphuric acid contained in artificial fulphate at 33, and Fourcroy at 35, of the fpecific gravity of 2,24, or that contained in fulphate of pot-afh; before the concentration of the acid contained in fulphate of ftrontian could be known, it was neceffary to make the following experiments:

7. 218,5 grains of artificial fulphate of barites were decompofed by digefting with a folution of carbonate of pot-afh, producing 190 grains of carbonate of barites, from which marine acid feparated 42 grains of carbonic acid, leaving nearly 140 grains of earth in folution: from this experiment it appears that fulphate of barites contains about 32,2 per cent of acid.

To find the concentration of this acid, 124 grains of fulphuric acid of 1,843 fpecific gravity, containing (according to Kirwan's table, in the Irifh Tranfactions) 109,12 grains of ftandard, or 97,42 grains of 2,24, were precipitated by baritic lime-water, producing 283,3 grains of fulphate of barites, containing nearly 34,4 per cent of acid of 2,24 fpecific gravity.

9. 92,2 grains of the fame acid were precipitated by a folution of muriate of barites, the fulphate of barites weighed nearly 212 grains, containing about 34,1 per cent of acid of 2,24. By taking the mean of thefe experiments, we may eftimate the quantity of acid contained in fulphate of barites at 33 per cent of the fpecific gravity of 2,24.

According to this calculation, the 250 grains of fulphate of barites, No. 6, would furnifh 82,5 grains of acid of the above ftrength.

10. To afcertain the difference between native and artificial fulphate of ftrontian, 204,2 grains of fulphuric acid of 1,843 (containing 160,44 grains of 2,24 fpecific gravity) were precipitated by ftrontian lime-water, producing 360 grains of fulphate of ftrontian, containing about 44,5 per cent of acid. This accounts for the 200,8 of fulphate being produced from 163 grains of carbonate of ftrontian, No. 5. Hence the proportion of acid in the artificial will exceed that of the native nearly 2 per cent.

11. To determine whether the folution, No. 2, contained any calcareous earth, a fmall quantity of oxalic acid was added to it; no precipitation, however, took place.

12. Pruffiate

12. Pruſſiate of pot-aſh occaſioned a ſlight blue tinge.

The different varieties containing, ſo nearly, an equal proportion of earth and acid, the ſtatement of a ſingle analyſis will be ſufficient for the whole. Should there be any difference between theſe, it will probably be found in the Ham-green varieties, containing rather more acid; the quantity of ſulphate of barites produced from it amounting to nearly 252 grains.

From the foregoing experiments, it appears that 200 grains of the fibrous variety contains:

Strontian	116,5
Acid of 2,24	83,5
With a ſmall proportion of iron	
	200,0

In addition to the modes of diſtinguiſhing the two earths already noticed, we may ſtate the cryſtallization of the ſulphures.

A warm ſolution of the ſulphure of barites depoſits, on cooling, an aſſemblage of ſeveral very thin layers of inclined oval plates, terminating in points, and radiating from a centre, while the ſulphure of ſtrontian runs into a baſe line ſupporting a number of parallel perpendiculars, gradually leſſening ſo as to form the diagonal of a ſquare.

The ſtrontian earth in a ſtate of purity frequently varies in its cryſtallization, ſometimes depoſiting ſolitary tables, and at others, arranging them in irregular lines. Both barites and ſtrontian combine with phoſphorus, and exhibit familiar appearances to the phoſphure of lime. Mixed with a few grains of oxygenated muriate of pot-aſh, and triturated in a mortar, an exploſion took place.

Note by the Editor. (Dr. Beddoes.)

Mr. Clayfield has lately been informed that another variety of ſulphate of ſtrontites is foun near Sodbury. Mr. Deriabin has ſeen a blueiſh fibrous variety, from a coal-pit, near Dumbarton. On a profeſſional journey to the north, I was ſtruck at Keſwick with a ſpecimen in Mr. Hutton's collection, labelled *ſtriated gypſum*. It is an exceedingly beautiful white ſulphate of ſtrontian, from Alſton, as the label bears. From Mr. Hutton, I have alſo received a blueiſh ſpecimen, cryſtallized in rhomboidal tables, which I took for ſulphate of ſtrontian; but Mr. Clayfield finds it to be barites. It comes from Cleter-moor, Cumberland. I have another from Newlands, Cumberland, having the exact appearance of the ſulphate of ſtrontian from Ham-green, which requires further examination. Many ſpecimens, ſuppoſed to be baritic, will doubtleſs, on examination, prove to be ſtrontitic. But the diſtinction will require nice inſpection, even from thoſe moſt verſed in the external characters of foſſils.

The following experiments were made, at my requeſt, by a friend:—Twelve grains of carbonate of barites were given to a two-months old rabbit. In half an hour not much affected — n hour nearly dead.—The barites had acted violently as a cathartic, and had produced almoſt general paralyſis. In about two hours, the rabbit died much convulſed. The ſtomach was greatly inflamed; the inner coat ſeparated from that below, lying in folds, and as if half macerated.

macerated. Five grains of barites in a ſecond experiment, and two in a third, killed ſimilar rabbits with the ſame effect on the ſtomach, only the fæces were not ſo much ſoftened. Forty grains of ſulphate of barites had no effect; twelve grains of carbonate of ſtrontites, obtained from the Briſtol ſulphate, acted as a cathartic only: of the ſulphate itſelf, twelve grains had no effect. This ſeems to ſhow that ſtrontites might be tried with little riſk, as a medicine, if any analogy ſhould afford hope of benefit from it.

At my further requeſt the following experiments were tried. Five grains of carbonate of barites, mixed with five of Cayenne pepper, were given to a ſimilar rabbit, which lived five hours, and was not much affected, till within an hour of its death, when it ſuffered as in the firſt experiment, and the ſtomach was ſimilarly injured. Conſidering that ſulphate of barites has no action, by reaſon, probably, of its inſolubility, I thought it poſſible, that innoxious abſorbents might engage the acid of the ſtomach, and prevent the effect of carbonate of barites; in which caſe, under certain circumſtances, we ſhould be provided with an antidote for this deadly poiſon. The following experiments ſhew the conjecture to be erroneous. Five grains of barites, with five grains of prepared potaſh, were given to a rabbit; the animal ſeemed immediately affected, in leſs than an hour was ſcarce able to move, and in leſs than two hours died convulſed, with a ſtomach extremely injured, as before. Five grains of carbonate of barites, with twenty of chalk, deſtroyed a rabbit in five hours: the animal died tranquilly. Carbonate of barites five grains, with olive oil, killed a rabbit in leſs than two hours. The animal having remained all night unopened, the inner coat of the ſtomach was ſo much looſened, as to ſhake out with its contents. Happening to try calcareous liver of ſulphur as an antidote, my friend obſerved the following phenomena.

Four grains of carbonate of barites, with a teaſpoonful of a ſolution of ſulphure of lime, being given to a rabbit two months old, the animal died immediately. The ſtomach being inſtantly examined, was found of a dark colour, and at its curvature (qu. the great curvature?) converted in a yellow ſpongy ſubſtance, which looked as if burned by a cauſtic, and when removed, left but a very thin membrane. The contents of the ſtomach near the darkened part, which ſeemed bounded by a whitiſh line, were alſo dark coloured. The *whole* inner coat was deſtroyed. As this rabbit, two days before, had been ſubjected to an experiment with ſulphate of barites, it was neceſſary to aſcertain whether this had not ſome ſhare in the unexpected effect. A teaſpoonful, therefore, of the ſame ſolution of calcareous ſulphure was given to a full grown rabbit; it had the ſame immediate effect in deſtroying life; and the ſtate of the ſtomach was found to be the ſame. On the 26th of January, 1799, a piece of ſolid ſulphure of pot-aſh, about the ſize of a pea, was given to a rabbit; and an hour afterwards, another piece of the ſame ſize. A few minutes after the firſt doſe, the animal had convulſions of the ſkin, which were conſiderably increaſed by the ſecond. The whole head then began to ſwell, and in two hours the tongue was ſo enlarged, as to be frequently bitten in attempts to maſticate. In two hours and a half, the ſwelling was ſo large, and the breathing attended with ſuch difficulty, and croup-like noiſe, that it ſeemed probable, the animal had but few minutes to live. The operator, heartily ſick of theſe cruel experiments, and unwilling to ſacrifice more victims, gave the rabbit two teaſpoonfuls of olive oil, and was much pleaſed to find that in ten minutes it appeared much relieved, and that the noiſe in reſpiration,

had

had ceased. The animal remained very still, and after oil had been applied to the swellings, was left all night in a basket of hay, in a warm situation.

January 27. The rabbit had left his basket; a little warm milk was given him. In the evening he eat his parsley, and drank his water heartily. On the evening of the 28th, the power of the antidote being supposed to be ascertained, the rabbit was killed, and a surgeon was desired to examine the swellings. He found that they arose from an enlargement of the maxillary glands, and of the lymphatics. The stomach was covered with a number of small specks, and seemed a little softened. It should be observed that this rabbit had been kept on dry food, which may account for the comparatively slow action of the sulphure. An accident confirmed this: in the course of the experiment, having put his nose into water, he received some on his tongue, which being swallowed, greatly increased his agonies. It happened shortly before the oil was given. The sulphure had been carefully prepared. The experiment with the solution of calcareous sulphure was twice repeated: each time the rabbit died in less than two minutes. The stomach was black and yellow, spongy, and much corroded.

X.

Letter from Dr. Beddoes, respecting the Discovery of Sulphate of Strontian, announced at Page 535, Vol. II. of this Journal.

To Mr. NICHOLSON.

SIR,

I SEND you the West-country contributions, supposing that it falls within your plan to announce the publication. You will do this the more readily, as an article in your last number erroneously represents Dr. Gibbes as the discoverer of the sulphate of strontian in this neighbourhood. This mistake you will see corrected in a note* to my table of contents. Whether Mr. Richardson told Dr. Gibbes what he had learned from Mr. Notcutt, viz. *that the specimen he gave the doctor was sulphate of strontian*, I have not yet learned; but I suppose he did, because it is so very natural to do so, in presenting a specimen; and because Dr. Gibbes, as it appears, immediately directed his experiment according to that supposition. The title of the paper, no doubt (Discovery of Sulphate of Strontian, near Sodbury, by T. S. Gibbes, M.B.),

* The note here alluded to is as follows.—As Dr. Gibbes has related an experiment of his own on the Sodbury sulphate of strontian in Mr. Nicholson's Journal, March, 1799, without referring to any other person's previous observations, it seems but just to mention the following circumstances: In the collection of the Rev. Mr. Richardson, at Bath, a friend of mine (Mr. Notcutt), instructed by Mr. W. Clayfield's specimens, pointed out some sulphate of strontian. Mr. Richardson gave a piece to Dr. Gibbes, in January last. I had exhibited this substance, as found in other places near Bristol, to a large audience, nine months before, viz. in Spring, 1798, and had sent specimens to Mr. W. Henry, who communicated the fact to the Philosophical Society, at Manchester. Dr. Gibbes does not, undoubtedly, mean to claim the discovery. He knew last year that Mr. Clayfield was analysing the fossil. Hundreds of persons might have anticipated Mr. C. in announcing the fact. It is, indeed, I observe, distinctly noticed in the *Appendix to the Monthly Review*, vol. xxv, p. 580, June, 1798. The manner in which this sulphate was detected in this neighbourhood is exactly related below.

M. B.), comes from you*. The thing itſelf is of ſmall importance. Nobody thinks it of leſs than the real diſcoverer of the great quantities of this mineral, near Briſtol. But unleſs rightful claims are reſpected in matters of this ſort, the confidence of private communication will be deſtroyed, than which few events could be more baneful to philoſophy. I leave this diſagreeable ſubject, with ſincere regret that juſtice could not be done to the parties by private explanation.

The ſecond edition of my introductory lecture will ſhew you that the project for giving ſelect lectures on anatomy and phyſiology, to a mixed audience, ſucceeded. We have now had two ſuch courſes. I intend (if I can find time) to give an extenſive courſe of phyſiological lectures, principally with a view to inſtruct parents in the means of preventing ſickli-neſs and diſeaſe.

Laſt ſpring, I publiſhed a propoſal for inſtructing mechanics in the principles of chemiſtry. But the advantage of the ſcheme not being felt, it was not carried into execution. But ſince the *Inſtitution for diffuſing Economical and Mechanical Improvements*, has received ſuch reſpectable ſanction in London, I ſhould hope that another attempt may ſucceed at Briſtol.

The *Medical Pneumatic Inſtitution* is open for out-patients; and ſoon will be for in-patients. I ſhall immediately circulate a proſpectus. I hope for further ſubſcriptions. Under the ſuperintendance of a young man of ſuch extraordinary genius, as Mr. Davy, I think we may expect uſeful and curious diſcoveries.

I am, Sir,

Your obedient Servant,

THOMAS BEDDOES.

Clifton, March 24, 1799.

* In every caſe where it has been expedient for me to prefix a title to the communications of correſpondents, I have endeavoured, and I hope with effect, to ſhew that I was the writer. In the preſent inſtance, the title ſeems to be appropriate to the narration, which appears to ſtate Dr. Gibbes to be the diſcoverer what the ſubſtance was.

SCIENTIFIC

SCIENTIFIC NEWS AND ACCOUNT OF BOOKS.

Rectification of Ether.

ONE principal object in the rectification of ether, is to deprive it of the sulphureous acid; for which purpose the addition of a re-agent is necessary. It has been usual to add an alkali. Dizé has found it much more advantageous to add a substance which might afford the requisite quantity of oxygen to convert this into the sulphuric acid; in which state it is not disposed to rise and come over. Various metallic oxydes were tried, among which the black oxyde of manganese proved the best and the cheapest. His process is as follows:

The sulphureous acid contained in unrectified ether being neutralized with oxyde of manganese, the fluid is decanted into a pewter vessel of the capacity of fifty ounces, which is placed on a water bath. To this vessel a head and worm are adapted, the latter of which passes through a refrigeratory constantly supplied with water in a stream from below, which causes the heated water to flow off above. The distillation is then performed by raising the bath to a temperature of 36° (113° Fahrenheit if the decimal thermometer be here meant). The rectification by this treatment usually requires a day to complete it. The flavour of the ether is of the best kind, and the product about one-sixth more than in the usual method with retort and receiver. Dizé has practised this method with success for three years.

Journal de Physique, April, 1798.

Effects of the Gases in producing Sound through a Pipe.

PROFESSOR CHLADNI, well known for his numerous discoveries relating to the theory of sound, engaged professor Jacquin, of Vienna, to make some experiments on the properties of the different gases considered as sonorous bodies, particularly those gases which constitute our atmosphere, and serve to produce vocal sounds. A glass bell was furnished with a metallic cork cemented to a neck at the top; and in the bore of this cock, within the glass, a small flute or pewter (etain) about six inches in length was fixed. The glass being then placed on the shelf of the pneumatic vessel, and filled with any particular kind of gas, a bladder also filled with the same gas, and provided with a cock, was adapted to the external aperture of the cock belonging to the bell-glass. In this disposition of the apparatus, the flute was made to sound by gently pressing the bladder. Comparative experiments were made with atmospheric gas, oxygen, hydrogen, carbonic acid, and nitrous gas. The intensity of the sound did not vary; but when compared with that produced by atmospheric air, the oxygen gas gave a sound half a tone lower; azotic gas, prepared by different methods, constantly gave a sound half a tone lower; hydrogen gas gave nine or eleven tones higher; carbonic acid gas one-third lower, and nitrous gas also very nearly a third lower. A mixture of oxygen gas and azote, in the proportions of the atmospheric air, afforded the tone of this last; that is to say, it was half a tone higher than each of the component parts alone. When the two gases were not uniformly mixed, the sound was abominably harsh. Chladni intends to give a fuller account of these interesting experiments.

Journal de Physique, Vol. IV. N.S. p. 57.

Contribution

Contributions to Phyfical and Medical Knowledge, principally from the Weft of England. Collected by Thomas Beddoes, M.D. octavo, 539 pages. Briftol, printed for Longman and Rees, London.

I received this work too late in the month to give it that attentive perufal which is requifite to enable me to give an account of its contents. The reader has already feen an extract in the prefent number.

Tables Portatives de Logarithms, &c. or Portable Tables of Logarithms; containing the logarithms of numbers, from 1 to 108,000; the logarithms of fines and tangents for every fecond in the firft five degrees, and for every ten feconds of the remaining degrees of the quadrant; and alfo for every ten-thoufandth part of the arc, according to the new centefimal divifions of the quadrant (to feven decimal places): with a preliminary difcourfe on the explication, ufe, and fummation of logarithms, and their application to aftronomy, navigation, practical geometry, and the computation of intereft: to which are added, a table of logiftic logarithms, with other tables of ufe for computing the longitude at fea. By Francis Callet, ftereotype edition; engraved, caft, and printed by Firmin Didot, 1795, in the third republican year. Sold at Paris by Didot, price 14 livres, and in London by Taylor and De Boffe, price 18s.

In addition to the acceptable information to men of fcience, that a good edition of this ufeful work is at prefent to be had, I fhall take this opportunity of noticing the ftereotype method of printing, which Didot has purfued with much fpirit, and with the advantage, as I underftand, of fupport from the French government in thefe tables. The term ftereotype is derived from the words στερεος, *firmus*, τυπος, *nota*; doubtlefs on account of the immutable connection between all the parts of the form from which the impreffion is to be given. I have not heard what may be the peculiarities of the method of Didot, for every ingenious man will make his improvements in the art he undertakes to carry into effect; but I fcarcely need take notice to thofe who are acquainted with printing, that the project of foldering a whole form together, or of cafting a new form from an impreffion made by a general fyftem of types, or page ready compofed, is not in itfelf new. Rochon mentions * the Salluft of Ged, under the title, *C. Crifpi Saluftii Catilinarii & Jugurthini Hiftoriæ, Edinburgi, Guill. Ged, Auri Faber edinenfis non typis mobilibus ut vulgo fieri folet, fed tabellis feu laminis fufis excudebat*, 1744, *in* 16*mo.* which, he fays, is well printed, and perfectly fimilar to a book printed with moveable types. Didot now, it feems, follows the fame procefs as Ged, that is to fay, he cafts plates from which he afterwards makes his impreffions; but the logarithms are from foldered types. The advantage of Ged's method is, that only a few original types, comparatively fpeaking, are neceffary to form the page, which is to ferve as the pattern for the fubfequent cafts; that thefe cafts or plates, fetting the type at liberty to be again ufed in other works, may be preferved for the purpofe of printing as many impreffions of the work as may afterwards be wanted; that the rifk of any greater number of impreffions than the public may actually call for, is, by this invention, done away, and that even the

* Journal de Phyfique, IV. Dr. S. 364. See alfo Profeffor Wilfon's paper in Philof. Journal, 11,000.

plates

plates themſelves may become an article of commerce, not only for ſhop-bills, advertiſements, and other ſimilar articles, but alſo for books of conſtant ſale and invariable ſtructure, ſuch as mathematical works, the claſſics, religious books, and the works of celebrated authors deceaſed. How far this invention might be of value, with regard to books which are altered and improved in every ſubſequent edition, may, perhaps, be queſtioned; but on a looſe conſideration of the ſubject, it ſeems as if it would, in every caſe, be advantageous to a bookſeller to print a few copies of a work, and keep the preſs ſtanding to print others as they may be wanted;—I ſay it would be advantageous if it were not for the immenſe value in types, which would, by that means, be locked up. But the ſtereotype method has all the advantage of keeping the preſs ſtanding, with none of its inconveniences, unleſs the weight and charge of metal in the plates ſhould approach to that of the types they repreſent. To form ſome judgment of this, it may be ſtated, that the works of Virgil, printed by Didot, in 18mo. which I have had from De Boffe, form a beautiful volume of 418 pages, of 35 lines each. The character ranges line for line with that called burgeois, No. 2. in Caſlon's book of ſpecimens, the face of the letter being rather ſmaller; and we are told * that the price of the plates of this work is twelve hundred franks, or 50l. ſterling. From this fact ſome judgment may be formed of the commercial queſtion, but I am not at preſent provided with the means of making the ſtatement.

Beſides the Virgil and the Logarithms, Didot has printed*, the fables of La Fontaine, Cornelius Nepos, Phaedrus, the Vicar of Wakefield, the works of Racine, 5 vols.—of Boileau, 3 vols.—of J. B. Rouſſeau, 2 vols.—and Lady Mary Wortley Montague's Letters. He is now employed on an edition of Voltaire's works. All the works here mentioned are of the eighteen ſize, which, I ſuppoſe, to be better adapted to the preſent ſtate of the art than the larger ſizes, perhaps from the difficulty of inſuring perfect caſts in the latter.

Propoſals for forming by Subſcription, in the Metropolis of the Britiſh Empire, a Public Inſtitution for diffuſing the Knowledge, and facilitating the general Introduction of uſeful mechanical Inventions and Improvements, and for teaching by Courſes of Philoſophical Lectures and Experiments, the Application of Science to the common Purpoſes of Life, by Benjamin, Count of Rumford, F.R.S. M.R.I.A. &c. Octavo, 50 Pages. Sold by Cadell and Davies in London, Price 6d.

This pamphlet conſiſts of an introduction, followed by the propoſals of Count Rumford, which have been adopted by the ſociety. In the introduction he ſtates, how ſlow the advancement of uſeful improvements has ever been, compared with the rapid adoption of thoſe changes which form the object of faſhionable caprice; that the force of habit operates in favour of old methods; that novelty is offenſive, and men aſhamed to learn in departments where they have ſuppoſed their knowledge to be complete; and that the practical introduction of new improvements is rendered more difficult by the ignorance of workmen who continually blunder, as well as their preſumption which leads them to alter what they know nothing of. Hence he deduces the great utility of a general collection of models of things really excellent; not only becauſe the

* La Decade Philoſophique, &c No. 6. An. VII.

instruction

inſtruction of artiſts would be the immediate conſequence, but likewiſe becauſe, even the moſt powerful imaginations would be wonderfully aſſiſted by ſuch an exhibition. The moral conſequences are likewiſe adduced in favour of an eſtabliſhment for this purpoſe. Such a reputable inſtitution would be above the ſuſpicions of intereſted motives in their purſuits. Jealouſy and envy, which never fail to attack ſuch active individuals as exert themſelves in diſintereſted projects for the public welfare, and in too many inſtances deter men from laudable exertions;—theſe mean energies would have leſs power to attack the managers of a public inſtitution, and the managers themſelves, on the other hand, would have the firmneſs, the ſpirit, and the power to reſiſt them. The obſtacles to diſcovery are not confined merely to objects of art. Science alſo ſuffers from the different ſtation, habits, and motives which give energy to philoſophers and practical artiſts. The philoſopher ſeeks fame; the manufacturer, profit. Various inducements tend to prevent a mutual communication between theſe claſſes of men, though both would be highly benefited by a cordial intercourſe. The new inſtitution has this great object in view. One circumſtance more is mentioned by the Count, in addition to the foregoing remarks; namely, that we are indifferent to improvements, and think them of little value, becauſe our forefathers did very well without them; and on this topic he juſtly remarks, that the argument is of no value, ſince it may be brought in defence of the moſt ſavage ſtate of human exiſtence; the only difference between this ſtate and that of higheſt culture and civilization being, that in the latter a proper attention has been paid to mechanical improvements. The introductory part concludes with an hiſtorical ſketch of the origin of the plan now ſubmitted to the public, into which, for the ſake of brevity, I muſt forbear to enter.

The ſame motive induces me conſiderably to abridge the propoſals, or outlines of the plan. The object or purpoſe of the inſtitution is clearly expreſſed in its title. In the execution of the plan, the firſt ſtep of the managers will conſiſt in preparing ſpacious and airy rooms, for the reception and public exhibition of all ſuch new and mechanical inventions and improvements, as ſhall be thought worthy of the public notice. The models or machines will, as much as poſſible, be of the full ſize, and exhibited in action as in their real application. A complete laboratory and philoſophical apparatus will be provided for making experiments; and men of the firſt eminence, will be engaged as lecturers.

The funds are to be ſupplied according to the following conſtitutional regulations: 1. By the ſubſcriptions which ſhall be received from the original founders, and ſole proprietors of the inſtitution, at 50 guineas each perſon, to be but once paid. 2. By the ſums contributed by a ſecond claſs of ſubſcribers, at 10 guineas each perſon, to be but once paid. 3. By annual ſubſcriptions at 2 guineas each. 4. By donations and legacies. And, 5. By ſums received at the door from occaſional viſitors.

The ſubſcribers at 50 guineas are the proprietors of the inſtitution, which is to be veſted in them, their heirs, and aſſigns, individually; but no aſſignment can be valid till ratified by the managers for the time being. One half of the amount of their ſubſcriptions will be permanently veſted in government ſtock, or freehold property, and no part of the money will be demanded of the ſubſcribers, nor any ſteps taken for carrying the plan into execution, before a charter ſhall have been obtained, which will effectually defend the members of the inſtitu-

tion

tion from the general consequences of co-partnership. The proprietors alone will have the privilege of voting for the appointment of managers and visitors, and they alone will be capable of serving in either of those offices. They will have two transferable tickets of perpetual admission into every part of the establishment, and two transferable tickets of admission to all the public philosophical lectures and experiments.

The subscribers of ten guineas each will receive one ticket of admission to every part of the institution, and another ticket for the public lectures and experiments. These tickets will be for life, but not transferable.

The annual subscribers will also receive two tickets for the same purposes, but they will be for one year only, and not transferable. An annual subscriber may become a subscriber for life, by paying eight guineas at any time before the expiration of the year he has subscribed for.

A member of any of the three classes will be entitled to copies or drawings of any of the models for the use of themselves or friends, at their own expence. The institution will be governed by nine managers, chosen by ballot by the proprietors from their own body. These managers will receive no pay or emolument. They are not to dispose of any of the property in premiums. The committee of visitors will also consist of nine proprietors. Their business is to inspect and report upon the state of the institution.

The list of proprietary subscribers on the 7th day of March, was as follows:

Sir Robert Ainslie, bart.
J. J. Anguftein, esq.
Rt. hon. sir Jos. Banks, K.B.
Tho. Bernard, esq.
Scrope Bernard, esq. M.P.
The earl of Besborough.
Rowland Burdon, esq. M.P.
James Burton, esq.
Timothy Brent, esq.
Henry Cavendish, esq.
Rich. Clark, esq. chamb. of London.
Sir John Colpoys, K.B.
John Craufurd, esq.
The duke of Devonshire, K.G.
Andrew Douglas, esq.
The lord bishop of Durham.
The earl of Egremont.
George Ellis, esq. M.P.
Jos. Grote, esq.
Sir Rob. Bateson Harvey, bart.
Sir John Cox Hippesley, bart.
Henry Hoare, esq.
Lord Hobart.
Lord Holland.
Henry Hope, esq.
Thomas Hope, esq.
Lord Keith, K.B.
Will. Lushington, esq. M.P.
Sir John Macpherson, bart. M.P.
Will. Manning, esq. M.P.
The earl of Mansfield.
The earl of Morton, K.T.
Lord Offulston.
Thomas Palmer, esq.
The lord viscount Palmerston, M.P.
Edward Parry, esq.
Rt. Hon. Tho. Pelham, M.P.
John Penn, esq.
Will. Morton Pitt. esq. M.P.
Sir Ja. Pulteney, bart. M.P.
Sir J. Buchanan Riddell, bart.
Count Rumford.
Sir J. Sinclair, bart. M.P.
Lord Somerville.
John Spalding, esq. M.P.
The earl Spencer, K.G.
Sir George Staunton, bart.
John Sullivan, esq.
Rich. Jos. Sullivan, esq.
Lord Teignmouth.
John Thomson, esq.
Samuel Thornton, esq. M.P.
Henry Thornton, esq. M.P.
George Vansittart, esq. M.P.
Will. Wilberforce, esq. M.P.
The earl of Winchilsea.
Hon. Ja. Stuart Wortley, M.P.
Sir Will. Young, bart. M.P. *

* The number of proprietary subscribers on the 23d of March amounted to seventy-eight.

The

The preſent managers are,

For three years.	*For two years.*	*For one year.*
The earl Spencer.	The earl of Egremont.	The earl of Morton.
Count Rumford.	Rt. hon. Sir. J. Banks.	The rt. hon. Tho. Pelham.
Richard Clark, eſq.	Richard Joſ. Sullivan, eſq.	Tho. Bernard, eſq.

By a reſolution of the 9th of March, Sir J. Banks was appointed chairman of all the meetings of the managers which ſhall be held before a charter ſhall have been obtained.

At the end of this pamphlet there is a blank form to be filled up by thoſe who intend to ſubſcribe. In this form, as well as in the body of the pamphlet, thoſe who are deſirous of becoming proprietors, are requeſted to conſider themſelves as candidates for proprietors' places, until they ſhall have been elected as ſuch by a majority of the managers. But I have ſince heard, that the regulation is, that every ſubſcriber of 50 guineas, who ſhall be recommended by a manager, will be admitted of courſe. Forty per cent of the ſubſcription-money will be wanted immediately, that is to ſay, after obtaining the charter; and the remainder may be paid in three equal half-yearly payments, unleſs the proprietor ſhould prefer paying the whole ſum at once.

I am much obliged to Mr. Sheldrake, of the Strand, for his offer of a drawing and deſcription of a machine for ruling lines for the uſe of engravers, which was conſtructed by him ſome years ago: but from his general account of the principal parts, namely, a ſteel rule and a ſcrew, and a ratchet wheel, as wide as from the confined nature of the object, I doubt whether another engraving would be acceptable to the public, however judicious his arrangement may have been.

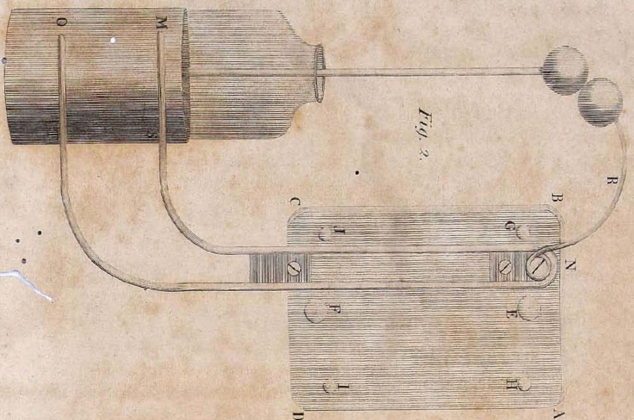

Fig. 2.

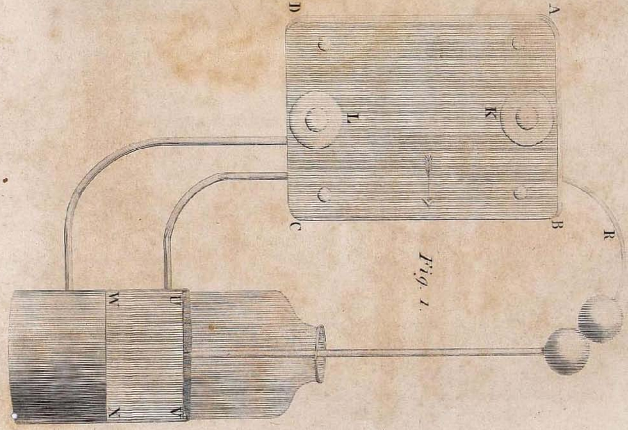

Fig. 1.

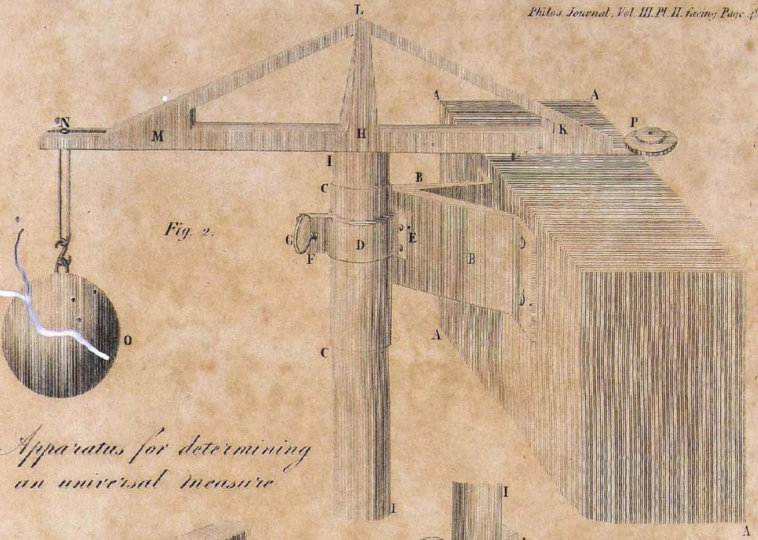

Fig. 2.

Apparatus for determining an universal measure

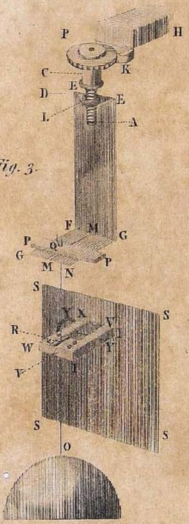

Fig. 3.

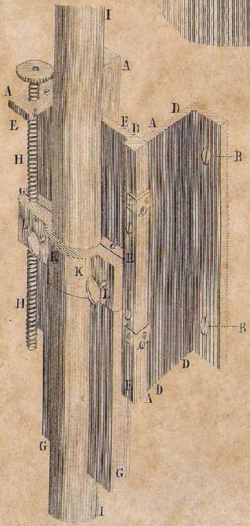

Fig. 1.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MAY 1799.

ARTICLE I.

On the philosophical Uses of a common Pocket Watch.—By the Rev. W. PEARSON.

THE theoretic philosopher, who ranges through the variegated fields of science, may gather the sweets as he passes along, and gratify his taste with fruits that he has had no share in rearing, at a very inconsiderable expence; but the pleasure which the practical man of science derives from the results of his succesful experiments is much more exquisite: the previous impression made by contemplating the harmony that appears in the laws of nature; the satisfaction of proving the agreement or discrepancy between theory and practice; the prospect of benefiting society in some shape, and, perhaps, also the gratification of a certain degree of vanity, all concur in stimulating his exertions. If he should labour under any inconvenience in procuring the best means of prosecuting his labours, he will naturally avail himself of the best substitutes that the circumstances of his situation will allow. From this consideration it becomes a matter of surprize that a more general attention is not paid to the philosophical uses that may be made of a common Pocket Watch. There are many observations and experiments in different departments of science, the accuracy of which depend greatly, and some of them entirely, on the accurate measurement of *minute* portions of time; such, for instance, as the determination of the velocity of sound, the nature of the descent of falling bodies, the measure of the sun's diameter, the distance of two contiguous, or at least apparently contiguous,

heavenly bodies taken at their paſſage over the meredian, and the diſtance of places from the difference of the velocity of light and ſound. A pendulum to ſwing ſeconds has uſually been applied for theſe and ſimilar purpoſes, and in an obſervatory is found to be very convenient; but a watch by being more portable is calculated to be more general in its application, and will meaſure *ſmaller* portions of time than any other inſtrument that has been invented*; beſides, it poſſeſſes this peculiar advantage, that in all ſituations the *beats* thereof may be counted by the ear, at the *ſame time* that the object of obſervation is viewed by the eye, ſo that no *loſs* is incurred, as muſt inevitably happen, when the eye is uſed to view both the object and pendulum or ſecond-index in *ſucceſſion*, though it be ever ſo quick.—But it will be objected here, no doubt, that few watches meaſure time accurately, and alſo that, from the different conſtructions of watches, the times correſponding to their beats vary in a very conſiderable degree. I allow theſe objections to be true, and conceive that the reaſon may be attributed to them, why the beat of a watch is not generally applied as the meaſure of the loweſt denomination of ſubdiviſions of time: I ſhall therefore endeavour in this paper to obviate theſe objections, by ſhewing how any tolerably good watch, whatever be its conſtruction, may be applied with advantage to many philoſophical purpoſes.

We muſt, in the firſt place, conſider that the portions of time which I propoſe to have meaſured by a watch are *ſmall* portions only, and thoſe to be counted not by a ſecond-hand, as is the cuſtom with medical men, but altogether by the beats; in which caſe, if the watch be not liable to loſe or gain time conſiderably in a day, the error in the rate of going will be extremely minute in the time correſponding to any number of beats that the memory can retain, or that the purpoſes to which I propoſe the application to be made will require: and even if the error in the rate of going be conſiderable, ſo as to amount to many minutes in a day, as it is uniform, it may eaſily be allowed for by a correction†. Hence the firſt objection, which relates to the error occaſioned by the rate of going of any watch, will conſtitute no real obſtacle to its application in the aſcertaining of ſmall portions of time, provided a ſudden change of temperature be avoided at the time of uſing it; for it will be neceſſary that the rate of going be eſtimated when the temperature is the ſame, or very nearly the ſame, as when the watch is uſed for philoſophical purpoſes; ſo that if it is uſually worn in the pocket, it may be held in the hand to the ear, but if it be hanging in a room or in the open air where the rate of going is aſcertained, it muſt be hung near the ear, under ſimilar circumſtances, where any obſervation is intended to be made by it.

As to the other objection, which applies to the variation in the lengths of the beats of two different watches, owing to the difference of their conſtructions, though they indicate hours and minutes alike, it may be removed very readily. All common watches have the ſame number

* The beat of a watch is quicker than that of any other chronometer in general uſe; but there have been inſtruments made to divide the ſecond into 100 parts. One of theſe, made by Whitehurſt, and regulated by a fly, repeatedly meaſured the time of fall of a leaden bullet (in ſome experiments which I ſaw) with no greater variation than one hundredth part of the ſecond.—N.

† If the error were five minutes per day, the allowance would be leſs than one three-hundredth part.—N.

ber of wheels and pinions, which are known by the same names, and placed, no matter how variously, so as to act together without interruption; but all watches have not their corresponding wheels and pinions divided into the same number of teeth and spaces, and to this circumstance it is entirely owing that the beats of different watches differ from each other. As the rate of going of a watch is regulated by the lengthening or shortening of a spring, without any regard being paid to the numbers which compose the teeth of the wheels and pinions, a great latitude is allowable in the calculation of those numbers; of which the different makers avail themselves according as the numbers on the engines they use for cutting the teeth require: but whatever the numbers may be of which the wheelwork consists, if we divide double the product of all the wheels, from the centre wheel to the crown wheel inclusively, by the product of all the pinions with which they act, the quotient will be invariably the number of beats of the watch in question in one hour; and again, if we divide this quotient by 3600, the number of seconds in an hour, this latter quotient will be the number of beats in every second, which may be carried to any number of places in decimals, and be copied upon the watch paper for inspection whenever it may be wanted.

When any particular watch is cleaned, the workman may be directed to count, and return in writing, the numbers of the centre wheel, the third wheel, the contrate wheel, and the crown (balance) wheel, and also of the three pinions which they actuate, respectively, from which the calculation of the length of a beat is easily made by the rule just given, and when once made will apply in all instances where that individual watch is used. It will be remarked here, that no notice is taken of the wheels and pinions which constitute the *dial-work*, nor yet of the *great wheel* and pinion with which it acts: the use of the former of these is only to make the hour and minute hands revolve in their respective times, and may or may not be the same in all watches; and the use of the latter, the great wheel and its pinion, is to determine, in conjunction with the number of spirals on the fusee, the number of hours that the watch shall continue to go, at one winding up of the chain round the barrel of the main spring: all these wheels and pinions therefore, it will be perceived, are unnecessary to be taken into the account in calculating the beats per hour. The reason why *double* the product of the wheels specified is taken in the calculation is this, that one tooth of the crown wheel completely escapes the palats at every two beats or vibrations of the balance. A few examples will render the general rule perfectly intelligible. Let us take for the first example the numbers of a common watch given by Mr. Emerson in his "Tracts," which, according to his method of arrangement, stand thus,

48 great wheel,
12——54 centre wheel
6——48 third wheel
6——48 contrate wheel
6——15 crown wheel
2 palats.

Now,

Now, omitting the great wheel and its pinion of 12, we have $54 \times 48 \times 48 \times 15 \times 2 = 3732480$ for double the product of the specified wheels, and $6 \times 6 \times 6 = 216$ for the product of the specified pinions; also $\frac{3732480}{216} = 17280$ are the number of beats in an hour, and $\frac{17280}{3600} = 4{,}8$ the exact number of beats per second: accordingly, Mr. Emerson says that this watch makes "about $4\frac{3}{4}$ beats in a second." The number of spirals on the fusee is 7; therefore, $7 \times \frac{48}{12} = 28$ is the number of hours that the watch will go at one winding up: likewise the dial-work $\frac{40}{10} \times \frac{36}{12} = \frac{1440}{120} = 12$ shews that whilst the first driving pinion of 10 goes twelve times round, the last wheel of 36 goes only once; whence the angular velocity of two hands carried by their hollow axles are to each other as 12 to 1.

For a second example, I will take a watch which is in my own possession, the numbers of which in the calculation of beats per second will be thus: $60 \times 60 \times 60 \times 13 \times 2 = 5616000$, double the product of the wheels; and $8 \times 8 \times 6 = 384$ the product of the pinions; then $\frac{5616000}{384} = 14625$ will be the beats in an hour, and $\frac{14625}{3600} = 4{,}0625$, the beats per second.

Besides this I have examined two other common watches, one of which requires this calculation: $54 \times 52 \times 52 \times 13 \times 2 = 3796416$ for double the product of the wheels, and $6 \times 6 \times 6 = 216$ for the product of the pinions; therefore $\frac{3796416}{216} = 17576$ are the beats in an hour, and $\frac{17576}{3600} = 4{,}882$, the beats per second by this watch: also double the product of the wheels of the other, viz. $56 \times 51 \times 50 \times 13 \times 2$ is 3712800, and the product of the pinions, as in the last, $6 \times 6 \times 6 = 216$; consequently $\frac{3712800}{216}$ gives 17188 beats in an hour, which divided by 3600 gives 4,7746 for the beats per second.

These four examples, it is presumed, will render the method of ascertaining the beats per second in any watch sufficiently easy for any person who is acquainted with common arithmetic.

It remains now for an instance or two to be adduced for the application of the beats of a watch to philosophical purposes, in order to shew the practical utility of the method here proposed of measuring very small portions of time.

Let us suppose, for one instance, with Dr. Herschel, that the annual parallax of the fixed stars may be ascertained by observing how the angle between two stars, very near to each other, varies in opposite parts of the year. For the purpose of determining an angle of this kind, where an accurate micrometer is wanting, let a telescope that has cross-wires be directed to the stars when passing the meridian, in such a manner that the upright wire may be perpendicular to the horizon, and let it remain unmoved as soon as the former of the two stars is just coming into the field of view; then fixing the eye to the telescope and the watch to the ear, repeat the word *one* along with every beat of the watch before the star is arrived at the perpendicular hair, until it is in conjunction with it, from which beat go on *two*, *three*, *four*, &c. putting down a finger of either hand at every *twenty* till the second star is seen in the same situation that

that the leading one occupied at the commencement of the counting; then these beats divided by the beats per second marked on the watch-paper, will give the exact number of uncorrected seconds, by which the following star passes later over the meridian than the leading one: when these seconds and parts of a second are ascertained, we have the following analogy for determining the angle, which includes also the correction, namely, as the time of a sidereal rotation of the earth (which at a mean rate* may be taken at 23h. 56′ 4″,098, but more or less accordingly as the earth is near the aphelion or perihelion of its orbit), the daily error in the rate of going, is to 360°, so is the observed number of seconds (of time) to the quantity of the required angles. The watch is here supposed to be regulated to shew solar time; but if it should be regulated exactly for sidereal time, instead of 23h. 56′ 4″,098 we must use exactly 24 hours in the analogy.

As a second instance, let it be required to ascertain the distance of the nearer of two electrified clouds from an observer, when there are successive peals of thunder to be heard: a little time before the expected repetition of a flash of lightning place the watch at the ear, and commence the numbering of the beats at the instant the flash is seen, as before directed, and take care to cease with the beginning of the report; then the beats converted into seconds, with the proportional part of the daily error added or subtracted, will give the difference of time taken up by the motion of the light and sound: if, lastly, we suppose light to be instantaneous at small distances, the distance of the nearer cloud will be had by multiplying the distance that sound is known to pass through in a second by the number of observed seconds obtained from the beats that were counted.

Many more instances might be here pointed out, in which the beats of a good watch would be extremely serviceable in the practical branches of philosophy; but the occurrence of such instances will always point out the propriety of the application, when it is once known and practised.

I shall therefore only mention one further advantage which seems peculiar to this mode of counting a limited number of seconds by a watch, which is, that it is free from any error that might arise from the graduations of a dial-plate, or unequal divisions in the teeth of wheels and pinions, where the seconds are counted by a hand.

In order to introduce this method of measuring small portions of time accurately, it is desirable that a watch be constructed so as to make an exact number of beats per second without a fraction, for then the reduction of beats into seconds would be more readily made. With a view of promoting this object, I have calculated numbers for a watch, which will produce the desired effect, and which, as they are equally practicable as those in use, I shall insert in this paper, in hopes of hearing at some future period, that they have been adopted by some good workman. By the method of arrangement already given, the numbers proper for such a watch, as will indicate hours, minutes, and seconds, by three hands, and also make just four beats per second, will stand thus, viz.

* I am not aware of any *observable* variation in the time of the earth's rotation. In theory it may vary from the variable disturbing forces of sun and moon, as well as from other causes which may affect its mean diameter. See Philos. Journal, I. 198.—II. 40.—and III. 30.—N.

50 great

50 great wheel
10——60 centre wheel
8——64 third wheel
8——48 contrate wheel
6——15 crown wheel
2 pivots.

Dial-work as usual.

Six spirals on the barrel—to go 30 hours.

By the preceding general rule for ascertaining the beats per second in any watch, the calculation of these numbers will be thus: $60 \times 64 \times 48 \times 15 \times 2 = 5529600$, and $8 \times 8 \times 6 = 384$; then $\frac{5529600}{384} = 14400$, the beats in an hour, and $\frac{14400}{3600} = 4$ exactly, for the beats per second; which agreement with the rule is a proof of the accuracy of the numbers.

Whilst I am upon this subject, I shall take the liberty of cautioning medical gentlemen against an imposition which some, I hope not many, watchmakers practise towards them in the sale of watches; and I the more readily make this caution public, because the health of thousands of individuals is connected with the imposition, which is this, that a second-hand, with a stop, and an appropriate face, are sometimes put to a watch, the wheelwork of which is not calculated to indicate seconds. The watch which is the second mentioned in this paper as being in my own possession, is one of this kind; I bought it of a clock-maker, who had it made in town, with his own name enamelled on the face, but unfortunately I kept it too long before its imperfection was discovered, so that I am now under the necessity of using it. Upon enquiry, I found that more of the same kind have been sold to medical gentlemen and others for the purpose of ascertaining the number of pulsations of invalids in a minute, in order that they may be treated accordingly. At first I suspected that the disagreement in the motions of the second and minute hands, which I observed might be owing to some inequality, or shake, as the workmen call it, in the teeth and spaces of the wheelwork; but upon counting the numbers I afterwards detected the real cause; that part of the train which lies between the axle of the centre wheel and the axle of the contrate wheel, on which the hands are placed, viz. $\frac{60}{8} \times \frac{60}{8}$ is equal to only 56,25 instead of *60, so that $3\frac{3}{4}$ seconds are in defect in every minute, which is equal to a whole revolution of the second-hand in every 16 minutes: hence, if the pulsations of any patient in a fever were really 120 in a minute, the determination by the second-hand of the watch in question would be only $112\frac{1}{2}$, consequently the judgment of the physician or apothecary would be proportionably biassed in drawing a conclusion from the pulse upon the state of the fever, and would undoubtedly prescribe medicines accordingly. It is to be hoped, therefore, that the observations here made upon the construction of a watch may, exclusively of philosophical purposes, prove useful in directing the choice of such gentlemen as may have occasion to purchase a stop-watch, and conse-

* If a wheel of 64 be substituted for either of those of 60 each, the seconds will be truly indicated.—P.

quently

quently may obviate the deception which has hitherto been practised by certain makers and venders of watches.

Lincoln, April 5th, 1799.

II.

Letter from Mr. H. DAVY, introductory to the Experiments contained in the subsequent Article, and on other Subjects relative to the Progress of Science.

SIR,

I SEND you for your Physical Journal, experiments and observations on the flint contained in the epidermis and other parts of certain vegetables: these experiments have been made within the last fortnight. I am induced to make them public so speedily, from the hopes that others, who have greater opportunities and more leisure, may be stimulated to further researches on this interesting subject. I doubt not that other plants will be found to contain silex, as well as the reeds, the canes, and the grasses.

In the Pneumatic Institution we have lately made some experiments on the nitrous phosoxyd (gaseous oxyd of azote), the principle of contagion of Mitchill. When it is mingled with $\frac{1}{3}$ of phosoxygen (oxygen gas), animals live in it without suffering any injury. I have made two inspirations of it pure, without any disagreeable effects. I have breathed it mingled with an equal quantity of phosoxygen (oxygen gas) for some minutes; the effects produced by it were very peculiar: should they be confirmed by future experiments, it will probably prove a valuable medicine.

In my essay on heat, light, &c. in the West-country Contributions, p. 101. ob. 4, an inaccuracy occurs with respect to the production of this gas. It is said "no light is produced "during the decomposition of phosnitrate of ammoniac by heat." In the experiments from which that observation was deduced, no luminous appearance was visible, as the phosnitrate was mingled with a large quantity of siliceous sand, and slowly decomposed by the heat of an Argand lamp. Lately I have made the experiment in the large way: when the heat is quickly applied and the phosnitrate mingled with but a small quantity of sand, a vivid luminous appearance is uniformly perceived. A paper on the nitrous phosoxyd will appear in the next volume of Dr. Beddoes' contributions.

With the hopes of discovering a cheap substitute for nitre, I have lately made the phosmuriates (oxygenated muriates) of strontian and barytes. The properties of the first correspond with the account I have given of them, in the West-country Contributions, art. Combinations of the muriatic Phosacid, except that its solubility is not so great as I at first suspected. The phosmuriate of barytes crystallises in plates, and detonates very slightly with charcoal and phosphorus. A solution of it in water, like that of the phosmuriate of strontian, becomes luminous when the sulphuric acid is poured into it.

I would wish to observe, that no affectation of singularity induces me to use a new nomenclature. Theory or arrangement of facts depends altogether on language, i. e. in applying certain

certain general terms to a number of similar ideas, or similar trains of ideas; consequently there cannot well be a new theory without a new nomenclature. Till my experiments on heat and light are rendered inconclusive, by numerous contradictory facts, or experiments, I consider myself as entitled to use my present nomenclature for the combinations of light.

Dr. Beddoes desires me to inform you, that the Pneumatic Institution, so far as we can yet judge, promises to do well. We were apprehensive that prejudice would prevent applications; but no sooner was its opening notified to the people, than they began to resort to it. We have already upwards of fifty patients. By degrees we shall provide apparatus for trying, in every way, such gases or vapours as promise any thing, or ought to be tried. He requests further, that you would notify that a considerable error has been discovered in the copy from which page 381, line 9, of the "West-country Contributions" was printed: it should be "calomel, three grains," instead of "calomel, eight grains." I remain, Sir, with wishes for the success of your academical undertaking,

Yours, with great respect,
HUMPHRY DAVY.

Clifton, April 11th.

To MR. NICHOLSON.

III.

Experiments and Observations on the Silex composing the Epidermis, or external Bark, and contained in other Parts of certain Vegetables. By HUMPHRY DAVY.

D. 1. A FEW days ago, Mr. Coates, of Clifton, informed me that his son, accidentally rubbing two pieces of bonnet-cane together, in the dark, had perceived a luminous appearance. This phenomenon was sufficiently novel and curious to induce me to examine it. I found that all canes of this kind, when briskly rubbed together, produced sparks of white light. The luminous appearance was much more vivid on collision. When the canes were violently struck together, sparks nearly as vivid as those from the gun-lock were produced. At the same time a strong smell, similar to that generated by the collision of flint, or the excitement of the electric fluid, was perceived.

D. 2. I first thought that the phenomenon was electric, and depended on some resinous matter in the cane. The electrometer, however, was not sensibly affected during the experiment. When the cane was struck against wood of any kind, no light was perceived. When a cane was struck violently against quartz, agate, or any siliceous stone, the light was as brilliant as when two canes were struck together. The luminous appearance was produced when sharp steel was struck against the cane. When the cane was struck against sulphate of strontian, or barytes, or carbonate of lime, no light appeared.

D. 3. These circumstances induced me to suppose that the phenomenon depended on siliceous earth in the epidermis, or in the whole of the cane. To determine this, I took off a small quantity of epidermis from one of the canes. It was hard, white, and had something

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the appearance of pulverized glass. When the epidermis was removed, the canes no longer possessed the property of giving out light on collision.

D. 4. To ascertain, with certainty, the nature of the epidermis, I obtained from 280 grains of cane 22 grains of epidermis; this was exposed, in a crucible, to the strong heat of an air-furnace for half an hour. It had lost three grains, was very white, infusible by the heat of the blow-pipe, and insoluble in any of the mineral acids. Ten grains of it were kept in fusion with caustic potash, in a silver crucible, for a quarter of an hour. The compound was white and semi-pellucid. It was perfectly soluble in water, without communicating to it any turbidity. When muriatic acid was poured into the aqueous solution, a copious white flocculent precipitate was produced. This precipitate collected, weighed about nine grains, and had every property of silex.

D. 5. To determine whether the wood and internal bark of the cane contained any silex, I burnt 240 grains, carefully deprived of the epidermis, for an hour. The ashes were perfectly white, and weighed about seven grains. When muriatic acid was poured upon them, a portion was dissolved with effervescence. This portion was chiefly carbonate of potash; the insoluble part, collected, weighed about two grains, and was apparently silex.

D. 6. Having ascertained, by these experiments, that the epidermis of the bonnet-cane was chiefly composed of flint, and that the luminous appearance above mentioned depended on this composition, I thought it probable that the other canes, particularly the sugar-cane, arundo saccharifera, and the bamboo, or arundo indica *, were similar in their organization. When two bamboos were struck together, I could perceive no luminous appearance. Four ounces of this cane only afforded seven grains of true epidermis. This exposed to a strong heat, left five grains of white matter, which had all the properties of flint. The reason why these canes produce no light, on collision, is, that the flint of the epidermis is too small in quantity, and too thinly diffused. The epidermis of the sugar-cane contained a still smaller proportion of flint: 200 grains of this gave five grains of white ashes, of which only one grain was insoluble in muriatic acid: the four grains of soluble matter appeared to be carbonate of lime. A large piece of bamboo (the weight of which I am ignorant of) deprived of the epidermis, gave a considerable quantity of white ashes, of which about two-thirds were soluble in the muriatic acid: the insoluble part was silex. The ashes of the sugar-cane, deprived of the epidermis, appeared to be chiefly composed of carbonate of lime, and carbonate of potash.

D. 7. The analogy between the English reeds and grasses, and the canes, and particularly the similarity of the appearance of the epidermis, induced me to suppose that they might likewise contain silex. On this supposition I first examined the arundo phragmites, or common reed. It produced no luminous appearance on collision with flint. Twenty-seven grains of the epidermis, exposed to a strong heat, gave 13 grains of white earthy matter, insoluble in

* I am aware that the ingenious Mr. Macie discovered the tabasheer, found in the bamboo, to be chiefly composed of flint. He, however, did not absolutely discover it in the wood of that cane; and no one, to my knowledge, has ever suspected its existence in the epidermis of any vegetable.—D.

the mineral acids. Ten grains of this was fufed with 34 grains of potafh. The compound was foluble in water. The nitrous phofacid threw down from the aqueous folution a white flocculent matter, which was neceffarily flint; this matter I did not weigh, but I conjecture that it was about feven or eight grains. One hundred and ten grains of the reed from whence the epidermis was removed, gave about fix grains of flint.

D. 8. I now examined the culm of wheat: 200 grains, burnt, gave 31 grains of white afhes; of thefe 18 grains were foluble, with effervefcence, in the muriatic acid. The remainder had all the properties of filex. The matter diffolved in the muriatic acid was potafh. The afhes of oats and barley afforded filex in nearly the fame quantities as thofe of wheat. The culms of the graffes, among which I examined anthoxanthum, poa pratenfis, and fome others, appeared to contain more filex in the epidermis, than even the corns with a much larger proportion of carbonate of potafh.

D. 9. The filex in all thefe vegetables, as in the canes, appeared to be contained in the epidermis, or in the fecond bark. When the plants are carefully burnt, the figure of the epidermis is preferved. In the cane, when well burnt, it has a white gloffy appearance, and is femi-tranfparent. In the reeds, corns, and graffes, it is white and opaque, and when viewed through a magnifier, appears to confift of longitudinal threads joined together by net work. In the microfcope, even the fmalleft particles have a diftinct reticular appearance.

D. 10. The quantity of carbonate of potafh in the afhes of the corns and graffes, induced me to fuppofe that, in a ftrong heat, they might be fufed into glafs. The afhes of the arundo phragmites were expofed to the ftrongeft heat of an air-furnace for fome minutes; there was no appearance of fufion; the carbonate of potafh was not fufficient to form glafs with the filiceous earth. The afhes of ftraw, in a ftrong heat, gave a fine white tranfparent glafs, perfectly infoluble in water, and indecompofable by acids. The afhes of hay gave a black glafs, with a fuperabundance of potafh. This converfion of corn and grafs into glafs, may be effected by the blow-pipe, and affords a pleafing experiment. A ftraw burnt with the blow-pipe, and urged with the ftrong heat of the blue flame, beginning at the top, is converted into a fine pellucid globule of glafs almoft fit for microfcopic experiments. A culm of grafs is fufed under the blow-pipe into a globule of glafs, black and opaque, probably from its containing iron.

D. 11. Thefe facts will afford fome curious inferences to the fpeculator on organized nature. The flint entering into the compofition of thefe hollow vegetables, may be confidered as analogous to the bones of animals; it gives to them ftability and form, and by being fituated in the epidermis, more effectually preferves their veffels from external injury. They will probably enable us to determine whether filex be a fimple or a compound fubftance. Reed or wheat might be eafily made to vegetate deprived of filex. Confined under mercury, in a foil compofed of known quantities of the faline earths, and fupplied with diftilled water and factitious air, we might difcover whether it would compofe filex, or fubftitute for it another earth. This experiment, as well as fome others on the fame fubject, we propofe to make, if leifure and opportunity occur. The numerous complex attractions of organic beings, refulting from their

their variety of compofition, are continually producing changes which the art of the chemift is unable to imitate. Though the chemift, at prefent, can extend his power no further by the fimple attractions and repulfive motions of inorganic matter, yet a path of fcience, difplaying a boundlefs field for inveftigation, feems now open in the changes effected in dead matter by living beings. By difcovering thofe changes, we might go far towards difcovering the laws of their organization.

IV.

*Analytical Table of the Refults of the Courfe of Experiments on the lateral Communication of Motion in Fluids. By Citizen J. B. VENTURE, Profeffor of experimental Philofophy at Modena, &c. drawn up by the author himfelf *.*

DESCRIPTION of the apparatus, page 172.—It is proper to miftruft all theories of Hydraulics, not excepting that which is exhibited in the prefent work, excepting fo far as thofe theories may be fupported by experiment, p. 173.

In any fluid, thofe parts which are in motion carry along with them the lateral parts which are at reft. Prop. I. exper. 1. *ibid.*—I call this phenomenon *the lateral communication of motion*; and I confider it as a principle of experiment, or elementary fact, without explaining its caufe, 174.—If water be drawn out of a veffel by an horizontal cylindrical pipe, of which the part neareft the veffel is contracted according to the form of the contracted vein of water which flows through an equal orifice in a thin plate, the expenditure will be increafed by this pipe, in the fame manner as if there had been no contraction. Prop. II. exper. 3 and 4. *ibid.*—The velocity of the ftream, within this tube, is greater than that of the jet through a thin plate, p. 175.—The increafe of expenditure of water through an horizontal cylindrical pipe, whether it be of uniform diameter throughout or contracted at the end next the refervoir, is caufed by the preffure of the atmofphere. Prop. III. exper. 5, 6, 7, *ibid.*—This increafe of expenditure, through a pipe, does not take place in the vacuum of the air pump. Exper. 8. p. 176.

When water is drawn through a defcending cylindrical tube, of which the upper part is of a divergent form, anfwerable to that of the contracted vein, the expenditure will be that which is anfwerable to the height of the charge above the lower orifice of the tube. Rectification of the theory afferted on this fubject by Guillielmini, and adopted by various philofophers.—Experiments. prop. IV. exper. 9, 10, 11, 12. p. 178.—The lateral communication of motion in fluids is the caufe which excites the preffure of the atmofphere to increafe the expenditure and internal velocity in horizontal conical tubes of a certain form. Prop. V. p. 273.—Experiments relative to this augmentation—the refult always falls fhort

* And fubjoined to his treatife.—The paginal numbers refer to the fecond volume of this Journal, except where the Number III. is prefixed, which of courfe implies the prefent volume.

of theory—cause of this defect. Exper. 13, 14, 15. p. 275.—Limits of the augmentation of expenditure in the same horizontal conical tube. Exper. 16, 17, *ibid.*—In horizontal cylindrical tubes, the increase of expenditure does not approach the maximum so nearly as in conical tubes. Prop. VI. exper. 18, p. 423.—Case of the simple cylindrical tube more particularly examined, *ibid.*—The Velocity of the fluid stream issuing through a tube is less than that which flows through an orifice in a thin plate. Cause of this difference. Exper. 19, p. 424.—The same law, and the same causes, also determine the expenditure through ascending and descending tubes. Exper. 20, 21, *ibid.*—The effect of the lateral communication of motion is produced in a very short portion of the length of the inner cavity of the cylindrical tube. Exper. 22. p. 425.—The effect itself is greater than could have arisen from the mutual attraction of the particles of the fluid, *ibid.*—The expenditure which takes place through a cylindrical tube of given dimensions, and under the same charge, may be increased to nearly double by proper adjutages. Prop. VII. *ibid.*—Roman law relative to this object, p. 426.—Application of the same law to the flues of chimnies. *Ibid.*

How far elbows and sinuosities diminish the expenditure through tubes. Exper. 23. p. 487.—Loss of expenditure occasioned by enlargements or inflated parts in tubes. It is necessary to have regard to this in the construction of hydraulic machines. Exper. 24. p. 488.

In the machine for blowing by means of a fall of water, the air is afforded to the furnace by the accelerating force of gravity, and the lateral communication of motion combined together. Prop. VIII. *ibid.*—Wind produced by falls of water in the internal parts of mountains, p. 489.—The wind of the water-blowing machine is not produced by the decomposition of water. Exper. 25. p. 490.—Quantity of wind which one of these machines is capable of affording in a given time. *Ibid.*

It is possible, in certain circumstances, by means of a fall of water, to drain a piece of ground without the help of machines, even though the ground should be on a lower level than the established current below the fall. Prop. IX. p. 491.—Application of the same principle to the tail water of mills. *Ibid.*

The eddies of water in rivers are produced by motion communicated from the more rapid parts of the stream to the lateral parts which are less rapidly moved. Prop. X. p. 492.—Vertical Eddies at the surface and at the bottom of the stream of rivers. *Ibid.*—These circular motions constitute one of the principal causes of the loss of active force, and the retardation of the current of rivers, p. 493.—In a river of which the course is permanent, and the sections of its bed unequal, the water continues more elevated than it would have done if the whole river had been equally contracted to the dimensions of its smallest section. *Ibid.*

Whirling motions, or eddies, formed in a reservoir from which the water issues by an horizontal aperture. Theory deduced from the doctrine of central forces. Prop. XI. vol. III. p. 13. The cavity of these whirls is convex on the side next the axis. III. p. 14.—Phenomena relative to these whirls, and their explanation. Exper. 26, 27, 28, 29, 30. III. p. 15.

The lateral communication of motion takes place in the air as well as in water. Prop. XII. III. p.

III. p. 16.—The excitation of ſound in organ-pipes is effected by this communication. *Ibid.* The ſame cauſe augments the force of ſound in conical divergent pipes. *Ibid.* p. 17.—Remarkable differences between the reſonant vibrations of the air in a pipe, and the pulſations propagated in the atmoſphere. *Ibid.*

The contraction of the fluid vein which iſſues through a thin plate, is not the Newtonian cataract. III. p. 18.—The velocity of the contracted ſtream through a thin plate is nearly the ſame as that of a heavy body which may have fallen through the height of the charge. III. p. 19.—Singular form of the ſtream which is emitted through a long hole or cleft. Exper. 31. III. p. 20.—In right-lined orifices the ſides of the contracted ſtream anſwer to the angles of the orifice, and the contrary. Cauſe of this phenomenon. *Ibid.*—The contraction of the fluid vein is made at a greater diſtance under a ſtrong charge than when the charge is weak. Exper. 32. III. p. 21.—Other varieties in the figure and velocity of the contracted ſtream. Exper. 33 and 34. *Ibid.*—Expenditure through a tube, the inner extremity of which is thruſt into the cavity of the reſervoir itſelf. Exper. 35. *Ibid.*

V.

A Memoir on the Typographic Art, by Alexis Rochon, of the French National Inſtitute, and Director of the Marine Obſervatory at the Port of Breſt.

TYPOGRAPHY is the art of multiplying copies. Amongſt the proceſſes to which the improvement of this art has given riſe, there is one for ever memorable in the records of ſcience and of art. This ſimple deſignation is ſufficient to point out the invention of printing; and the reader will be ready to obſerve, that it is unneceſſary to deſcribe the utility of this happy expedient, the fruitful and inexhauſtible ſource of mental illumination, the ever-durable regiſter of all nations. Men of information well know, that the notions acquired by reading conſtitute the baſis of all our ſcience; and it is to the art of printing that we are moſt indebted for theſe acquiſitions.

Henceforth the precious depoſit of our ſciences and arts is preſerved, and nothing ſhort of univerſal convulſion can deſtroy it. This wonderful art gives to man the faculty of tranſmitting the reſult of his enquiries to his fellow men, however diſperſed over the ſurface of the globe, or even though removed to thoſe periods of duration which ſhall be occupied by our remoteſt poſterity. If this precious invention had been known to the ancients, we ſurely ſhould not have to regret, at this moment, the numerous maſter-pieces of compoſition, and the many uſeful arts which time has for ever covered with obſcurity.

Let thoſe who ſhew an ignorant contempt for the mechanic arts, direct their attention to this, and repent their errors. Let them renounce thoſe unhappy prejudices which tend to diſcourage the numerous and reſpectable claſs of induſtrious artiſts. Much ſagacity has certainly been required to carry our arts to the degree of perfection they at preſent poſſeſs. I will ſay more, and bring the art of printing in ſupport of my aſſertion.—One ſingle happy diſcovery

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is sufficient to change the face of the universe. Let us leave those men who have more erudition than genius to seek the origin of printing in the ages of antiquity. They are ignorant that the great advantage of this art consists still more particularly in casting the characters, than in their mobility; for which reason we join in the opinion of those who affirm John of Gottenburgh, Furst, and Schoeffer, made the discovery in 1439. We must, however, observe, that Schoeffer, the clerk or principal workman of the goldsmith, Furst, was the inventor of moveable types, according to the learned Trithemius, author of the Chronicle Hersanges, who was particularly acquainted with Schoeffer. Trithemius was so well satisfied with this process, that he gave his daughter in marriage to Schoeffer. But whatever may be the decisions respecting this point of history, the immediate object of my memoir will not allow me to enter into any further discussion respecting the degrees of perfection which this art has successively acquired since that memorable epocha. I cannot, however, pass over in silence a method invented in Scotland, by William Ged, to make fixed plates with moveable characters. By means of these plates he printed an edition of Sallust, with the title "*C. Crispi Salustii Catilinarii & Juguthini Historiæ. Edinburgi, Guill. Ged, Auri Faber edinensis non typis mobilibus ut vulgo fieri solet, sed tabellis seu laminis fusis excudebat*, 1744," in 16mo.

The Sallust of Ged is well printed, and resembles, in every respect, a book printed with moveable characters. But Ged reduced his printing characters to a moderate price, and obtained, besides, a much greater saving on the paper, because he printed no greater number of copies, at a time, than he had reasonable ground to expect he should sell. His process is more particularly advantageous for classical books, which are of slow, though steady, sale. These Polytype plates, as they are now called, are likewise capable of affording a considerable profit to the founder, who may make them, for the purpose of exportation or sale, at great distances from the place of fabrication. Hoffman, an industrious artist, communicated, in 1784, to the French Academy, a process similar to that here described; but he applied his art to the composition of a journal, for which it was little calculated. If we could flatter ourselves with making any addition to the fame of the Didots, we should not fail to make honorable mention of their labours. But the object of the present communication is less the art of printing, or impression, than of typography. Typography embraces the generality of the art of multiplying copies, whether the plates present an engraving in relief or excavated, and whether the characters be fixed or moveable. It will not be contended, but that the art of typography was very anciently known in China. Duhalde, and every other writer who has treated of the industry of that nation, informs us, that they transfer to a block, of apple or pear tree, the work of which they are desirous of multiplying copies, by first pasting a written copy, upon thin paper, on the face of the block, through which they trace the marks of the writing with a point, and afterwards clear away the blank parts with the graving-tool. Duhalde observes, that this method of procuring copies has the great inconvenience of requiring an excessive multiplicity of blocks for such works as are voluminous. But how could it be possible for the Chinese to make use of moveable cast types, since they have near one hundred thousand characters. Nevertheless, according to the same author, this people are not ignorant of the

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uſe of moveable characters, becauſe they make them in wood, and uſe them to correct the Account of the State of China, which is printed at Pekin every three months. Duhalde does not here ſeem to be aware of the ineſtimable advantage of caſting types. The very celebrated Dr. Franklin, who was long a printer at Philadelphia, ſhewed me ſome eſſays which he had made for ſpeedily multiplying the copies of his own hand-writing. I do not here ſpeak of thoſe Engliſh preſſes which anſwer the purpoſe of a copyiſt, but of a proceſs which gave birth to that notion, a conſiderable time afterwards. This method conſiſted in writing upon ſmooth paper, with ink containing much gum, which was afterwards ſanded with emery, or powder of caſt-iron, and by means of a rolling-preſs, ſuch as is uſed by the copper-plate-printers, the ſtrokes of the writing were transferred to a plate of roſe-copper, or pewter. This plate affords as many copies as the depth of the engraving can permit; but it muſt be confeſſed, theſe copies are very far from being beautiful, and the ground is ſpotted and ſoiled. Though Franklin did not immediately communicate his proceſs to me, I ſhewed him, before the illuſtrious Turgot, that by writing with a ſteel point on a copper-plate, previouſly varniſhed, a more ſatisfactory reſult might be obtained by etching the ſtrokes with nitric acid to a ſufficient depth for the ſubſequent uſe of a liquid ink ſimilar to that of the printers. In this caſe the plate may be wiped without precaution, and twelve or more copies may be pulled off upon coarſe paper. Theſe proofs are foul and reverſed; whence, in order to have them neat and in the proper direction of the writing, it becomes neceſſary to place the ſame number of leaves of white paper, wetted and prepared, upon the twelve proofs, and, while the ink is ſtill freſh, the whole being paſſed together through the rolling-preſs, the ſame number of counter-proofs are obtained as there were proofs: ſo that inſtead of twelve turns of the preſs, thirteen will be required to afford twelve counter-proofs, very black, neat, and legible, even when the plate has not been perfectly well wiped. This method is certainly not to be compared with fine engraving; but it may be uſeful in military operations, and all other caſes in which a ſpeedy multiplication of copies is required. No precaution is here neceſſary: whether the nitrous acid be more or leſs ſtrong, or remain a longer or ſhorter time upon the plate, or whether the plate be ſomewhat heated to increaſe the ſtrength of the ſolvent, the ſucceſs of the operation will never fail; provided the ſteel point, made uſe of to trace the characters through the varniſh, ſhall lay the copper perfectly bare. It is of advantage that the nitric acid ſhould bite deep, becauſe the counter-proofs are, by this means, much darker. The plate need not be well wiped, becauſe it is of no conſequence whether the proof which is uſed to afford a counter-proof ſhould be very clean, provided it do not ſpot the copy intended to be procured. The moſt liquid kind of printers' ink may be made uſe of.

The art of multiplying copies by a copper-plate engraved by excavation, appears to have been an invention of the fifteenth century. The Italians and Germans contend for the honour of this diſcovery. This art, which is particularly diſtinguiſhed by the name of engraving, is, nevertheleſs, more generally attributed to a goldſmith of Florence, named Thomas Siniguerra. It is more eſpecially uſed to tranſmit to poſterity copies of paintings and deſigns of great maſters. Engravers, or rather copper-plate-printers, have, in certain circumſtances,

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made uſe of counter-proofs; but they are little eſteemed. Theſe counter-proofs have put me in poſſeſſion of a method of procuring, with celerity, a number of copies of a manuſcript of no great extent; and I can affirm that this application of the art has not been without conſiderable utility. Intereſting memoirs, of which the impreſſion had been prohibited, were printed in this manner. The ſame proceſs has alſo been uſed in the publication of flying gazettes. Without attaching any particular importance to a proceſs of ſuch eaſy invention after what was previouſly known, I ſhall only add, that it may be uſefully applied in all caſes where a printing-preſs cannot be had recourſe to. The proceſs here deſcribed may be alſo found in a work which I publiſhed in 1783, on mechanics and natural philoſophy. They are contained in a memoir, entitled, "*Deſcription d'une Machine à graver.*"

It was the illuſtrious Turgot who engaged me, after theſe firſt eſſays, to turn my attention to the mechanical method here deſcribed. The reſtraint under which the liberty of the preſs then laboured, cauſed this great man to wiſh that authors ſhould be able to compoſe and print their works under their own inſpection. The learned Morellet, who became ſtrongly intereſted in this project, gave me ſome punches, which I uſed in my firſt attempts. If I have not entirely accompliſhed the wiſh of Turgot and Morellet, I have, nevertheleſs, produced a machine which has contributed to the perfection of paper-money, and will probably be uſeful, hereafter, in the proceſs which Ged has pointed out for the compoſition of claſſical works. It would, no doubt, be difficult, without the aſſiſtance of a great number of drawings, to ſhew, in detail, the different parts of this machine, which is neceſſarily very complicated, though its action is infinitely ſimple: for when it is uſed, a detent is held by one hand, and the other, by means of a handle, brings the letter which is required under the preſſing tool, which letter, as well as its place, is preciſely indicated by a moveable piece, which paſſes along till it is properly diſpoſed with regard to an index: the detent is then let go, and firmly retains a wheel which carries the letters of hardened ſteel: a ſmall lever is then moved, of which the ſtop is nearer or further off, according to the width of the letter: the lever, while it ſtrikes the ſtop, delivers the preſſing-ſcrew from a tooth which ſtopped its action; ſo that by theſe three operations, any letter whatever is ſpaced and ſtruck with equal preciſion and facility.

Franklin was deſirous of making a compariſon between the time employed with this machine, to engrave a plate of 900 letters, and that required by a ſkilful compoſitor in performing the ſame work by moveable characters. He was very much ſurprized to ſee, that, without practice, the machine engraved the plate in nineteen minutes, without any fault, and his compoſitor employed twenty-three minutes to ſet up the ſame, which ſtill required to be corrected *.

A report of the commiſſaries of the Academy of Sciences, of the 23d December, 1781, will

* Nine hundred characters are contained in about twelve lines of this journal. It ſeems probable, that the machine of the author was worked nearly at the ſame rate as the compoſitor placed his types in the ſtick, but that the latter being neceſſitated to take up his ſpaces, might loſe time on this account. The ſpeedieſt writer with a pen, who has come under my obſervation, would have written the ſame quantity, perfectly legible, in ſix minutes.—*N.*

ſerve

ſerve to give a firſt notion of this machine. "The characters intended to be engraved are diſpoſed in a circle, on a moveable wheel, ſucceſſively brought under a preſſing-ſcrew, which engraves them upon a plate of pewter. The inventor has imagined different means, as well for ſpeedily bringing the deſired character beneath the ſcrew, as to regulate the preſſure in proportion to the extent of the letter itſelf, in order that all the different impreſſions may be made to the ſame depth. The plate itſelf has a double motion, one of which ſerves to ſpace the letters and the words, and the other to regulate the diſtance between the lines. Theſe motions are performed in ſuch a manner, that any diſtance, at pleaſure, may be allowed between the letters, the words, and the lines, and theſe diſtances poſſeſs the moſt perfect equality. The different widths of the letters preſent an obſtacle here: in fact, it may eaſily be underſtood, that in order to render theſe ſpaces equal, it is neceſſary, not that the letters themſelves ſhould occupy equal portions of the line, but that a conſtant interval ſhould be allowed between them. The beauty of printing, likewiſe, demands that theſe ſpaces ſhould be, in ſome inſtances, varied in a ſmall degree, in order that the juſtification ſhould be perfect, that is to ſay, that the ends of the lines ſhould preſent to the eye a regular and ſtraight termination. The machine of our author affords the means of doing this.

"This machine appears to us to unite ſeveral advantages: 1. Engraved editions of books may be executed, by this means, ſuperior to thoſe which can be made by the hand of the engraver, however ſkilful; and theſe engraved originals will be made with much more ſpeed, and much leſs expence. 2. As this machine is portable, and of no conſiderable bulk, it may become very uſeful in armies, fleets, and public-offices, for the impreſſion of orders, inſtructions, &c. 3. It poſſeſſes the advantage which, in a variety of circumſtances, is highly valuable, of being capable of being uſed by any man of intelligence and ſkill, without requiring the aſſiſtance of any profeſſional workman. And, laſtly, it affords the facility of waiting for the entire compoſition and engravings of a work before any of the copies are pulled off; the expence of plates, even for a work of conſiderable magnitude, being an object of little charge; and this liberty it affords to authors, may prove highly beneficial in works of which the chief merit conſiſts in the order, method, and connection of ideas.

"So that even if it ſhould prove, as experience only can ſhew, that it could not acquire the ſame ſpeed as the common method of printing, nor an equal degree of facility for alterations and corrections, it would, nevertheleſs, be of very great utility.

"We think, therefore, that the idea of this machine being new, and the means adopted by the inventor, to give it the degree of perfection to which it is brought, being ſimple and ingenious, and its application being of great utility, the ſame is worthy of the approbation of the Academy; and that the deſcription of the machine, executed with the machine itſelf, and preſented by the author, is worthy of being publiſhed under the privilege of that Academy.

(Signed) "CONDORCET and BOSSUT.

"Given at the Louvre, 22nd December, 1781."

 Deſcription

Description of the Machine for Engraving.

This machine consists of two brass wheels, placed on the same axis above each other, and separated by a number of pillars, each two inches in length (Plate III). These two wheels, with the interval which separates them, are equivalent to a single wheel about three inches thick. In order, therefore, to simplify my description, I shall consider them as a single wheel which moves freely on its axis.

This wheel is perforated near its circumference with a number of square holes, which are the sheaths or sockets through which a like number of steel punches, of the same shape, are inserted, and are capable of moving up and down. They are very well fitted, and from this circumstance, as well as the thickness of the double wheel, they have no shake, or side motion, independent of the motion of the wheel itself. Every punch is urged upwards by a separate spring, in such a manner, that the wheel armed with its characters, or steel types (the lower faces of the punches being cut into the figures of the several letters), may turn freely on its axis; and if it be moved, the several punches will pass in succession beneath an upright screw, for pressure. The screw is fixed in a very firm and solid frame, attached to the supports of the machine; and by this arrangement, a copper-plate, disposed on the table, or bed of the apparatus, will receive the impression of all the punches in succession, as they may be brought beneath the vertical pressing-screw, and subjected to its action.

But as the press is fixed, it would necessarily follow that each successive impression would, in part, destroy, or mutilate, the previous impressions, unless the plate itself were moveable. It therefore becomes necessary that the plate should be moveable in two directions; the first to determine the interval between the letters and words, and form the lines; and the other motion, which is more simple, because its quantity may remain the same through the whole of a book, serves to give the interval between line and line, and to form the pages.

It will easily be conceived, that it would be a tedious operation to seek, upon the circumference of the wheel, each several character, as it might be required to come beneath the press, because it is necessary to repeat this operation as many times as there are characters in a work. I have considerably diminished the time and trouble of this operation, by fixing upon the axis of the great wheel, which carries the punches, another small wheel, about four inches in diameter, the teeth of which act upon a rack, which carries a rule moving between two sliders. This rule, or straight line, will, therefore, represent the developement, or unfolding of the circumference of the wheel which causes it to move, and will shew the position of the great wheel, which carries the punches. For these two wheels being concentric, the developement of the small toothed wheel, of about two inches radius, will exhibit, in a small space (for example, that of a foot), an accurate register of the relative positions of the punches, with regard to the pressing-screw. To obtain this effect, nothing more is necessary than to place a fixed index opposite the moveable rule, which last is divided in the following manner:

The punch on which the first letter of the alphabet is engraved, must be brought under the

centre

centre of the preffing-fcrew; and a line of divifion then drawn upon the moveable rule, to which the letter itfelf muft be added to diftinguifh it. The index, already mentioned, being placed oppofite, and upon this firft divifion, will ferve to place immediately beneath the preff-ing-fcrew, the punch, or rather the character, correfponding with the divifion upon the rule, without its being afterwards neceffary to infpect the place either of the punch or the fcrew, with regard to each other. Confequently, as foon as the divifions which correfpond with all the punches inferted in the wheel, are engraved upon the ftraight rule, the fixed index will immediately determine the pofition into which that wheel muft be brought, in order to place the punches under the preffing-fcrew, in the order which the work may require.

This regifter *(tableau)*, for by this name I fhall hereafter diftinguifh the rule and its index, has no other function in the machine, than to guide the hand of the operator, and to fhew when the punch is very near its proper pofition beneath the preffing-fcrew. When this is the cafe, the required pofition is accurately obtained by means of a detent, or catch.

The detent which I ufe for this operation, is a lever with two tails, one of which is urged toward the circumference of the wheel, by a fpring. To this extremity of the lever is fixed a piece of hardened fteel, which has the figure of a wedge, which, by means of a fpring, is preffed towards the axis of the great wheel, but may be relieved, or drawn back, by preffure on the oppofite tail of the lever, fo as to permit the great wheel to revolve at liberty.

In the next place it muft be explained how this detent takes hold of the wheel, fo as to retain it precifely in the fituation neceffary to caufe any one of the punches, at pleafure, to give its impreffion to the plate. For this purpofe there are a number of notches cut in the circumference of the wheel, for the purpofe of receiving the detent. Thefe notches may be about half an inch deep, wider towards the circumference than elfewhere, and it will be of advantage that this outer width fhould be as great as the circumference of the wheel can conveniently allow. By this contrivance, the wedge will not fail to prefent itfelf oppofite one of the notches into which it will fall, and draw the wheel exactly to its due fituation, even though the index of the regifter fhould not be brought precifely to the line of divifion appropriated to any particular letter. For, if this laft degree of precifion were required in working the machine, it would be very prejudicial to the requifite fpeed which, above all things, is required in its ufe. When the wedge is, therefore, left at liberty, it not only enters immediately into its place, and moves the wheel till its two fides apply fairly to the interior furfaces of the notch, but retains the wheel in this ftate with the neceffary degree of ftability.

The method of giving the proper figure to thefe notches is very eafy. For this purpofe neceffary, in the firft place, to imprefs all the characters contained in the wheel, on a plate of copper or pewter. The fupport on which the plate is fixed muft be moved in a right line, after each ftroke of the punch, through fuch a fpace, that the characters may be arranged one after the other without touching. Now, as the perfect linear arrangement (fuppofing every other part to be true) muft depend on the notches, it might feem fufficient to cut thefe according to the method ufed for the wheels of clock-work: but as it is very difficult to avoid fome obliquity on the face of the punch, and, perhaps, in the hole through which it paffes, it is in almoft every

cafe neceffary to retouch the notch itfelf. The requifite degree of precifion may be eafily obtained, when, upon examining with attention the print of the characters engraved upon the plate, the inequalities fhall have been afcertained by a very fine line paffing exactly under the bafe of two fimilar letters, affumed as objects of comparifon: for the irregularity of linear pofition may, by this means, be determined, with great exactnefs, and remedied to the moft extreme nicety. In this operation, the workman muft file away part of that furface of the notch which is oppofite to the direction of the motion the character requires. Great care muft be taken to file only a fmall portion at a time, in order that the inftant may be feized at which the wedge, by entering into the notch, brings the character to its due fituation.

Thefe details refpecting the right-lined arrangement on the characters, muft not divert our attention from the very great celerity with which any letter is brought to its place under the prefs, by means of the regifter and detent. This celerity is an object of fo much importance in the engraving of a great work, that every means ought to be purfued which may tend to increafe it. For this reafon it is, that inftead of following the alphabetic order in the arrangment of punches on the furface of the wheel, we ought to prefer that in which the fum of the different motions to be given to the wheel, for engraving an entire work, fhall be the leaft poffible. This tedious enquiry may well be difpenfed with, by obferving the order in which printers difpofe their cafes of characters, that the letters of the moft frequent recurrence may be moft immediately under the hand of the workman.

If all the characters afforded an equal refiftance to impreffion in a plate of metal, a conftant force would never fail to drive the punches to the fame depth. But the faces of the letters are very unequal, and, confequently, it will be neceffary to ufe a variable force. Moft workmen ufe the hammer, and not a fcrew, as in this machine, for ftamping. If the hammer had been ufed in this machine, it is evident that if we fuppofed it to have fallen from the fame height upon every one of the punches, the force of the ftroke could be rendered variable according to the nature of the characters, by placing a capital, or head, upon each, of an height properly adjufted to receive the hammer after paffing through a greater or lefs fpace. But the heads of our punches are variable at pleafure, becaufe they are fcrewed on; and thus it is that, by experimentally adjufting the heads of all the punches, a fet of impreffions are obtained, of equal depths from every one of them. When, for example, the letter *i* is placed under the hammer, the upper part of its head is at a fmall diftance from the head of the hammer, in order that its fall, which begins always at the fame place, may ftrike this letter weakly; but when the letter M is brought under the hammer, the upper part of its head being much lefs elevated than that of the letter *i*, will receive a much ftronger blow. The impreffions of the letters M and *i* will, therefore, always be equally deep, if the heads of the punches be once properly fixed by experiment.

Though I have already obferved, that the preffure in this machine is given by means of a fcrew, I fhould certainly have ufed the hammer, if it were not for the inconvenience arifing from

from the tremulous motion it gives, particularly to thofe parts which are hammer-hardened *. The preffure of a fcrew has not the fame inconvenience. Its effect is gradually performed, without occafioning thofe fudden jars fo inimical to precifion and durability of a machine. It neverthelefs happens, in fome inftances, that the impreffion on metals made by a fcrew, do fometimes partake of the circular motion of the fcrew; but this defect may be avoided, by giving its threads a great inclination. The fcrew I make ufe of has eight threads, which are fo inclined that it runs through its female focket, and would fall out merely by its own weight. This conftruction affords the double advantage of preferving the impreffions from the effects of the circular motion, and of affording a fall in the fcrew of nearly nine lines for each revolution. The head of this fcrew is folidly fixed in the centre of a brafs wheel, of which the pofition is horizontal. The diameter of this wheel muft be fufficiently large, that its motion may not be perceptibly affected by the irregularities of friction in the fcrew. This confiderable diameter is alfo requifite, becaufe the preffure of the fcrew depends not only upon the force which is applied, but the diftance of the place of application from the centre of movement.

It is effential that this wheel fhould have very little fhake; for which reafon it is adviſeable that the axis of the fcrew fhould be prolonged above the wheel itfelf, that it may flide in a focket firmly fixed to the frame of the machine. In this fituation, the wheel, which is fixed on the prolongation of the fcrew, will have its plane conftantly preferved in a fituation parallel to itfelf, without any libration, notwithftanding the rife and fall of near nine lines, or three quarters of an inch, which it undergoes for each revolution on its axis.

It has been ftated, as a requifite condition, that the fcrew fhould conftantly fall from the fame fixed point, or elevation, upon the heads of every one of the punches. To accomplifh this effential purpofe, a lever is firmly fixed to the fupport of the fcrew, which lever refembles the beam of a balance, having one of its extremities armed with a claw, and the other ferving to give it motion through a fmall vertical fpace. The claw falls into a notch in the upper furface of the wheel attached to the fcrew, as foon as that wheel has rifen to the defired elevation, and the lever itfelf is fo far limited in its motion, that it cannot take hold of the wheel, excepting when it has reached that height. The wheel, therefore, remains confined and immoveable, by means of this detent, and cannot defcend until it is delivered by preffure upon the oppofite tail of the lever. In my machine, the wheel which has the preffing fcrew for its axis, does not perform an entire revolution. It was with a view that there might never be any fall capable of fhaking and difturbing the machine, that I determined to ufe only two-thirds of a revolution to ftrike thefe punches, which afford the ftrongeft refiftance. The fcrew confequently falls only through fix lines, upon thofe heads which are

* Which alters their figure, and loofens the fittings. The effect of agitation or ftroke, in reftoring hammer-hardened metals to their original ftate, is well known to workmen. Filing and turning of hammer-hardened fteel, in particular, caufes it to regain its figure. Thus, for example, when a circular faw is ground, and flattened by the hammer of the faw maker, it cannot afterwards be rendered thinner and truer in the lathe, becaufe the point of the graver produces a tremulous motion which removes the effect of the hammer.—*N.*

are leaſt elevated, and about two lines upon thoſe which ſtand higheſt. Whence the difference between the extreme heights does not exceed four lines.

It is obvious, that ſo ſmall a difference is not ſufficient to ſtrike all the characters from M to the letter *i*, when the wheel which governs the ſcrew is put in motion by a conſtant weight, of which the impulſe, like that of the hammer, is increaſed only by the acceleration of its fall. It is evident that this requiſite variation of force might be had by changing the weight; but it is equally clear, that the numberleſs and inceſſant changes which the engraving of an entire work would demand, would be incompatible with that degree of ſpeed which forms one of our firſt requiſites. I was, therefore, obliged to render the force of the weight, which turns the ſcrew, variable, by cauſing it to act upon levers of greater or leſs lengths, according to the different quantities of impulſe required by the ſeveral punches. For this purpoſe, I adopted the following conſtruction. I connected by a ſteel chain to the wheel, which moves the ſcrew, another wheel, having its axis horizontal, ſo that the two wheels reſpectively command each other. They are of equal diameter, and the chain is no longer than to make an entire turn round each wheel. This ſecond wheel or leading pulley, is intended to afford the requiſite variations of force, which it does by means of a ſnail fixed upon its axis. The ſnail is acted upon by a cord paſſing over its ſpiral circumference, or groove, and bearing a weight which is only to be changed when a new ſet of punches for characters of a different ſize are put into the great wheel. The ſpiral is ſo formed, that when the weight deſcends only through a ſmall ſpace, the part of the cord, which is unwound, acts at a very ſhort diſtance from the center of the pully; but when the fall is greater, the part of the ſnail upon which it acts is ſo far enlarged as to afford a much longer lever, and, conſequently, to give a proportionally greater effect to the ſtroke. This conſtruction, therefore, by giving the advantage of a longer lever to a greater fall of the ſcrew, affords all the power which the nature of the work and the different ſpaces of the letters demand.

The ſupport on which the plate is fixed, muſt, as has before been remarked, move ſo as to form ſtrait lines. This motion, which ſerves to ſpace the different characters with preciſion, is obtained by means of a ſcrew, the axis of which remains fixed, and carries a female ſcrew or nut. The nut itſelf is attached to the ſupport of the metallic plate, which receives the letters and carries it in the right lined direction without any deviation, becauſe it is confined in a groove formed between two pieces of metal. The ſcrew is moved by a lever which can turn it in one direction only, becauſe it acts by a click upon a ratchet-wheel, which is fixed to the head of the ſcrew. The action of this lever always begins from a fixed ſtop; but the ſpace through which it moves is variable, according to the reſpective breadths of the letters. This new conſideration induced me to fix upon the rule or plate of the regiſter, a number of pins, correſponding with the different diviſions which anſwer to each punch: theſe pins determine the diſtance to which the lever can move. It, therefore, becomes a condition that its poſition in the machine ſhould be oppoſite the fixed index which determines the character at any time beneath the preſſing-ſcrew. The lever and its pin are, therefore,

the

the sole agents employed to space the characters. If the plate were not moved by the lever, the impressions would fall upon each other, and thus, for example, the letter *i* would be totally obliterated by the impression of the letter *l*.

Whenever, therefore, it is required to dispose the letters *i* and *l* beside each other, the plate must be moved after striking the letter *i* through a space equal to the quantity of the desired operation. Suppose this to be one fourth of a line, and that the lever should run through an arc of ten degrees to move the plate through this quantity: as soon as the pin of the letter *l* shall be adjusted to the necessary length to enable the lever to describe an arc of ten degrees, the operation of spacing the two letters *i* and *l* will be reduced to that of placing the last letter beneath the fixed index, and moving the plate till the lever shall be stopped by the pin belonging to the letter *l*. All the other letters will be equally spaced, if the disposition of the punches in the wheel be such, that the last stroke of any letter shall confound itself with any letter of a single stroke, supposing them to be impressed one after the other, without moving the lever between stroke and stroke. This arrangement deserves to be very seriously attended to, because the process could not be performed without it.

Many well informed persons are of opinion, that the perfect equality which this machine for engraving affords in the formation of letters and signs the most difficult to be imitated, may afford a means of remedying the dangers of forgery. It is certain that the performance exhibits a simple and striking character of precision, which is such, that the least experienced eyes might flatter themselves in certain cases to distinguish counterfeits from originals. My unfortunate colleague, Lavoisier, whom the friends of science and the arts will not cease to regret, made some experiments of this kind for the *caisse d'escompte* which were attended with perfect success. Artists appointed for that purpose endeavoured, in vain, to imitate a vignette formed by the successive and equal motion of a character of ornament*. My first machine for engraving was executed by Carrochez, with whom I had very much trouble, from the assiduity with which it was necessary to attend to his operations. I was more fortunate in the construction of the second; a skilful artist, Richer, executed this for a lover of the arts, with a degree of intelligence which was altogether surprising, and he even rendered it more perfect by the addition of a snail. The third machine of this kind, which was intended for the fabrication of assignats, is not finished. It is by the same artist.

While I was employed in the construction and improvement of my machine for engraving, Condorcet engaged me to make use of, in a short memoir of the celebrated Dupati, a method I had imagined to compose a large work with a very small number of types. I complied with his demand, and presented to the academy a polytype plate, entitled *Essai d'imprimerie, présenté à l'académie des sciences le* 8 *Février*, 1786.

The number of moveable cast types which I possessed, was contained in a box of the form of a book, and was sufficient to compose only four lines. When these four lines were

* This argument seems to apply only upon the supposition of the machine itself, being generally unknown to the public.—N.

composed,

composed, I took their impression in fine plaister, mixed with charcoal-dust*; this mould enabled me to cast a number of copies of the four lines which I had composed. I soon perceived that the first and last letter of each line lost its position by sinking further back than the face of the other letters. It was easy for me to obviate this defect, by placing a small metallic support at a certain distance from the beginning and end of each line, to prevent the sinking of the letters. It is scarcely necessary to remark, that the mould ought to be very dry, and that a slight pressure upon the metal while in the half fluid state, is useful to obtain a good plate: seven or eight of these are required to form a page in octavo. I can take upon me to assert, that this process is neither tedious nor embarrassing, and it has the convenience of including the power of making corrections and additions.

This, in short, is the account I have to give of my enquiries into the typographic art. Reth, formerly director of the fabrication of assignats, is better acquainted than any other person with what has since been done for the improvement of this important art. I have but a few words to add. In my essay on ancient and modern coins, I described a method of imitating ancient and modern coins in bell-metal, by the polytypic art. For this purpose I place in a cavity in an anvil the piece of bronze heated to a proper degree, to render it soft: the use of the cavity is to enable the metal to sustain the action of that pressure which it is to receive from the screw or ram at the instant the medallion is applied to it, which is intended to give its hollow impression. If the metal be heated to a proper degree of softness, the original will not be in the least altered, whether it be of gold, silver, or copper; but it is requisite that the piece should not be suffered to become hard by cooling, nor the medallion itself to remain in contact with it a sufficient time to alter its own temperature. The success of this process depends, therefore, on the quickness of the operation, and the practical knowledge of the degree of heat which the object of the impression can bear to soften it without injuring it in other respects: it is necessary to fasten the medal to the face of the screw or ram, in order that it may not touch the heated metal but at the moment of its fall.

Two silver medals struck under the magistrature of Titus Cavisius, Triumvir, Monetarius, which exhibit the instruments used at Rome for coining, suggested to me the notion of the process I have been describing. There are two dies, one superior and the other inferior; the lower die is supported on an anvil, beside which lie a hammer and pair of tongs. The form of the tongs leave no doubt respecting the use to which they were applied; they serve to place the blanks or pieces of metal of the legal weight and fineness between the two dies, and these blanks being sufficiently heated, could, by a single stroke of the hammer, receive both impressions. By this design the ancient art of polytypage is displayed. The two Roman medals which bear for their design, the Dies, the Pincers, the Anvil, and the Hammer, were struck at Rome three hundred years before the Christian era, at the epocha of the war with the Tarentines. Andrew Morell, in his work upon the medals of the Roman Families, expresses himself as follows:

* The material used by the celebrated Tassie, in a similar process of extreme delicacy, was a mixture of sulphate of lime (plaister of Paris) and tripoli. See Philos. Journal, II. 63.—N.

Capus

Caput Junonis monetæ ſalutaris : inſtrumenta monetalia, incus, malleus, forceps, cum vulcani pileo in laurea.

In bello, videlicet, contra Pyrrhum et Tarentinos, quum pecunia deſtituerenter Romani, eos ſcribunt Junoni ad comparandam eandem, vota feciſſe, deam vero monuiſſe illos ut Juſticiæ armis uterentur ; ſic enim pecuniam iis non defecturam. Quod monitum quum ſalutare Romanis fuerit cognomina Monetæ Salutaris inde Juno adopta eſt. Laurea ad victoriam illam olim deæ monitu relatam reſpicere poteſt.

This Memorandum is tranſlated from the J. de Phyſ. IV. N. S. p. 363.

VI.

On the Proceſſes for Manufacturing Hats, the Uſe of Machines, and other Objects. By N. L.

To MR. NICHOLSON.

Newcaſtle, 15th April, 1799.

SIR,

IN addition to the information contained in my former letters on the ſubject of hat-making by engines, I would add the houſe of Meſſrs. Wells and Chatterton of Brenchly, in Kent. From theſe inſtances, it would appear that the queſtion regarding the poſſibility of making hats by engines, is clearly decided in the affirmative. The enquiry now apparently is, how far can thoſe already in uſe be ſimplified ; their principles accurately diſplayed to the public; or their numbers encreaſed ?—For I believe the directions given already are all that are erected.

The knowledge of hat-making neceſſarily includes the practice of bowing, baſoning, felting, pulling (theſe two laſt are included in the term working ; that is, at the plank); ſo that to a hat-maker the remainder of the proceſſes of felting he would have to inveſtigate (whether of animal or vegetable filaments, amongſt which might be included even the combing of wool, the dreſſing of flax, and the fabrication of ſoldiers' belts), would be trifling. For a perſon to have complete ſucceſs in the formation of machines, ſuch as thoſe in queſtion, the fabricator ſhould have a comprehenſive and determined mind, to bear with, and to overcome the impediments which may be thrown in the way by the journeymen ; and an enlarged view of chemical, and eſpecially of mechanical, operations, a ſufficient portion of which, few indeed of the maſters of the preſent day poſſeſs.

Your account of hat-making in your laſt number, ſuggeſted by a viſit to the manufactory of my eſteemed friend Mr. Collinſon, will convince your readers how far from the truth any former accounts are as given to the public in the Encyclopedia, and alſo in the Univerſal Magazine for the year 1750, which are all the places, or nearly ſo, in which it has appeared. They will not wonder at the few improvements regarding the introduction of machines into a buſineſs which has, till very lately, been hidden by the *laboured obſcurity* of prejudiced and ſelf-intereſted manufacturers. Few, indeed, yet think, *that the beſt way of improving a manufacture is to give it publicity.* The progreſs of our cotton and woollen ma-

nufactories to their preſent ſtate, has not probably been impeded by a knowledge of the reſpective buſineſſes being developed. It is not improbable but that the intervention of machines has ſufficiently repaid to the intereſted manufacturer, any loſs attending that which he might ſuppoſe to be an impolitic diſcloſure. We have found, in the courſe of our enquiry, that machines are invented and at work; from their ſmall number, and the ſtrong prejudice exiſting againſt them, even in the minds of the *majority of the maſters*, the undertaking muſt be acknowledged to be only beginning to expand. Is it *impoſſible* to accompliſh it? A few years ago the maſters in Lancaſhire generally ſmiled at the attempt, and the houſe at Lea Wood, in Cromford, was ridiculed by the majority, and pitied by a few. But ſhall ingenuity, Sir, *fabricate* engines for other buſineſſes, and muſt the mere attempt at machines for this appear as an abſurdity? Surely the condemnation of ſuch attempts is, if not futile, at leaſt premature; indeed it appears to me a ſtep gained, and that no inconſiderable one, to be able *publicly* to produce the addreſs of houſes who uſe engines. The contemplation of ſuch facts muſt overthrow the opinion of the impoſſibility of their erection, that many maſters wiſh to ſpread abroad.

The queries you ſuggeſt with regard to the dregs, are anſwered by my men in the following manner: The operation of planking depending, in a great degree, upon the acidity of the liquor, they prefer old dregs to new, becauſe the older they are the ſourer they become.—The ſame reaſon, namely, its acidity, is given by the felt-makers for preferring old urine, as mentioned in a former letter, in the boiling of felt-hats; as to thoſe no * vitriol is uſed in the working. The dregs ought to be thick, or the top (the thin) poured off. If thin or new they ſpoil the body of the hats by hindering their working. I may mention here, that in conſequence of the ſtuff-hat-makers uſing vitriol, they are obliged to have a leaden kettle, whilſt the felt or woolmakers have only a caſt-metal one; though wool hats feel much ſofter when worked in a lead kettle than a caſt-metal one, if worked in each with clean water, (the reaſons for this difference would oblige the author of this). The kettle or bath heated to a leſs degree than that you mention in your memoir, has no power, even though with an additional quantity of lees or vitriol; the hats are generally obſerved to work (ſhrink in) as the kettle "comes too," that is, heats.

Before the operation of planking is begun, the hat is dipped into the boiling kettle, and allowed to lie upon the plank until cold again; this is called ſoaking, that is, being perfectly ſaturated with the hot liquor; if they are put in too haſtily in this ſtate, for they are then only bowed and baſoned, they would burſt from the edges, each bat not being ſufficiently felted into the other. Vitriol alone would harden the hat too much; the dregs keep it mellow and thicken the body, for the vitriol alone eats or purges the ſtuff too much; the hats feed as it were upon the dregs †. The journeymen tell me that the dregs are to hold or

* The term *vitriol* appears to deſignate the ſulphuric acid *(oil of vitriol)* in manufactories; and not the ſulphate of iron or copper.—N.

† There is a degree of obſcurity in the deſcription, as well as the rationale of what happens here. Might it be conjectured, that the ſulphuric acid by diſſolving or diſengaging a natural mucilage from the face of the hair,

or fill the body, whilst a little vitriol cleanses it of the dirt, &c. that may be on the rabbit or other wools; too much vitriol would make the whole that was weighed out to the journeymen work into the hats, but by the mutual action of the vitriol and the dregs, the quantity of the first being small, about a small wine glass full, the dirt and the strong hairs* get purged out, whilst at the same time the dregs keep the hat plump. After the body is got up to a certain size, the workmen put on gloves made of the sole of a shoe, to shield their hands from the vitriol, to enable them to work the hat tighter, and to bear hotter water. The kettle is usually weakened before blocking the hats, lest the vitriol should eat out the dregs. There is only one cloth used at the hurdle, which with paper is generally thought sufficient, and the cloth from its being unbleached and soiled in the wear, might naturally enough convey the idea of being dyed brown. The bow is best made of ash, that it may be heavy and steady in working: it is composed of the stang † or handle made of the above wood: the bridge at the smaller end, or that nearest the window, when held in the hand in the act of bowing, is called *the cock*, and that at the other end, which from its being hung to balance, and being thicker, is the nearer to the workman's hand, is called the breech. After bowing, and previous to the basoning, a *hardening skin*, that is, a large piece of skin, about four feet long and three feet broad, of leather alumed or half tanned, is pressed upon the bat, to bring it by an easier gradation to a compact appearance, after which it is basoned, being still kept upon the hurdle. This operation, the basoning, derives its name from the process or *mode of working*, being the same as that practised upon a wool hat after bowing, the last being done upon a piece of cast metal, four feet across, of a circular shape, called a bason: the joining of each bat is made good here by shuffling the hand, that is, by rubbing the edges of each bat folded over the other to excite the progressive motion of each of the filaments in felting, and to join the two together. Many journeymen, to hurry this work, use a quantity of vitriol, and then, to make the nap rise and flow, they kill the vitriol, and open the body again by throwing in a handful or two of oatmeal; by this means, they get a great many made, though, at the same time, they leave them quite grainy ‡ from the want of labour. This, in handling the dry grey hat when made, may be in part discovered, but in part only.

Another advantage attending the use of dregs, whether of beer, porter, or wine, is, that as the boiling in the dying does not draw out much of the mucilage from each hat, when they

hair, may render the action of felting too speedy, close, and irregular, so as not to allow time either for purging or repairing; and that the mucilage of the dregs prevents this, and has the advantage of being afterwards washed out to any required degree towards the end of the process?—N.

* Because they are straiter?—Philos. Journal, I. 401. note.—N.

† An old north-country word, signifying a pole or staff.—N.

‡ The cause, as I presume, of hats wearing into shining or greasy spots. This was a principal defect of the cloth made under Booth's patent, in which the staple of the raw material, namely, cotton, flax, or wool, was wrought together, by an engine, with little or no dependance on the felting property of the fibre itself.—N.

come to be ftiffened, the dregs form a body within the hat, fufficiently ftrong or retentive to keep the glue from coming through amongft the nap: vitriol alone purges or weakens the goods too much, confequently half of the quantity does better with the addition of dregs, and they alfo allow the body to be clofer from its getting mere work.

Let us now examine the opportunities that journeymen have to fteal from their mafters; and probably this review will fuggeft ftill ftronger arguments, for the more general introduction of machines, than has been advanced in a former number. For we fhall find that the goodnefs of the bodies, as well as the quantity of beaver weighed out for the napping or covering, which you call the facing, may both be greatly impoverifhed, and the mafter, from the difhonefty of the men, nearly ruined before he is aware: from the mafter who weighs to the maker, and from him after he has got his hat ready for the dreffing, or raifing the nap, the beaver may be eafily ftolen, and yet remain unknown to the mafter, by the hats coming in damped, though imperceptible to the hand, and of courfe weighing heavier. Or it may be faid, by the journeymen, that the hats were wider laid in the bafoning, and had purged more at plank, and of courfe loft weight. Nay, the very dregs in which it is feen the hats are worked, may increafe the weight, and even beating them when dry, does not free them *entirely* from the dried mucilage. The colour of the covering, or beaver, is no infallible guide, as the journeyman who cuts it, often puts inferior beaver into the middle of a hood, lock, or ball of beaver, and fecretes a part of the beft, which they, as well as the journeymen hat-makers, fell to the inferior mafters at a low price; detection in either cafe being almoft impoffible. The journeyman hatter knows this, and can fay the beaver was light coloured; though that article might be very good, and the covering appear light from the hare wool or inferior beaver put in by the journeyman hat-maker, after having ftolen an equal weight of good beaver (the value of beft hare's wool being about 16s. per lb. and beaver from 30s. up to 60s. per lb.), efpecially if backs of hare's wool be weighed into the body. It is from thefe depredations that the ufe of machines appears fo urgent: the fame inconveniencies attend the mafter furrier or fkinner; he does not examine each lock or ball of beaver minutely, for his time will not allow him, and any mixture of inferior beaver (which is very eafily obtained and effected), worth, as above, about 30s. with the beft at about 60s. and a part of that beft ftolen, will within a very little time, arrive to a large amount. Thefe, and many fuch, will demand, I had almoft faid, imperioufly, the attention of the ingenious mechanic, whilft the certain advantage from the above ftatement, will not be loft fight of by the fabricator, in examining the arguments for and againft the introduction of machines into each bufinefs. To the moralift, who views the duplicity, the crimes and their parent ignorance of his fellows, the recollection of the depravity fo deeply impreffed upon each circle of journeymen—from the eafe with which thefe thefts may be conducted, from the great wages they receive, many in London more than 30s. per week, and from their confirmed habits of debauchery, confequent upon both thefe—motives fufficiently ftrong will prefent themfelves, to accelerate an enquiry, *at leaft*, into the impediments, or the advantages, attending the introduction of engines, if they were only for the bowing, hardening, and bafoning, for then the journeymen would be totally

hindered

hindered from ftealing; or if thofe few already in ufe were more general, and probably more fimplified, the purpofe might be anfwered. Let it then be the practice of the well-wifhers to virtue, to diminifh, or remove as far as poffible, the impediments, whether arifing from moral or other caufes, and vice will prefent to them too ftriking a front, not to induce them to hold out the advantages in an unornamented, though decifive manner. With regard to flax, the object of the journeymen in that bufinefs, and in that of the wool-combers, being the fame, that is, to lay each filament in a ftraight line in certain proportions, an engine which would accomplifh the one, muft with a trifling alteration effect the other. I find in the General Evening Poft for the 14th March, 1799, that there is a manufactory eftablifhing in the neighbourhood of Prefcot, in Lancafhire, for the purpofe of *combing wool* and *fpinning flax*—How far might a hint be taken from this to accomplifh the former?

Your's &c.

N. L.

VII.

Account of a Subftance found in a Clay-pit, and of the Effect of the Mere of Difs upon various Subftances immerfed in it. By Mr. BENJAMIN WISEMAN, *of Difs in Norfolk. With an Analyfis of the Water of the faid Mere. By* CHARLES HATCHETT, *Efq. F.R.S. In a Letter to the Right Honourable Sir* JOSEPH BANKS, *Bart. K. B. P.R.S. &c.**

THE fubftance I have inclofed was found near Difs, in a body of clay, from five to eight feet below the furface of the foil. All the pieces I obferved laid nearly in a horizontal direction; and varied in fize, from two or three ounces, to as many pounds. The colour of the fubftance, when taken frefh from the clay-pit, was like that of chocolate; it cuts eafily, and has the ftriated appearance of rotten wood. The pieces were of no particular form; in general, they were broad and flat, but I do not recollect to have met with a piece that was more than two inches in thicknefs: it breaks into laminæ, between which are the remains of various kinds of fhells. The fpecific gravity of this fubftance, dried in the fhade, is 1.588; it burns freely, giving out a great quantity of fmoke, with a ftrong fulphureous fmell.

By a chemical analyfis, which I cannot confider as very accurate, one hundred grains appear to contain,

	grains.
Of inflammable matter, including the fmall quantity of water contained in the fubftance	41.3
Of mild calcareous earth	20.0
Of iron	2.0
Of earth that appears to be filex	36.7
	100

* Philof. Tranf. 1798. page 567.

On

On the Effect of the Mere of Difs upon various Subftances.

Obferving feveral years ago, that flint ftones taken out of the Mere of Difs were incrufted with a metallic ftain, I was induced to make fome experiments, in order to difcover the nature or compofition of this metallic fubftance. Nitrous acid readily removes it, diffolving a part, and leaving a yellowifh powder, which, wafhed and filtered, was found to be fulphur. Vegetable fixed alkali precipitated from the nitrous acid a ferruginous coloured powder, which was iron.

With a view to determine what length of time was neceffary for the formation of this metallic ftain upon flint ftones, or other fubftances, I inclofed in a brafs-wire net the following articles: flint ftones, calcareous fpar, common writing flate, a piece of common white ftone ware, and likewife a piece of black Wedgwood-pottery. After remaining in the water from the fummer of 1792, to Auguft, 1795, the flints and Wedgwood-ware had acquired the metallic ftain in a flight degree, and the flate had affumed a ruft colour; the other fubftances appeared not to be at all altered. I was greatly furprifed to find the copper wire that held the net, furrounded with a metallic coating of a confiderable thicknefs; it was of a deep lead colour, and of a granulated texture. When taken from the wire, and ground in a mortar, it had a black appearance, interfperfed with very hard fhining particles. The wire was evidently eroded, and this fubftance depofited in the place of the copper that was decompofed, fomewhat fimilar to the decompofition of iron in cupreous waters. By repeated chemical analyfis of this fubftance, one hundred grains contain, of copper, 70; of fulphur, 16.6; of iron, 13.3 grains.

I have never met with an account of the decompofition of copper, in waters impregnated with iron, in any chemical work; and, as iron appears to have a greater affinity to the vitriolic acid than copper has (as is conftantly evinced in the neighbourhood of copper mines), it appears an anomaly in chemiftry, that I am not adept enough in the fcience to account for.

[The Prefident and Council of the Royal Society thinking the effects of the water of Difs Mere deferving of further inquiry, defired Mr. Wifeman would fend fome of the faid water, for the purpofe of examination. Mr. Wifeman accordingly fent a quantity of the water, accompanied by the other fubftances defcribed in the following letter to the Prefident.]

SIR, Difs, May 29, 1798.

As the Society have expreffed a wifh, through Mr. Frere, to have fome of the water in which the copper wire was depofited, which Mr. Frere, at my requeft, laid before the Society, I have fent two gallons of the water of Difs Mere (No. 1), with a fmall quantity of copper cuttings (No. 2), which laid in the fame water, a few feet from the fide, and fix feet in depth, from the 7th of February, 1797, to the 20th of the prefent month, May, 1798. The pieces of copper, when laid in, weighed 3051 grains; when they were taken out, and wafhed from the mud that lightly adhered to them, preferving and weighing the fcaly matter that came off, they weighed 2944 grains, indicating a lofs of 107 grains. Examining the pieces of

of copper, the ſame evening they were taken out of the water, I obſerved a number of ſmall cryſtals formed upon ſome of them, in the form of pyramids joined at their baſes; theſe cryſtals loſt their ſhining appearance, by the evaporation of the water of cryſtallization, in the warmth of the ſucceeding day. Whether they will be preſerved in a journey of nearly 100 miles, is perhaps doubtful. No. 3. contains two pieces of copper, on which the cryſtals were moſt abundant. No. 4. contains a ſmall quantity of the ſubſtance formed upon the copper, that came off in waſhing and in weighing it.

The town of Diſs is principally ſituated on the NNE. and E. ſides of this piece of water. The land runs pretty ſteep on the W. and N. of it, to the height of 40 to 50 feet: on the SE. the ground comes within a few feet of the level of it. The ſoil of the upper part of the town is a ſtiff blue clay; that of the lower part, to the SE. a black ſand, beneath which it is a moor. The water in the higher parts of the town is good; in the lower parts, it is a chalybeate, of which a ſpecimen is ſent (No. 5.)

No. 6. contains a quantity of flint ſtones, taken from the SE. ſide of the Mere, where the water is ſhallow; many of which are ſtrongly marked with the metallic ſtain, which they acquire by lying in this water a few years.

The Mere contains about eight acres, and is of various depths, to twenty-four feet: from its ſituation with reſpect to the town, it may naturally be ſuppoſed to contain a vaſt quantity of mud, as it has received the ſilt of the ſtreets for ages. In ſummer the water turns green; and the vegetable matter that ſwins on its ſurface, when expoſed to the rays of the ſun, affords vaſt quantities of oxygen gas. I cannot help conſidering this proceſs as having a conſiderable agency in the corroſion, and in the formation of the metallic cruſt upon the copper depoſited in this water. Some of this vegetable matter will be found in the water ſent to the Society.

I intend to make ſome further experiments with different metallic ſubſtances, at different parts, and at various depths; but, as the proceſs is ſlow, if in the mean time you, Sir, or any of the members of the Society, will have the goodneſs to point out any experiment you or they may wiſh to have made, I ſhall be very glad to contribute all in my power towards the illuſtration of the ſubject.

I have the honour to be, &c.

BENJ. WISEMAN.

The Right Hon. Sir JOSEPH BANKS, Bart.
K.B. P.R.S.

(The

[The water, and other ſubſtances deſcribed in the foregoing letter, were delivered to Mr. Hatchett, who had been previouſly requeſted, by the Preſident and Council, to examine them. The reſult of his examination is related in the following letter to the Preſident.]

Analyſis of the Water of the Mere of Diſs, by CHARLES HATCHETT, Eſq.

DEAR SIR, Hammerſmith, September 14th, 1798.

In conſequence of the requeſt which you and the Council of the Royal Society have done me the honour to make, that I would examine the water of Diſs Mere, and the other ſubſtances ſent by Mr. Wiſeman, I now haſten to acquaint you with the reſult of my experiments.

The ſubſtances ſent by Mr. Wiſeman are as follows: Some copper-wire, with a blackiſh-grey incruſtation. Water from Diſs Mere (marked No. 1.) Copper-cuttings, covered with a blackiſh cruſt, ſimilar to that on the copper-wire (marked No. 2.) Some cuttings ſimilar to thoſe above mentioned (marked No. 3.) The paper, No. 4, contained ſome of the black cruſt, detached from the cuttings. No. 5, a quart bottle, containing ſome water from the lower part of the town of Diſs, and called, by Mr. Wiſeman, a chalybeate water. No. 6, ſome flints, taken from the SE. ſide of the Mere, where the water is ſhallow, and having, as Mr. Wiſeman terms it, a metallic ſtain.

My firſt experiments were made on the incruſtation of the copper-wire, mentioned in Mr. Wiſeman's firſt letter. This incruſtation was eaſily detached from the wire, and being reduced to powder, was digeſted with nitro-muriatic acid, in a gentle heat: a green ſolution was formed, and there remained a reſiduum, of a pale yellow, which proved to be ſulphur.

The ſolution being diluted with two parts of diſtilled water, was ſuperſaturated with pure ammoniac, by which a few brown flocculi of iron were precipitated. The ſupernatant liquor was blue; and, being evaporated, and re-diſſolved by ſulphuric acid, the whole was precipitated by a plate of poliſhed iron, in the ſtate of metallic copper. The component parts of this coating were, therefore, copper, and a very ſmall portion of iron combined with ſulphur. I could not extend theſe experiments, as the whole quantity of the coating that I was able to collect, amounted only to three grains and a half*.

The next experiments were made on the black cruſt of Nos. 2, 3, and 4. This I found to be exactly the ſame as that formed on the copper-wire, viz, it conſiſted of copper combined with ſulphur, and a very ſmall portion of iron.

* The copper-wire, when the coating was removed, was perfectly flexible, and the ſurface did not appear unequal or corroded: this is commonly the caſe under ſuch circumſtances; for, when ſulphur has combined ſuperficially with a metal, the compound is obſerved to ſeparate eaſily, ſo as to leave the metal underneath not injured in quality, and very little, if at all, affected in appearance. Thoſe who diminiſh ſilver coin, make uſe of the following method: They expoſe the coin to the fumes of burning ſulphur, by which a black cruſt of ſulphurated ſilver is ſoon formed, which, by a ſlight but quick blow, comes off like a ſcale, leaving the coin ſo little affected, that the operation may ſometimes be repeated twice or thrice, without much hazard of detection, if the coin has a bold impreſſion.

I next

I next examined the water of Diss Mere (No. 1.), and I was at length led on, step by step, to make a regular analysis of the fixed ingredients. Before I made the analysis, I examined this water with certain re-agents, and remarked the following properties. 1. The water of Diss Mere has a yellowish tinge, and the flavour is rather saline; but it has not any perceptible odour. 2. Prussiate of potash did not produce any effect. 3. Acetite of lead produced a slight white precipitate. 4. Nitrate of silver formed one very copious. 5. Tincture of galls had not any effect. 6. Muriate of barytes caused a slight precipitate. 7. Ammoniac, potash, and oxalic acid, severally produced precipitates, when added to different portions of this water.

Analysis.

A. Three hundred cubic inches of the water, by a gentle evaporation, left a pale brown scaly substance, which weighed 58 grains. B. These 58 grains were digested in alcohol, without heat, during 24 hours, and afforded a solution, which, by evaporation, yielded muriate of lime, slightly tinged by marshy extract, 18 grains. C. Six ounces of distilled water were then poured on the residuum, and, with repeated stirring, remained during 24 hours. By evaporation, this afforded muriate of soda, with a very small portion of sulphate of soda; in all, 10 grains. D. What remained was boiled in 800 parts of distilled water, and the solution being evaporated, left of selenite 1.70 grains. E. The undissolved portion now weighed 25 grains, and was digested with diluted muriatic acid: a great part was dissolved, with much effervescence, and, being filtrated, afforded, by ammoniac, of alumine 1.50 gr. From this I afterwards separated a very minute quantity of iron, by means of prussiate of potash. F. Carbonate of soda was then added to the liquor, and precipitated carbonate of lime 20 grains. G. The insoluble residuum weighed 3.50 gr.; and proved to be principally carbon (produced by decomposed vegetable matter), with a very small quantity of siliceous earth. The result of this analysis was, therefore,

B. Muriate of lime - - - - - -	18
C. Muriate of soda, with a very small portion of sulphate of soda	10
D. Selenite - - - - - - -	1 70
E. Alumine, with a portion of iron too small to be estimated -	1 50
F. Carbonate of lime - - - - - -	20
G. Carbon, with a little siliceous earth - - -	3 50
	54 70
Loss	3 30
	58 0

It is worthy of notice, that the iron present was in so very small a quantity as not to be detected by any test, till it had been separated in conjunction with the alumine.

The water, No. 5, from Mr. Wiseman's account, does not appear to have been concerned in

in producing the effects which he has obferved, and the quantity was too fmall to be fubjected to a regular analyfis; I noted, however, what follows:

1. It has a very ftrong hepatic flavour and fmell. 2. A plate of polifhed filver, put into it, became black in a few hours. 3. It became faintly bluifh with pruffiate of potafh, after ftanding five or fix hours. 4. Tincture of galls produced a faint purple cloud. 5. Solution of acetite of lead afforded a brown precipitate. 6. Nitrate of filver produced the fame. 7. Potafh, and ammoniac caufed a precipitate; but that of the former was the moft copious. 8. Oxalic acid produced a precipitate. 9. Muriate of barytes had alfo a flight effect.

The water, No. 5, cannot, therefore, be confidered as a chalybeate (the quantity of iron contained in it being fcarcely perceptible); but it appears to be a water containing fome hepatic gas, together with fubftances fimilar to thofe contained in No. 1. From the above experiments it is evident, that the water, No. 1, does not contain any of the component parts of the cruft formed on the copper-wire and cuttings, although it is certain that the incruftation took place during the immerfion of thofe bodies; but, before I mention my ideas on this fubject, I fhall give an account of fome experiments made on the flints, No. 6. Thefe were coated with yellowifh fhining fubftance, which appeared to me to be pyrites; and, as the flints could not have contributed any metallic fubftance to form this coating, I was enabled by their means to afcertain, whether the copper of the cruft, formed on the wire and cuttings, had been furnifhed by the pieces of copper, or by any thing in the vicinity of the water.

1. I poured nitro-muriatic acid on fome of the flints, in a matrafs, fo as completely to cover them. The coating was rapidly diffolved, with much effervefcence; and, when the flints appeared perfectly uncoated, and in their ufual ftate, I decanted the liquor.

2. A yellow matter fubfided, which proved to be fulphur.

3. Pruffiate of potafh produced Pruffian blue; and the remaining part of the folution, being fuperfaturated with ammoniac, afforded an ochraceous precipitate of iron. The fupernatant liquor did not become blue, as when copper is prefent, nor was the fmalleft trace of it afforded by evaporation.

Martial pyrites is, therefore, the only fubftance depofited on bodies immerfed in the water of Difs Mere; and the copper of the cruft, formed on the wire and cuttings, was furnifhed by thofe bodies.

It is proved by the analyfis, that the water of Difs Mere does not hold, in folution, any fulphur, and fcarcely any iron; it has not, therefore, been concerned in forming the pyrites; but it appears to me, that the pyritical matter is formed in the mud and filth of the Mere; for Mr. Wifeman fays in his letter, that "the Mere has received the filt of the ftreet for ages." Now it is a well-known fact, that fulphur is continually formed, or rather liberated, from putrefying animal and vegetable matter, in common-fewers, public ditches, houfes of office, &c. &c.; and this moft probably has been the cafe at Difs. Moreover, if fulphur thus formed fhould meet with filver, copper, or iron, it will combine with them, unlefs the latter fhould be previoufly oxydated. The fulphur has, therefore, in the prefent cafe, met with iron, in, or approaching, the metallic ftate, and has formed pyrites; which (whilft in a minutely

a minutely divided ſtate, or progreſſively during formation) has been depoſited on bodies, ſuch as the flints when in contact with the mud. But an exceſs of ſulphur appears to be preſent; for, when copper is put into the Mere, the ſulphur readily combines with it; and, at the ſame time, a ſmall portion of iron appears to unite with the compound of copper and ſulphur, poſſibly by the mere mechanical act of precipitation.

The incruſtation on the copper wire and cuttings is, in every property, ſimilar to that rare ſpecies of copper ore, called by the Germans *kupfer ſchwärtze (cuprum ochraceum nigrum)*; and I conſider it as abſolutely the ſame. In reſpect to the martial pyrites on the flints, there can be no heſitation; and, as in theſe two inſtances, there were evident proofs of the recent formation of ores in the humid way, I was deſirous to aſcertain the effect on ſilver. I therefore wrote to Mr. Wiſeman, to requeſt that he would take the trouble to make the experiment; and received from him the following anſwer, accompanied by the ſpecimens.

"Sir, "Diſs, 8th of September, 1798.

"Immediately upon the receipt of your letter (27th July), I laid ſome ſilver plate, and ſilver wire, into the Mere; the whole weighed 235.6 gr. I took it out on Thurſday laſt (September 6th), and, after cleaning it carefully from mud and weeds, I find it weighs 242.8 gr.; an increaſe of 7.2 gr. The ſilver plate you will find much tarniſhed, in ſome parts almoſt black; the wire is in many places fairly incruſted, which cruſt, upon the preſſure of the fingers, comes off in thin ſcales. The whole appearance of the ſilver ſtrongly indicates the preſence of ſulphur, which I have no doubt abounds in every part of the Mere. The peculiar ſmell of the mud gives me reaſon to ſuppoſe, that a great deal of hepatic air is produced; which, probably, uniting with the iron held in ſolution in the water of the Mere, may account for the martial pyrites found on the flints. By what affinity the copper wire, laid in this water, is attacked, I am not chemiſt enough to determine.

"I have begun a ſet of experiments, with the view of producing the ſame effects upon copper wire by artificial means; but whether I ſhall ſucceed, I am not at able preſent to ſay.

"I am, &c.

"BENJ. WISEMAN."

P. S. By experiments I have lately made, I find hepatic gas precipitates carbonate of iron in the form of a black flocculent matter; 71 parts of which are iron, and 29 ſulphur.

The ſilver plate I found (as Mr. Wiſeman has mentioned) much tarniſhed, and in many places almoſt black, but I could not detach any part of it. I ſucceeded better with the wire, and collected a ſmall portion of a black ſcaly ſubſtance, which, as far as the ſmallneſs of the quantity would allow it to be aſcertained, was ſulphuret of ſilver; and was ſimilar, in every reſpect, to the ſulphurated or vitreous ore of ſilver, called by the Germans *glaſertz*. This effect on the ſilver was to be expected; and I recollect to have read, not many months ago, in one of the foreign journals, that Mr. Prouſt had examined an incruſtation, of a

dark grey colour, formed in the course of a very long time, on some silver images, in a church at (I believe) Seville. This incrustation he found to be a compound of silver with sulphur, or, in other words, vitreous silver ore. The same principle is the cause of the tarnish which silver plate contracts with so much ease, particularly in great cities; for this tarnish is principally a commencement of mineralization on the surface, produced by the sulphureous and hepatic vapours dispersed throughout the atmosphere, in such places.

To Mr. Wiseman's observations we are much indebted, as they make known the recent and daily formation of martial pyrites, and other ores, under certain circumstances. It is not to be supposed that such effects are local, or peculiar to Diss Mere; on the contrary, there is reason to believe that similar effects, on a larger scale, have been, and are now, daily produced in many places. The pyrites in coal mines have, probably, in great measure thus originated. The pyritical wood also may thus have been produced; and, by the subsequent loss of sulphur, and oxydation of the iron, this pyritical wood appears to have formed the wood-like iron ore which is found in many parts, and particularly in the mines on the river Jenisei, in Siberia. In short, when the extensive influence of pyrites in the mineral kingdom, caused by the numerous modifications of it, in the way of composition and decomposition, is considered, every thing which reflects light on its formation becomes interesting; and I cannot but regard as such, the effects which Mr. Wiseman has observed in the Mere of Diss.

With great respect, I remain, &c.

CHARLES HATCHETT.

The Right Hon. Sir JOSEPH BANKS, Bart.
K.B. P.R.S. &c.

VIII.

Abstract of Experiments and Observations on the internal Use of Phosphorus. By ALPHONSUS LEROI, *Professor at the School of Medicine in Paris*.*

1. THE internal administration of phosphorus in such diseases as exhaust the vital powers, appears to give a certain degree of activity to the energy of life, and afford spirits to the patient without raising the pulse in the same proportion. The author relates several cases in his own practice. Among others is the following. He was called to a woman at the point of death, who was exhausted with weakness after three years' sickness. He yielded to the pressing intreaties of her husband, who earnestly desired that he would prescribe something. He composed a draught consisting of syrup diluted with water, which had remained upon the sticks of phosphorus. She was much better the following day, and continued to recover for several days afterwards. She died about sixteen days subsequent to this prescription.

* From the bulletin of the Société Philomathique, copied into the Journal de Physique, IV. N. S. 402.

2. He

2. He himfelf had, as he expreffes it, the imprudence to take two or three grains of folid phofphorus, fimply united with theriaca. The confequences were dreadfully alarming. His firft fenfation was that of a burning heat in the region of the ftomach, which organ appeared to him to be filled with gas, and even emitted elaftic fluid by the mouth. In this fhocking ftate of torment, he endeavoured but in vain, to caufe himfelf to vomit, and found no relief but by drinking cold water from time to time. His pains, were, at length affuaged; but the following day an aftonifhing mufcular force was developed through the whole habit, with an almoft irrefiftible difpofition to exert that force. The effect of the medicine at length terminated after a violent priapifm.

3. In many circumftances the author has employed, or continues to prefcribe, phofphorus internally, with the greateft fuccefs, to reftore and eftablifh the forces of young perfons exhaufted by too frequent fenfual indulgence. He defcribes the procefs by which he divides the phofphorus into very fmall particles. He agitates the phofphorus in a bottle filled with boiling water, by which means it becomes divided into globules; and by afterwards continuing to agitate the bottle beneath cold water, he obtains a kind of precipitate or very fine powder of phofphorus, which he levigates gently with a fmall quantity of oil and fugar, and dilutes the whole in the yolk of an egg, to be ufed as a lohoch. By the help of this medicine he has performed cures, remarkable for the fpeedy reftoration of ftrength obtained by his patients.

4. In malignant fevers, the internal ufe of phofphorus, to ftop the progrefs of gangrene, fucceeded beyond all hope. The author relates feveral inftances.

5. Pelletier related to him, that having neglected a portion of phofphorus in a copper bafon, the metal was oxyded, and remained fufpended in the water; that having, by accident, thrown this water into a fmall court where ducks were kept, thofe birds drank of it, and every one died; but the male covered the females to the laft inftant of his life: an obfervation which agrees with the fact of the priapifm which the author experienced.

6. The author relates a fact, which fhews the aftonifhing divifibility of phofphorus. Having ufed, in the treatment of a patient, certain pills, into the compofition of which a quantity of phofphorus, amounting at moft to one-fourth part of a grain, entered; he found upon the occafion of opening the body, that all the internal parts were luminous, and even the hands of the operator, though wafhed and well dried, preferved the phofphoric light for a confiderable time.

7. The phofphoric acid ufed as a lemonade, proved very advantageous in the cure of a great number of diforders.

8. Leroi affirms, that having oxyded iron with phofphorus, he obtained a white oxyd, fcarcely reducible by the ordinary methods, which he thinks may be advantageoufly fubftituted inftead of white lead, in the arts, particularly in oil and enamel paintings. This white oxyd of iron produced a very ftrong naufea in the author, who ventured to place a particle upon his tongue. He does not hefitate to confider it as a dreadful poifon. He could not reduce it but by fixed alkali and phofphoric glafs.

9. The

9. The author affirms, that by the aſſiſtance of phoſphorus he has decompoſed and ſeparated the ſulphuric, muriatic, and nitrous acids, from their baſes; and that he has tranſmuted the earths by means of phoſphoric acid, ſo as to obtain conſiderable quantities of magneſia from calcareous earth. And he declares, that by his operations upon phoſphorus he has obtained proceſſes, by means of which he has effected the frit of rubies *, the fuſion of emeralds, and vitrification of mercury.

IX.

Letter of Enquiry reſpecting the proper Form of a Boiler for Steam Engines.

To MR. NICHOLSON.

SIR, 17th April, 1799.

I AM about to erect a large ſteam-engine, but am in doubt of the proper conſtruction of boilers. Thoſe which I uſe at preſent, appear to me very improper for raiſing the greateſt quantity of ſteam with the leaſt fuel. The form, however, has been in repute for 10 or 15 years paſt. My boilers are 8 or 9 feet in height, 4 to 5 in breadth, and 9 feet in length. Through the middle lengthways is a flue of iron plates, as is the reſt of the boiler, and the water muſt always cover this flue, conſequently ſtand 5 to 6 feet deep in the boiler. This flue, with the flues on the outſide of the boiler running parallel to it, is ſaid to accelerate greatly the production of ſteam, and to have the ſame effect as a great fire-place below a broad and ſhallow boiler. But I have often obſerved, that a flame paſſing through any flue in which there are no obſtructions, to make the flame reverberate, communicates very little heat to the ſide in proportion to the fuel waſted. And, in my boilers, while the four ſides of the internal flue have been perfectly entire, the end of the flue where it turns off has been burnt through. If this internal flue communicates therefore little heat to the ſurrounding water, the greateſt part of the ſteam raiſed, muſt be from the loweſt ſtratum of water in contact with the bottom of the boiler; but by the conſtruction this ſtratum has to overcome a column of 5 or 6 feet of water. Hence I conclude, that the conſtruction of boilers to raiſe the greateſt quantity of ſteam with the leaſt fuel ſhould be ſhallow, and flues placed below, with obſtructions to force the flame againſt the bottom. Engineers object to ſuch a form on account of the danger of burning the boiler, by inattention in the engine-keeper, in not ſupplying water enough always to cover the parts acted on by the fire, and the difficulty of preſerving a highly elaſtic ſteam. But ſurely it is eaſy to proportion the ſupply of water to the evaporation, and a contrivance ſimilar to a ball-cock in water-ciſterns would obviate the firſt objection. By forming the crown of the boiler with a flat arch not far removed from the ſurface of the water, I can diſcover no reaſon why ſteam as elaſtic as in the preſent form of boilers may not be always at command.

You will oblige many of your readers by giving your opinion on this ſubject, ſo intereſting to the arts.

I am, &c.

A. MINER. W.

* *Il opère la frite des rubis.* An expreſſion which I do not underſtand.—N.

The

This letter arrived too late in the month for me to give an abridgement of ſo much of Count Rumford's excellent *Eſſay VI. *On the Management of Fire and the Economy of Fuel*, as is applicable to the enquiry it contains. If I can have the Count's permiſſion to copy ſome of the engravings (of which I have no doubt), this ſhall appear next month. In the mean time I muſt refer my correſpondent to the eſſay of the ſame author, on the propagation of heat in fluids, of which accounts are given in this Journal, vol. I. 289. 341. 563. He will there obſerve that fluids communicate no perceptible quantity of their heat in any other way, than by the actual contact with ſolids, produced by the relative motions of their parts; and from this leading fact it will follow, that the quantity of heat communicated to the boiler from a given fire, will be governed by the arrangements for cauſing the greateſt number of particles of heated air and flame to come into contact with it, and for preventing the heat being conducted off. It is very obvious, that the boiler he deſcribes is not calculated to abſorb all the heat, but that from the ſhortneſs and ſtraitneſs of the channes, thoſe heated fluids muſt paſs off at a very high temperature, to the conſequent diminution of effect and waſte of fuel. I do not here minutely enquire what may be the beſt figure for a ſteam-boiler of iron, at the uſual ſtrength of 10lb. to the ſquare foot, to ſuſtain an internal preſſure of between one-ſixth and one-fourth of an atmoſphere, becauſe every one is aware of the advantages of a figure approaching to the ſpherical form, and becauſe other figures of much leſs ſtrength will hold very well under the circumſtances above-mentioned.

The principal queſtion excluſive of the ſaving of heat, hereafter to be conſidered, is to determine how far the depth or ſhallowneſs of the water, in the boiler, may influence the production of ſteam.

If the parts of a maſs of water could preſerve the ſame relative poſitions, during the application of heat, and this heat were applied at the bottom under a depth of ſix feet, it would depend upon the conducting power of the fluid, whether the upper or lower parts ſhould give off the greateſt quantity of ſteam in a given time. If the lower parts were to afford the greateſt product, this would be done under a greater preſſure and at a higher temperature; ſo that it would become an object of experimental reſearch, whether ſuch ſteam, by giving out heat to the ſuperincumbent water, through which it muſt riſe, and rendering a portion of it elaſtic, might not be as effectual in its ultimate operation, as if the heat had been employed upon a ſhallower maſs. But the fact is, that the parts of fluids do not preſerve their relative ſituation during the application of heat: the water in the boiler will expand and riſe with rapidity as it acquires heat, and, in all probability (for in this caſe alſo we are in want of facts), will give off a much larger portion of ſteam while it circulates near the upper ſurface, and is loſing its elevated temperature, than while it glides along the bottom in the act of receiving heat. From this view of the ſubject, though I am diſpoſed on the whole to conclude that the ſhallower maſs of water may, in many reſpects, deſerve the preference, yet I doubt whether the difference ariſing from mere preſſure be an object of any conſiderable importance. As the medium temperature of the whole maſs of a fluid, heated from beneath, is

* Publiſhed before the commencement of this Journal.

is higher when deep than when ſhallow, it is found to be of ſome conſequence in the diſtillation of fermented liquors, that the charge ſhould not be deep. But this appears to be leſs on account of any ſuppoſed rapidity of evaporation, than becauſe the flavour of the product may be altered, by ſuch increaſe of temperature.

To afford a regular ſupply of water to the boiler, without depending upon the engine-keeper, is certainly an object of no difficulty, and is actually done in many ſteam-engines, by means ſimilar to that ſuggeſted by my correſpondent. In Mr. Kier's engine, deſcribed in this Journal, I. 422. the water is admitted through a valve, kept ſhut by a float, which falls as the water ſubſides by evaporation. On which particular, I may here add, that the body immerſed in the water is not a ſimple float, but a piece of ſtone that hangs from the valve, which is aſſiſted in its tendency upwards by a balance-lever and counter weight. When the ſtone is immerſed to a certain depth, the weight attached to the lever predominates, but when the water falls below that depth, the ſtone exerts more of its gravity, and keeps the valve down, till the requiſite ſupply of hot water has flowed in.

21ſt April, 1799.

W. N.

X.

Deſcription of the Furnace for converting Bar-iron into Steel, by Mr. JOSEPH COLLIER*.

THE furnaces for making ſteel are conical buildings, about the middle of which are two troughs of brick, or fire-ſtone, which will hold about four tons of iron in the bar. At the bottom is a long grate for fire. The ſteel furnace, however, is not well adapted with deſcription. I ſhall, therefore, avail myſelf of an accurate account, which was communicated to me by a gentleman converſant with the manufacture.

A layer of charcoal-duſt is put upon the bottom of the trough, and upon that a layer of bar-iron, and ſo on alternately, until the trough is full. It is then covered over with clay, to keep out the air, which, if admitted, would effectually prevent the cementation. When the fire is put into the grate, the heat paſſes round by means of flues, made at intervals, by the ſides of the trough. The fire is continued until the converſion is complete, which generally happens in about eight or ten days. There is a hole in the ſide, by which the workmen draw out a bar, occaſionally, to ſee how far the tranſmutation has proceeded. This they determine by the bliſters upon the ſurface of the bars. If they be not ſufficiently

* Extracted from his *Obſervations on Iron and Steel*, in the Mancheſter Memoirs, V. 109.—The paper itſelf contains a conciſe and clear, though in ſome reſpects imperfect, account of the uſual proceſſes of the ſmelting and purifying this uſeful metal. This author, who, inadvertently, blames M. Fourcroy and myſelf for this inevitable conſequence of abridgment in our elementary writings, has himſelf ſhewn that the taſk of deſcribing complex operations in a few words, is ſufficiently difficult to afford a claim upon the public indulgence.—On Steel, ſee Philoſ. Journal, I. 210. 248. 381. 468. 575; II. 64. 102.

changed,

changed, the hole is again closed carefully to exclude the air; but if, on the contrary, the change be complete, the fire is extinguished, and the steel is left to cool, for about eight days more, when the process for making blistered-steel is finished.

Fig. 1, plate IV. is a plan of the furnace; and fig. 2 is a section of it, taken at the line A B. The plan is taken at the line C D. The same parts of the furnace are marked with the same letters, in the plan and in the section. E E are the pots, or troughs, into which the bars of iron are laid, to be converted. F is the fire-place. P, the fire-bars. And R, the ash-pit. G G, &c. are the flues. H H is an arch, the inside of the bottom of which corresponds with the line I I I I, fig. 1, and the top of it is made in the form of a dome, having a hole in the centre at K, fig. 2. L L, &c. are six chimnies. M M is a dome, similar to that of a glass-house, covering the whole. At N there is an arched opening, at which the materials are taken in and out of the furnace, and which is closely built up when the furnace is charged. At O O there are holes in each pot, through which the ends of three or four of the bars are made to project quite out of the furnace. These are called tasting-bars, one of them being drawn out occasionally to see if the iron be sufficiently converted.

The pots are made of fire-tiles, or fire-stone. The bottoms of them are made of two courses; each course being about the thickness of the single course which forms the outside of the pots. The insides of the pots are of one course, about double the thickness of the outside. The partitions of the flues are made of fire-brick, which are of different thicknesses, as represented in the plan, &c. by dotted lines in the bottom of the pots. These are for supporting the sides of the pots, and for directing the flame equally round them. The great object is to communicate to the whole an equal degree of heat in every part. The fuel is put in at each end of the furnace, and the fire is made the whole length of the pots, and kept up as equally as possible.

XI.

On the Origin of the Areometer, by Citizen EUSEBE SALVERTE*.

AT the present moment, while Hassenfrantz is publishing his interesting memoirs upon Areometry in your Journal*, it will, probably, be some gratification to your readers to receive some information concerning the origin of the instrument to which that science has given its name.

In the Encyclopédie méthodique. Physique, I. 257, is the following passage: "It is commonly thought, that the Areometer was invented about the end of the fourth century by Hypacia, the daughter of Theo †, as we learn in the fifth letter of Synesius Cyreneus, in his fifth letter."

* Annales de Chimie, XXVII. 113.

† Hypacia was a Platonic philosopher, equally celebrated for her virtue, science, and beauty. The people of Alexandria, excited against her by St. Cyril, slew her in the 415th year of the Christian era.—S.

This

This notice is not accurate, if the poem De Ponderibs et Menfuris, printed at the end of the works of Prifcian, and admitted by all the learned to have been written by Rhemnius Fannius Palaemon be truly afcribed to that grammarian: Rhemnius lived under Tiberius, Caligula, and Claudius Cæfar, and, confequently, was three centuries anterior to Hypacia. The follow ing is the defcription which he gives of Areometry, which is equally valuable for its per fpicuity and exactnefs:

Ducitur argento tenuive ex ære cylindrus,
Quantum inter nodos fragilis producit arundo,
Cui cono interius modico pars ima gravatur
Ne totus fedeat, totufve fupernatet undis,
Lineaque à fummo tenuis defcendit ad ima,
Ducta fuperficie, tot quæque in frufta fecatur
Quot fcruplis gravis eft argenti ærifve cylindrus.
Hoc, cujufque potes pondus fpectare liquoris.
Nam fi tenuis erit, majori immergitur undâ;
Sin gravior, plures modulos fupereffe notabis.
Aut fi tantumdem laticis fumatur utrinque
Pondere præftabit gravior; fi pondera fecum
Conveniunt, tunc major erit quæ tenuior unda eft.
Quod fi ter feptem numeros texiffe cylindri,
Hos videas latices, illos cepiffe ter octo,
His drachmâ gravius fatearis pondus ineffe.
Sed refert æqui tantum conferre liquoris,
Ut gravior fuperet drachmâ, quantum expulit undæ
Illius aut hujus, teretis parfima cylindri*.

It cannot, therefore, be doubted but the Areometer was an inftrument well known, and commonly ufed, three hundred years before the birth of Hypacia. It is difficult to conceive how Synefius, contemporary and friend of this celebrated lady, could attribute the invention

* In Englifh, " A cylinder is made of filver, or thin brafs, in length equal to the diftance between the knots of a brittle reed. The lower part is loaded within, fo that it fhall neither totally fink, nor entirely float. A fine line is drawn from the upper to the lower extremity of its furface, and divided into as many portions as the fcruples which exprefs the weight of the cylinder. With this inftrument the relative weight of any fluid may be afcertained: for, if it be light, the cylinder will fink deeper; or, if heavy, the number of divifions above the furface will be more confiderable. If equal bulks of different fluids be taken, the denfeft will exceed the other in weight; or, if the weights be equal, the rareft fluid will occupy the greater fpace. For example, if the cylinder be found to fink through twenty-one divifions in one fluid, and in another through twenty-four, it may be concluded, that the heavier exceeds the other by three fcruples, or one drachm. But, it is more accurate, inftead of attending to the difference, to compare the two fluids, by attending to the quantities of each in magnitude, which are difplaced by the immerfed part of the cylinder."

to

to her *. But the following line immediately after this defcription in Rhemnius, is ftill more remarkable :

Nunc aliud partum ingenio trademus eodem.

" Let us now defcribe another invention of the fame genius."—After which he proceeds to defcribe the method ufed by Archimedes, to afcertain the quantity of filver contained in the crown of Hiero.

It appears, therefore, to be certain, that we owe the invention of the Areometer to the fame man who enriched the accurate fciences with fo many difcoveries, and who, to the glory of his talents, added, that of living for the welfare of his country, and dying in its defence.

The poem of Rhemnius, or, rather, the fragment which remains, deferves to be known. Independent of the two defcriptions I have quoted, and a complete fyftem of ancient meafures, it contains other interefting details. Such is the following obfervation, which fuppofes experiments of fome delicacy on the fpecific gravity of liquids.

† Namque nec errantes undis labentibus amnes
Nec merfi puteis latices, nec fonte perenni
Manantes, par pondus habent : nec denique vina,
Quæ campi aut colles, nuperve aut ante tulêre.

Permit me to obferve the elegance and accuracy of thefe expreffions. They appear to me to belong to a writer of good latinity : and to obviate all the doubts which can be raifed concerning the date of the poem, another citation will confirm this idea :

————Pondus rebus natura locavit
Corporeis : elementa fuum regit omnia pondus.
Pondere terra manet ; vacuus quoque ponderis Æther
Inde feffa rapit volventis fydera mundi.

An author of the fixth century, fuch as Prifcian, the only one to whom the poem of Rhemnius could be attributed, could not eafily have compofed thefe verfes.

* With all the advantages of the art of printing, we find, that valuable inventions are forgotten, and re-invented, during the lapfe of periods much fhorter than three centuries. Whoever will perufe the writings of Boyle, Kunkel, Hooke, Wren, and the regifters of the learned focieties and fcientific correfpondences during the laft century, will have ample reafon to be convinced of this.—Hypacia might be the fecond inventor.—N.

† In Englifh, " The weights of river, well, and fpring-water, are different ; as are likewife the wines, ac-" cording to the place of their growth, in hills or places, and the time of keeping."

XII.

*Chemical Considerations on the Use of the Oxydes of Iron in the Dying of Cotton. By J. A. Chaptal *.*

THE oxyde of iron has so strong an attraction to the fibre of cotton, that if the latter be plunged in a saturated solution of iron in any acid whatever, it assumes a chamois yellow colour, of greater or less intensity, according to the strength of the solution. It is no less curious than easy to make the experiment of passing a piece of cotton through a solution of sulphate of iron rendered turbid by the oxyde which remains suspended in the fluid. Nothing more is necessary than to pass the cotton through the bath, from which it seizes the last particle of the oxyde, and restores its transparancy. The solution, which before was yellowish, immediately becomes more or less green, according to its strength.

The colour which the oxyde of iron gives to cotton, becomes deeper by simple exposure to the air; and this colour, which is soft and agreeable at first taking out of the bath, becomes hard and ochreous by the progressive oxydation of the metal.

The colour of the oxyde of iron is very solid. It resists the action not only of the air and water, but likewise of alkaline solutions. Soap gives it brightness without perceptibly diminishing its intensity.

From these properties it is, that the art of dying has availed itself of the oxyde of iron as a most valuable colouring principle. But I have succeeded in giving a new extension to the applications of this oxyde. I shall confine myself in presenting to the Institute those results only which deserve to be adopted in the practice of manufacturers, and have been executed for several years in my dying-works.

In order that the oxyde of iron may be conveniently applied to the cotton thread, its solution must first be effected. Acids are used as the best solvents.

Most dyers make a mystery of the acid they use; but the universal practice is confined to the acetous, the sulphuric, the nitric, and the muriatic acids.

Some dyers attribute great differences to the acid they use in the solution of iron; but the preference is commonly given to the acetous acid.

This predilection appears to me to be grounded much less upon the difference of the colours, which one acid or the other may give, than upon the corrosive action which each of them exerts to different degrees on the stuff. This is so great, with respect to the sulphate and the muriate, that if the piece be not washed immediately as it comes out of the bath, it will certainly undergo an effect of the same nature as combustion; whereas the solutions in the acetous, or any other vegetable acid, do not produce this inconvenience.

The iron appears to exist at the same degree of oxydation † in the different acids, since it produces the same shade of colour when precipitated; and any acid salt may be indiscriminately used, provided the nature of the salt, and the degree of saturation in the acid, be sufficiently known;

* Read to the French National Institute, 21 Germinal, in the sixth Republican year, and inserted in the Annales de Chimie, XXVI. 266, whence this translation is made.

† On this subject see, however, the Philos. Journal, I. 453.

known; for the subsequent operations may then be properly conducted, and the inconvenience attending the use of some of these salts may be prevented. This no doubt is a leading advantage possessed by the man of information, beyond the simple operative artist, who is incapable of varying his processes according to the nature and state of the salts he may use.

I shall confine this paper to accounts of the colour obtained from the oxyde of iron: 1. When it is used alone upon goods which have received no previous preparation, and, 2. when it is employed jointly with the red of madder, on a piece prepared to receive the Adrianople red.

1. If sulphate of iron or any other martial salt be dissolved in water, and cotton be plunged therein, it will acquire a chamois tinge, more or less deep, accordingly as the solution may be charged with the salt. The affinity of cotton with the iron is such, that it attracts the metal, and takes a great part from the acid which held it in solution.

(To be concluded in our next.)

SCIENTIFIC NEWS, &c.

Respirability of the Gaseous Oxyd of Azote.

Extract of a Letter from Mr. H. DAVY.—*Dated, Clifton, April* 17, 1799.

"I HAVE this day made a discovery, which, if you please, you may announce in your Physical Journal, namely, that the nitrous phosoxyd or gaseous oxyd of azote, is respirable when perfectly freed from nitric phosoxyd (nitrous gas). It appears to support life longer than common air, and produces effects which I have no time to detail at present. Dr. Mitchill's theory of contagion is of course completely overturned; the mistake of Priestley and the Dutch chemists, probably arose from their having never obtained it pure. I am now preparing a paper on this subject, for the next volume of the West-country Contributions."

In a subsequent communication, Mr. Davy expresses his apprehension, lest a general notice of the respirability of gaseous oxyd of azote should induce any one to make injurious experiments on himself, and therefore wishes it should be added, that the circumstances of safety and of hazard will be speedily pointed out to the public. A train of experiments, by which he hopes to clear up this perplexed subject, are in progress; and he is the more solicitous that this general notice should be published, because some most remarkable phenomena witnessed by divers persons, in the pneumatic institution, and leading to useful practices, have been a good deal talked of at Bristol, and an erroneous anticipation may get into print, before the true account can be prepared.

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I underſtand by a letter from the Rev. Mr. Pearſon, of Lincoln, that he did not mean to imply, by the words in the firſt parentheſis on page 53, that the earth has any perceptible irregularities in its rotation, of which he obſerves that a ſuſpicion was entertained by Kepler. The inaccuracy of expreſſion aroſe from his attention being directed to the length of the apparent ſolar day, inſtead of mean ſolar time.

New Produce of European Sugar.

I have heard much, within the laſt fortnight, concerning the product of ſugar from a ſpecies of the beet, or mangel wurzel, by Mr. Achard. The experiments of Margraaf and others many years ago, who obtained ſugar from vegetables by treatment with alcohol, are well known to chemiſts. But it is ſtated, that the chemiſt of Berlin, has obtained it by a proceſs ſufficiently cheap, and in quantities ſo conſiderable, that if the reports which are circulated be true, the diſcovery will bid fair to change a large part of the commercial ſyſtem of the world, with regard to this great article of modern conſumption. The experiments are ſaid to have been confirmed, on a large ſcale, by Klaproth. But I muſt defer any further report, until the ſubject can be exhibited in an unqueſtionable ſhape.

A tranſlation of Dr. Gren's Elementary Treatiſe on Chemiſtry, in two volumes octavo, is ready for the preſs. This work is an abridgement by the author, of his own ſyſtem of chemiſtry, of which the ſecond edition, in four large octavo volumes, was publiſhed in 1796. A work highly eſteemed throughout Germany, for ſyſtematical arrangement, and the extent of knowledge it diſplays. Though Dr. Gren was neither of the phlogiſtian nor antiphlogiſtian order of chemiſts, but maintained a ſyſtem of his own, in which both theories were combined, he has neverthelefs choſen to follow the antiphlogiſtian ſyſtem in his abridged work. The tranſlator has added the diſcoveries made ſince the year 1796.

Dr. Frederick Albert Charles Gren, public profeſſor of medicine at Halle, died on the 26th of November laſt, of a nervous fever, in the 38th year of his age. The public proofs which he gave of ability and induſtry, have rendered his loſs a ſevere affliction to the ſcientific world. He was author of an Introduction to natural philoſophy, of which the third edition was publiſhed in 1797;—Elements of Pharmacology, in two volumes octavo, of which the ſecond edition was publiſhed in 1798;—and alſo the Journal of Natural Philoſophy, began in 1770, of which eleven volumes are extant.

April 27, 1799.

I have received ſome papers from Dr. Beddoes, relating to the ſubject animadverted upon at page 41 of the preſent volume, and alſo a letter from Dr. Gibbes. Dr. Beddoes does not conſider his documents as ſufficiently intereſting to the public, to deſerve a place in the Journal;

Journal; but wishes them to be printed on a separate leaf. But as such a leaf would, no doubt, be considered and preserved by all my readers as a part of the Journal, and as the general cause of morality and science is involved in discussions of this nature, I have preferred that an abridged statement should appear in the Journal itself.

The papers I have received are: 1. Letter from Dr. Beddoes, dated April 24. 2. Letter from the Rev. Mr. Richardson, of Bath, to Dr. Beddoes, dated 17 (I suppose April). 3. Questions to Mr. Notcutt, by Dr. Beddoes. 4. Mr. Notcutt's answer, dated April 21. 5. Queries to Mr. Davy, with his replies. These papers were received yesterday, and by this morning's post I received a letter from Dr. Gibbes, dated April 26. In Mr. Richardson's letter it is stated to be a misrepresentation, that Mr. Notcutt pointed out some sulphate of strontian in his (Mr. R's) collection; but he admits, that Mr. N. said the specimen in question might probably be sulphate of strontian; but Mr. Richardson does not remember ever mentioning this conjecture to Dr. Gibbes.—Mr. Notcutt in his letter admits, that Dr. Beddoes' statement of Mr. N. having recognized the sulphate of strontian, is correct to the best of his recollection; that in a late conference with Mr. Richardson, this last gentleman had affirmed, that Mr. Notcutt had not spoken with certainty in giving his opinion of the specimen at the time alluded to: upon which Mr. Notcutt observes, that he cannot pretend to repeat exactly the words he may have used, but that it is evinced, that he felt the strongest conviction of its being that substance, by his informing Dr. Beddoes, Mr. Clayfield, and others, immediately upon his return to Bristol, that it had been found near Sodbury, as well as by his authorizing the former to notice it in the volume of papers just published; that he, Mr. Notcutt (from Dr. Gibbes' knowledge, that considerable quantities of the same substances had been discovered near Bristol by Mr. Clayfield, and that Mr. Clayfield had long been engaged in a course of experiments upon its analysis, and that no official account of it was before the public), cannot account for his omitting to notice what was so immediately connected with the subject of his paper, without supposing that he wished to take to himself more than the merit of having analyzed the specimen from Sodbury.—Mr. Davy in his answers to the queries proposed to him, states, that Mr. Notcutt, in a conversation previous to the publication of Dr. Gibbes' experiment in the Philosophical Journal, did inform him that sulphate of strontian was found in large quantities near Sodbury; and admits, that Mr. Notcutt had mentioned before that period, that he (Mr. Notcutt) had positively told Mr. Richardson, that the mineral in question was sulphate of strotian. And lastly, he partly admits and partly states, that Dr. Gibbes was present at conversations respecting Mr. Clayfield's analysis, who confidentially communicated to him (facts or incidents) respecting the same mineral in the same district. And that Dr. Gibbes must have known, that by publishing his analysis prior to Mr. Clayfield's, and without communicating it to him, he (Dr. G.) would be considered as the first discoverer of the sulphate of strontian, to the prejudice of Mr. Clayfield, whose name ought to have been mentioned.—The testimony of Mr. Clayfield himself could not be obtained in time for Dr. Beddoes to send it with the other papers, because Mr. C. was then absent from Bristol.

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On the other hand, Dr. Gibbes himself states, that he knew of Mr. Clayfield's experiments before he wrote me the account of his own, and had noticed them, and Mr. Clayfield's name, prior to that time, in a work published last January *; that he never knew that the variety of which he sent so imperfect an analysis had been noticed by any one, and he understands that no one has to this day subjected it to the test of experiment but himself. He thought Mr. Clayfield's experiments had been publicly announced, and now finds by the note to p. 41 of this Journal, that this was in fact the case;—that the great difference in the specimens of this mineral found in Gloucestershire, have deceived the best judges;—that Mr. Richardson positively affirms, that Mr. Notcutt did not know what the mineral in question was, but that had he even affirmed it to be the sulphate of strontian, it would be of no consequence to the case, because Mr. Richardson never mentioned his name nor his opinion to Dr. Gibbes;—that in the papers transmitted to me (which have been communicated to Dr. Gibbes), he accuses Dr. Beddoes of omitting to mention a second letter of Mr. Richardson, which was strong in favour of Dr. Gibbes;---that the idea of losing Mr. Clayfield's good opinion, for whom he entertains the most profound respect, is the only circumstance which can give him pain in this business; and that the high gratification he has received from the Philosophical Journal, produced the wish to give a testimony of approbation by a communication, in consequence of which he instantly embraced the first favourable opportunity of so doing.

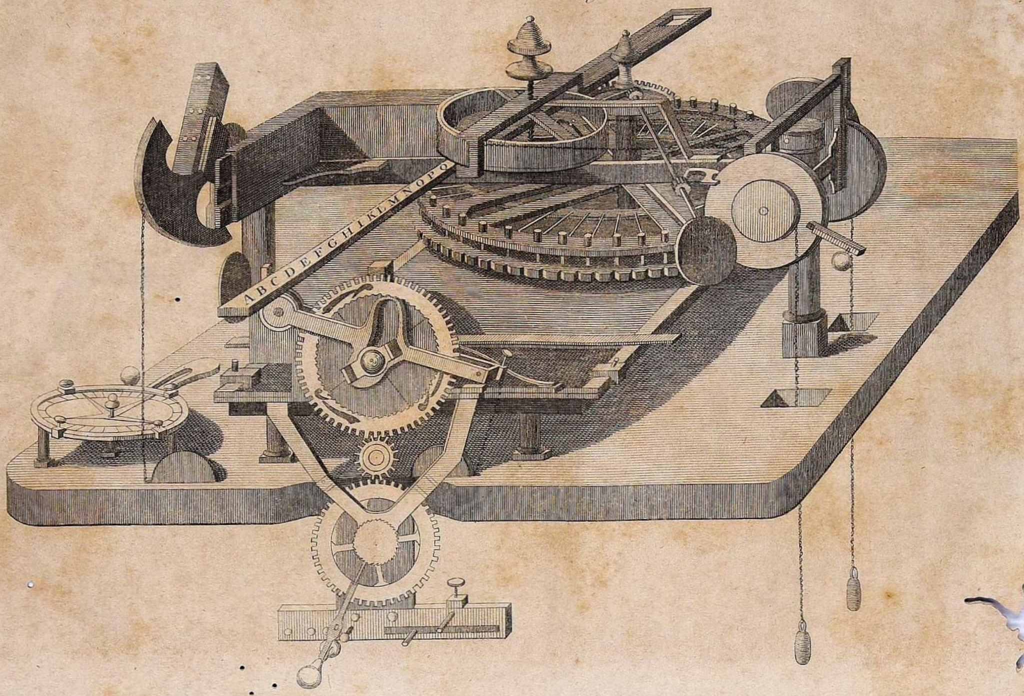
I hope that the length of this abridged account will be a sufficient apology for my not having inserted the original papers. As an individual, to whom the parties are no otherwise known than by their public labours, and respectable stations, I may venture to add the following observations.

It appears on the whole: 1. That Mr. Clayfield discovered the sulphate of strontian in England long ago, as related at p. 36 of our present volume. 2. That this discovery was imparted to hundreds of persons; to a large audience at Bristol; to a learned society at Manchester; and to a periodical work of large circulation (page 41). Whence, it seems hard to imagine, that the mere discovery could be any part of a confidential communication to Dr. Gibbes. 3. That the probability of the Rev. Mr. Richardson having mentioned the opinion of Mr. Notcutt to Dr. Gibbes, is removed by the negative of Mr. Richardson himself, as to that point. And 4. lastly, that though it might be wished, that Dr. Gibbes, while mentioning his own experiments, had noticed their connexion with those other observations of such public notoriety; yet, the chief and probably the only cause of offence in the publication, at page 535 of vol. 2. of this Journal, will be found to consist in the apparent ostentation of the title, which was not written by Dr. Gilles, but by the Editor.

* This work I presume to be his *Syllabus of a Course of chemical Lectures*, where I find the following head. "STRONTITES—Where found—Analogous to barytes—Its properties—Produces red flame when applied "to a candle—In what differs from barytes.—Phosphure of strontian.—M. Pelletier, Mr. Hope, Mr. Schmei- "ser, Mr. Clayfield.—Sulphate of strontites found near Bristol."—N.

Philos. Journal. Vol.III. Pl.III. facing p. 96.

Rochon's Machine for engraving Metallic plates.



Barlow sculp.

Section & Plan of the Furnace for converting Bar Iron into Steel.

Fig. 2.

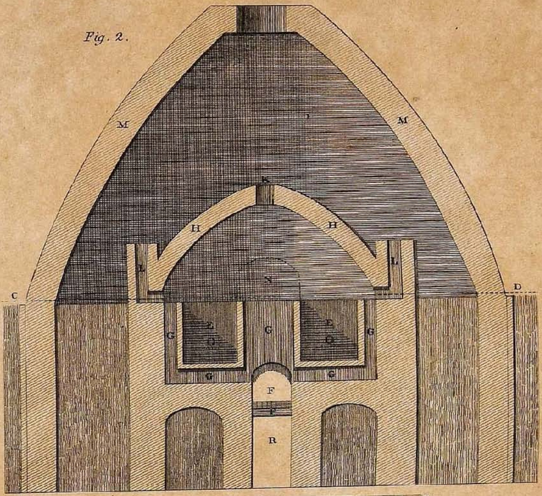
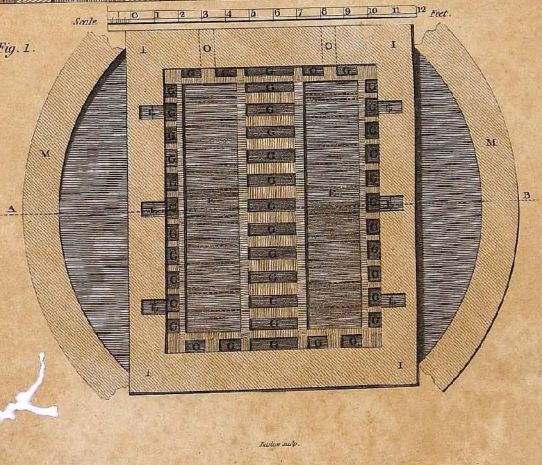


Fig. 1.



Barlow sculp.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE 1799.

ARTICLE I.

An Account of ſome Endeavours to aſcertain a Standard of Weight and Meaſure. By Sir GEORGE SHUCKBURGH EVELYN, *Bart. F.R.S. and A.S.**

HAVING for ſome years turned my thoughts to the conſideration of an invariable and imperiſhable ſtandard of weight and meaſure, as being a thing in a philoſophical view highly deſirable, and likely to become extremely beneficial to the public, I had, ſo early as the year 1780, taken up the idea of an univerſal meaſure, from whence all the reſt might be derived, by means of a pendulum with a moveable centre of ſuſpenſion, capable of ſuch adjuſtments as to be made to vibrate any number of times in a given interval; and by compariſon of the *difference* of the vibrations, with the *difference* of the lengths of the pendulum (which difference alone might be the ſtandard meaſure), to determine its poſitive length, if that ſhould be thought preferable, under any given circumſtances; by which means all the difficulties ariſing in determining the actual centre of motion and of oſcillation, which have hitherto ſo much embarraſſed theſe experiments, would be gotten over.

§. 2. I made ſeveral computations of the probable accuracy that might be expected from ſuch an experiment, and was ſatisfied with their reſult. But not ſeeing clearly how ſuch a

* Philoſ. Tranſ. 1798.

pendulum could be connected to a piece of mechaniſm to number the vibrations without affecting them, I dropped the idea for that time. I learnt, however, ſome time afterwards, that Mr. John Whitehurſt, a very ingenious perſon, had been in purſuit of the ſame object, with better ſucceſs, and had contrived a machine fully correſponding to his expectations and my wiſhes. This he afterwards explained to the world in a pamphlet entitled "An Attempt to obtain "Meaſures of Length, &c. from the Menſuration of Time, or the true Length of Pendulums," publiſhed in 1787. Mr. Whitehurſt having therein done all that related to the ſtandard meaſure of length, and ſuggeſted that of weight, it appeared to me that it remained only to verify and complete his experiments.

§. 3. For this purpoſe, by the kind aſſiſtance of my friend Dr. G. Fordyce, who at Mr. Whitehurſt's death had purchaſed his apparatus, I was furniſhed with the very machine with which Mr. Whitehurſt had made his obſervations. I alſo procured to be made by Mr. Troughton a very excellent beam-compaſs or divided ſcale, furniſhed with microſcopes and micrometer, for the moſt exact obſervations of longitudinal meaſure; as alſo a very nice beam or hydroſtatic balance, ſenſible with the $\frac{1}{100}$ of a grain when loaded with 6lb. troy at each end. Mr. Arnold made me one of his admirable time-keepers, in order to carry time from my ſidereal regulator in my obſervatory, with which it was adjuſted, to the room wherein I had fixed Mr. Whitehurſt's pendulum; and who having taken a journey into Warwickſhire, was ſo good as to aſſiſt in the beginning of theſe experiments. Thus equipped, I went to work in the latter end of Auguſt, 1796, when the temperature was about 60°, firſt to examine the length of the pendulum; when, to my great mortification, I found that the thin wire of which the rod conſiſted was too weak to ſupport the ball in a ſtate of vibration; and that after 15 or 20 hours' action it repeatedly broke. The ſame misfortune attended my trials with three other different ſorts of wires that I had obtained from London. Whether this accident happened from any ruſt in the old wire, or from want of due temper in the new, or from its being too much pinched between the cheeks *, I cannot tell. I can only obſerve, that all the wires that I uſed were conſiderably heavier, and, therefore, probably ſtronger than what Mr. Whitehurſt mentions, viz. 3 grains in weight for 80 inches in length; nay, mine proceeded as far as from 5 to 6 grains for that length, and yet I could never get it to ſupport the ball during the whole period of my experiment. This being the caſe, and being in the country, far removed from the manufactory of this fine wire, I was reluctantly compelled to relinquiſh this part of the operation to ſome more favourable opportunity. In the mean while, however, I thought it deſirable to meaſure the difference of the lengths of Mr. Whitehurſt's pendulum from his own obſervations; for very fortunately the marks that he had made on the braſs vertical ruler of his machine were ſtill viſible, and this interval, which he calls "59,892 inches," I determined on my divided ſcale made by Troughton from Mr. Bird's ſtandard to be=59,89358 inches, from a mean of four different trials in the temperature of 64°; that mean differing from the extremes only=,0003 inch †.

* *c c* Fig. 1. of Plate II. in Mr. Whitehurſt's pamphlet.

† See the remark in Philoſ. Journal, III. 32. note.—N.

§. 4. By

§. 4. By this examination, if I have not verified I have at least preserved Mr. Whitehurst's standard; and, for the present, I shall consider this measure of the difference of the length of the two pendulums, vibrating 42 and 84 times in a minute of mean time, as correct. On this presumption I shall proceed to the examination of weight.

§. 5. From the opinion of different skilful persons with whom I have conferred, as well as from the result of my own considerations, I am inclined to believe, there is hardly any body in nature with which we are familiarly acquainted, that is of so simple and homogenous a quality as pure distilled water, or so fit for the purposes of this enquiry; and I have concluded, that if the weight of any quantity of water whose bulk had been previously measured by the abovementioned scale, could be attained under a known pressure * and temperature of the atmosphere, we should be in possession of a general standard of weight.

§. 6. With this view, I directed Mr. Troughton to make, in addition to the very sensible hydrostatic balance before mentioned, a solid cube of brass, whose sides were 5 inches; and also a cylinder of the same metal, 4 inches in diameter and 6 high. From St. Thomas's hospital, by favour of Dr. Fordyce, I procured 3 gallons of distilled water; with these I made the following observations: but before I relate the experiments, I will describe the apparatus.

Mr. Whitehurst's machine for measuring the pendulum, has been sufficiently explained in his pamphlet mentioned above; my divided scale, which was a new instrument, was as follows:

§. 7. *Description of the Beam-compass, or Divided-scale of equal parts.*

a b (Plate V. fig. 1.) is a block or beam of mahogany, 6 feet 3 inches long, 6 inches deep, and 5 wide, upon which are laid two brass rules, *c d e* and *f g*, each divided into 60 inches and tenths. The former of these, called the scale, is for a time kept immoveable by the finger-screws *c e d*, and is furnished with fine hair-line divisions, intended to be viewed only by the microscopes, *h i*: the latter, called the beam, has no motion but by means of the screw *g*, and bears stronger divisions upon it, with which the sliding pieces or indexes at *k* and *m* may readily be compared by the naked eye, and is intended only to set the microscopes, or rather the wires in their focus, to the required distance nearly, viz. to within $\frac{1}{100}$ or $\frac{1}{200}$ of an inch. The microscopes are compound, and similar to those described by the late General Roy in his account of his large theodolite. (See Phil. Transf. vol. LXXX.) The one at *h* contains only cross-wires fixed in its focus; the other at *i* has a micrometer also, by means of which its cross-wires may be moved to the right or left, or over the image of the divisions of the scale, any given space not exceeding $\frac{1}{10}$ inch; and the quantity so moved may be measured by the divisions on the screw-head passing under the index at *o*. The divisions on these rules have been called inches and tenths; it was not necessary that they should be more than equal parts; but they were in fact laid down by Mr. Troughton from a scale of the late excellent artist Mr. J. Bird, who had divided into inches several scales of different lengths; one of which, 42 inches long, belonged to the late General Roy; a second, of 5 feet, was

* I do not here mean to infer any opinion respecting the compressibility of water, but only to say, that where water or any thing else is weighed in air, the density of that medium, as shewn by the barometer and thermometer, must be known in order to make allowances for it if necessary.

 purchased

purchased by Alexander Aubert, esq.; and a third, of 90 inches, which is now the property of the Royal Society, is kept in their archives, and is said to have been used by Mr. Bird, in dividing his large mural quandrants *. Besides these, he made two standards of three feet, by order of the House of Commons; of which I shall speak more hereafter. The mode of using this instrument is as follows.

§. 8. Let the object to be measured be supposed to be about six inches, and let it be desired to compare it with the interval between the 20th and the 26th divisions in the scale *c d*: move by hand the microscope *b*, with its sliding plate, until the division of the index at *k* coincide with the division of 20 inches on the rule of *f g*; then move by hand also the microscope *i*, with its sliding plate, and appendage *l m n o*, until the index division near *m* coincides with 26 on *f g*: the axes of the microscopes, or centres of their cross-wires, will be at the approximate distance of 6 inches. To correct this, examine if the wires of *p* correspond with a division on *c d*; if not, move the rule *f g* backward or forward by the screw *g* till they do; then will the microscope *b* be adjusted. Now examine if the wires in *i* cover exactly a division; if they do so, the true interval of 6 inches between the microscopes is obtained; if not, move the microscope *i* a little by means of the screw *l* till they do, and both the microscopes will be adjusted: then remove the rule *c e d* from its place, by taking out the screws *c e d*, and place the object to be measured in its room, at the same time taking care that it be exactly in the focus of the object-glass of the microscope, in such a manner that one extremity may correspond with the wires in the microscope *b*; that done, if the other extremity coincide with the wires in *i*, the dimension of the object is exactly 6 inches; if not, restore the coincidence by turning the micrometer screw *n*, and the divisions at *o* will give the difference in 1000ths and 10,000ths of an inch + or — 6 inches.

§. 9. *Description of the Hydrostatic Balance.*

a b c d (Pl. V. fig. 4.) is a box, which contains the whole apparatus when not in use; and when used, serves as a foot to the hollow brass pillar *e f g h*, which is fixed into it by the four screws at the bottom *e* and *f*. This pillar contains another within it, and which is raised up and down about $\frac{1}{10}$ inch, by means of the screw *x*: *n o* is the beam, 27 inches long, and 3,9 inches wide in its greatest diameter; each arm of which is made hollow and conical, for strength and lightness; through the centre at *m*, passes the axis of motion, the ends of which, when used, are suffered to fall gently upon two crystal planes, which are set horizontally, by means of the spirit levels *k l*, and the screws underneath the box, at *c* and *b*. The ends of this axis are of hardened steel, of a wedge-like shape, and reduced to a fine edge, viz. to an angle of about 40°, so as to move upon the planes with very little friction, and at the same time so hard, as (with due care in using) to be in no danger of being blunted: to prevent which, the inner pillar has a motion upwards, as has been said, by the screw *x*, and by means of a semi-circular arm at its upper extremity, lifts the beam of its bearings, when it is not used, or is greatly loaded. This axis is placed carefully at right-angles to the beam; and by means of two small brass springs

* A farther account of these scales is given in the appendix.

that

that press gently at the ends, is brought always to have the same bearing upon the crystal; so that no error need be feared from a small deviation from the right-angular position of the axis to the beam, should any such exist, and from its shape and quality, it may be considered as inflexible in ordinary experiments. At *p*, is a small adjusting screw, which raises or depresses a weight within; and with it, in consequence, the centre of gravity in the whole beam; by this means the motion in its centre may be brought to almost any required degree of sensibility. Should the centre of gravity be raised above the centre of motion, the beam would turn over; if it be in that centre, the beam would stand every where indifferently, without any vibration; if it be placed much below it, the vibration would be too quick, and its sensibility not sufficient; it is therefore brought by the screw *p* a very small quantity below the centre of motion, so as to describe one vibration in 40 or 50 seconds: the sensibility is then fully sufficient. At each end of the beam are circular boxes, *n* and *o*, through which pass the steel centres, from whence are suspended the scale pans *q* and *r*: these centres resemble, in some degree, those at *m*, but have their chamfered or angular edges upwards, and thereon hang the hooks β, to which are affixed the links α, and to them the three silken lines of the scale. Each of these centres has a motion in its respective box, by means of two small adjusting screws; that in *o* laterally, and that in *n* vertically; the former to make the two arms of the beam of an equal length, the latter to bring the three points of suspension of the beam and scales into a right line. At the extremity of the boxes are fixed two needle points, or indexes, which play against the ivory scale of division at *s* and *t*. These divisions, although they do not, indeed they cannot, shew any definite weight, are nevertheless very useful in making the adjustments, and even in weighing to the small fractions of a grain. *u v* are two steady plates that are raised or depressed by the wooden nut *w*, to check the vibrations of the scales *q* and *r*, and bring them speedily to an equilibrium; *y z* is a table, whereon the whole is placed, to raise it to an height convenient for experiments*.

To use with this beam I had three sets of weights made, viz.

The first set, or series, of fifteen weights, rising in a duplicate progression, from one to 16384 grains, viz.

No.		*Grains.*
1 =	- - - -	1
2 =	- - - -	2
3 =	- - - -	4

* This beam is constructed after that made by Ramsden for the Right Honourable Sir Joseph Banks, Bart. P.R.S. &c. and known by the name of the Royal Society's Balance. It is to be regretted that we have no good account of that instrument. The chief differences between this, and the beam described in the text, are, 1, that certain pieces, or rings, similar to those called *stops*, in telescopes, are driven into the cones, and greatly prevent flexure, by preserving the circular section. For a tube can scarcely bend, unless its cavity be made to change its figure: and 2, the planes which bear the fulcrum are set and ground together, and are levelled by a spirit level placed on the face of the stones themselves.—N.

No.	Grains.
4 =	- - - - - 8
5 =	- - - - - 16
6 =	- - - - - 32
7 =	- - - - - 64
8 =	- - - - - 128
9 =	- - - - - 256
10 =	- - - - - 512
11 =	- - - - 1024
12 =	- - - - 2048
13 =	- - - - 4096
14 =	- - - - 8192
15 =	- - - - 16384

Fractions of a Grain.

$\frac{1}{32}$

$\frac{1}{16}$

$\frac{1}{8}$

$\frac{1}{4}$

$\frac{1}{2}$

The second series of weights in an arithmetical order, as follows, viz.

Grains.	*Grains.*	*Grains.*	*Grains.*	*Decimal Fractions of a Grain, viz.*	
				100*th Grain.*	*Tenths.*
1	10	100	1000	,01	,10
2	20	200	2000	,02	,20
3	30			,03	,30
4	40	400	4000	,04	,40
5	50			,05	,50
6	60	600	6000	,06	,60
7	70			,07	,70
8	80	800	8000	,08	,80
9	90			,09	,90
			10,000		N.B. The fractions of a grain are made of fine wire flatted.
			20,000		

The third set consists of a weight of { 1 ounce, 2 ounces, 4 ounces, 8 ounces, 1 pound } troy.

§ 10. *a b c d* and *a b c d* (Plate V. fig. 5.) is the brass cube of five inches, that has been mentioned, suspended in its own scales by means of four fine wires from the arm *o* of the beam, fig. 4. by taking away the common scale *a r*. The cube rests upon a cradle, or cross, three arms of which are seen at *g h i*, and by this means may be weighed either in air or water, by immersion into the large glass vessel *g h*, fig. 7.

At fig. 6 is seen the cylinder *a b c d* and *a b c d*, four inches in diameter and five high, slung

flung in another cradle, part of which is seen at *g b h i*, supported by four wires from the point *f*.

In fig. 7, is seen a sphere of brass, *d*, six inches in diameter, flung in a cradle, *a b c*, by three wires*, from the links *f*, suspended in a glass jar†, containing near four gallons of water, whose temperature is shewn by a thermometer at *e*.

§ 11. It was necessary to measure the exact size and correctness of figure of this sphere. For this purpose was made a wooden gauge or frame *a b c d e* (Plate V. fig. 2), in which the sphere was placed upon semicircular pieces, within-lined with green cloth to prevent bruising it; upon this frame was placed a brass square *k l m n*, whose sides were about $\frac{1}{100}$ inch in length more than the diameter of the sphere. This square, by raising or lowering the screws *o r s*, was easily made to coincide with a plane passing through the centre of the sphere. *p* is a micrometer screw, the interior extremity of which is brought just to touch the surface of the sphere, while the opposite side bears gently against the interior side of the frame at *o*; and by turning the sphere round so as to present different diameters to these points of contact, any variety in the diameter may be seen by the index *l* and plate *q* divided into 10,000ths inch. To render this operation more convenient, three great circles were drawn with a pencil upon the sphere, at 90° distance from each other (the two former were traced by the artist in the lathe, while the sphere was making, and the third was drawn from them), and each was divided into 8 equal parts. The immediate result of these experiments would only give the differences, and not the absolute quantity of the diameter; for this purpose, a brass ruler, *r*, fig. 3, was made of such a length as just to go within the brass frame *k l m n*; and being substituted in the place of the sphere, could easily be compared with any given diameter, and afterwards measured with the divided scale, fig. 1. With these instruments I made the following observations, August 31, 1796, the thermometer being at 61°.

§ 12. Examination of the dimensions of the brass cube, by means of the divided scale.

The microscope and micrometer being both adjusted, as well with respect to their focus‡

* These wires were of such a size that 91 inches weighed 20,71 grains, consequently 1 inch = 0,2276 grain, and the three wires = 0,6828 grain; and their specific gravity being 8,7, their loss of weight by sinking 1 inch in water, would be 0,0785 grain. This correction it may be necessary hereafter to attend to.

† The glass jar is made somewhat conical, being in

	Inches.
Diameter at top	12,0
Ditto at bottom	8,7
Mean ditto	10,35
Mean height within	11,8
Contents in cubic inches	992,78
Which is in ale-gallons	= 3,8 = $15\frac{1}{5}$ quarts.

It may also be noted, that one inch in depth of the water near the top is = 113 cubic inches, which is equal to the exact bulk of the sphere, as will be seen hereafter.

	Inches.
‡ The focal length of the object lens is	= 0,75
The distance of the cross-wires from the object lens	= 2,00
The focal length of the combined eye-glass	= 1,50
Whence the magnifying power of the microscope becomes	= 14,2 times.

as

as to the value of the micrometer ſcale, the croſs-wires in their focus were removed to a diſtance from each other of five inches *nearly* on the beam (the former being at 27, and the latter at 32 inches), and then *correctly* adjuſted to this interval on the divided ſcale. I muſt obſerve, indeed, that the value of the micrometer ſcale was not exactly ten revolutions of the ſcrew to $\frac{1}{10}$ inch, as Mr. Troughton deſigned; but this meaſure by the ſcrew*, from 6 trials, was deficient by 0,0002 inch; viz. two ten-thouſandths of an inch were to be added to each tenth of an inch meaſured by the micrometer, and ſo in proportion for a leſs quantity: but this correction is hardly worth notice.

	On the ſcale. Inches.
	Inch Inch
The interval of the croſs-wires in the microſcope and micrometer - - - -	27 and 32 = 5,0000
Interval of ditto on another part of the ſcale, viz. - - - - - -	26 and 31 = 5,0000
Ditto ditto - - - - -	25 and 30 = 5,0001 †

I therefore ſay, this interval was 5 inches correctly, to within leſs than the twenty-thouſandth part of an inch on this ſcale.

Meaſurement of the cube, viz. of the ſide 1 (ſee fig. 5.)

Inches.			Mean. Inches.
From *a* to *b* = 5—,0114	therefore	= 4,9886	= 4,98882.
a to *c* = 5—,0115	———	= 4,9885	
c to *d* = 5—,0105	———	= 4,9895	
b to *d* = 5—,0113	———	= 4,9887	

The ſide 2.

From *a* to *b* = 5—,0106	———	= 4,9894	= 4,98955
a to *c* = 5—,0098	———	= 4,9902	
c to *d* = 5—,0102	———	= 4,9898	
d to *d* — 5—,0112	———	= 4,9888	

Height of the cube, from ſide 1 to ſide 2.

From *a* to *a* = 5—,0110	———	= 4,9890	= 4,98925 †
b to *b* = 5—,0105	———	= 4,9895	
c to *c* = 5—,0107	———	= 4,9893	
d to *d* = 5—,0108	———	= 4,9892	

* One revolution of the ſcrew of the micrometer was $\frac{1}{100}$ inch.
Each grand diviſion, of which there were ten . . $\frac{1}{1000}$ inch.
Theſe again ſubdivided into five each, became . . $\frac{1}{5000}$ inch.
And half a diviſion, which is very viſible, is . . . $\frac{1}{10000}$ inch.

† It cannot eſcape notice, that all theſe meaſures were ſomething leſs than 5 inches, the quantity propoſed; it aroſe from this, Mr. Troughton informs me, he was more ſolicitous to obtain a true figure, than the exact ſize; neither of which, however, were very important, as both were to be proved by the mode I have adopted. What was important was to have the ſides true planes, and theſe were examined, as I am informed, by the reflected image of the moon ſeen through a large teleſcope, the focus of which would be altered, if the ſurface were either hollow or convex.

§ 13. Now

§ 13. Now the three foregoing mean meaſures of the ſide of the cube, multiplied into each other, will give $=124,18917$ cubic inches for the contents of the braſs cube, which muſt be very near the truth; for if not, let us ſuppoſe the error in taking each of theſe meaſurements to be half a thouſandth of an inch, which is much greater than is probable, viz. $=\frac{1}{10000}$ part of the ſide of the cube; and let us ſuppoſe each of theſe errors to lie the ſame way, which is alſo very improbable; in that caſe, the error in determining the ſolid content would be only $\frac{1}{10000}$ of the whole; in the above inſtance, about 0,03 cubic inch: but more probably the error does not amount to half this quantity.

§ 14. Examination of the cylinder.

The micrometer and microſcope of the divided ſcale (Plate V. fig. 1.) being removed till their croſs-wires were four inches diſtant, viz. from 54 inches to 58 inches, and the thermometer at 62°, I obſerved of the end or baſe of the cylinder, No. 1. fig. 6.

	Inches.	Inches.	Mean. Inches.
The diameter $a\ b$	$=4-,0027$	$=3,9973$	$=3,99745$
$c\ d$	$=4-,0024$	$=3,9976$	

End 2, of the cylinder.

	Inches.	Inches.	
The diameter $a\ b$	$=4-,0014$	$=3,9986$	$=3,99785$
$c\ d$	$=4-,0029$	$=3,9971$	

Height of the cylinder.

The microſcope and the micrometer being placed reſpectively at 52,1 inches and 58,1 inches, viz. at the interval of exactly 6 inches on the ſcale, I found

	Inches.	Inches.	Mean. Inches.
The height from a to a	$=6-,0049$	$=5,9951$	$=5,99502$
b to b	$=6-,0047$	$=5,9953$	
c to c	$=6-,0047$	$=5,9953$	
d to d	$=6-,0054$		
Repeated	$\left\{\begin{matrix}58\\56\end{matrix}\right\}$	$=5,9944$	

Now the mean diameter of the cylinder having being found

	Inches.
At the end 1	$=3,99745$
At the end 2	$=3,99785$
The factor for the ſquare of the diameter of a circle to find the area, being, as is well known.	$=0,7854$
And the height of the cylinder	$=5,9950.$

The above four quantities multiplied into each other, give for the contents of this cylinder in inches $=74,94823$; and this reſult may be taken at leaſt as correct as that of the cube, viz. to about the third place of decimals.

§ 15. Having adjuſted the beam of the balance (fig. 4.) with reſpect to the length of

of its arms, its centre of gravity, and the three points of suspension of the beam and scales, and having examined the weights, I proceeded to the remaining parts of this experiment.

September 2d, 1796. The balance-beam adjusted by the screw *p*, till the vibrations were so slow as to require more than 50 seconds of time for each, $\frac{1}{10}$ grain appeared to move the index through three divisions* of the scale *s* and *t*, $=\frac{1}{7}$ inch, when the beam was not loaded; but when the beam was loaded with 16384 grains, or near 3lb. troy, $\frac{1}{100}$ grain was equal only to 0½ division † of the same scale.

§ 16. September 4th. The thermometer being at 63°, and the barometer at 29,36 inches,

		Oz.	Grains.	Grains.
The weight of the counterpoise to the pan or scale for weighing the cube in air, was - - -	=	1	75,02	=555,02
To which the weight of the common pan, with the silk lines on the left arm of the beam, and marked with *x*, the common right-hand pan having been removed - - -		—	—	=413,40
And the whole weight of the pan or apparatus for weighing the *cube in air*, becomes - - -		—	—	=968,42
§ 17. The counterpoise to the pan or scale for weighing the cylinder in air, was found - - -	=	1	72,34	=552,34
To which add the weight of the common pan on the left arm, as before		—	—	=413,40
And the whole weight of the pan or scale for weighing the *cylinder in air* becomes - - -		—	—	=965,74

Note, in the preceding and such-like experiments, the common right-hand scale being removed, and the left-hand scale being always used and always the same weight, viz. 413,40 grains, when either the cube or cylinder, or any large body, is weighed, notice need only to

* Twenty divisions are=1,0 inch.

† That is, the beam was sensible with $\frac{1}{1600000}$ part of the whole weight. Mr. Harris's beam, with which he and Mr. Bird made their observations on the exchequer weights, turned with $\frac{1}{230000}$ part of the whole weight, and was consequently only one $\frac{1}{7}$ part so sensible as this. See "The Report of the Committee of the House "of Commons in 1758, to inquire into the original standards of weights and measures in this kingdom, and "to consider the laws relating thereto." See also a second Report in 1759; both of which contain a vast deal of useful information on this subject, extending through fifty folio pages, and are to be found in the 2d vol. of Reports, from 1737 to 1767. A bill was brought in, in consequence, but afterwards dropped; and it is much to be lamented that this inquiry did not go to the full length of an act of Parliament. Note farther, the largest of the beams, of which there are some of different sizes now made use of in the duchy court of Lancaster, for the actual sizing of the weights of the kingdom, is about three feet long, and is moveable with about 30 grains when 56lb. avoirdupois are in each scale, viz. about $\frac{1}{1000}$ part of the whole.—G. S. E.— An account of nine other balances may be seen in my *First Principles of Chemistry*, chap. VI.—N.

be

be taken of the counterpoise weight, viz. 555,02 grains, or 552,34 grains respectively; and these are to be deducted from the general amount of all the weights in the left-hand scale marked x; but it certainly would have been more convenient to have had single weights ready adjusted for these counterpoises, both in air and water. These, though at first omitted, have since been supplied.

	Grains.
§ 18. The counterpoise to the scale for the cube in distilled water, with the heat of 61° -	=442,75
To this add the weight of the common scale as before	=413,40
And we have the whole weight of the scale for the cube in water - - -	=856,15
But the weight in air having already been found	=968,40
The difference of the weights - - -	=112,25
Gives for the specific gravity of this brass -	= 8,62
§ 19. The counterpoise to the scale for the cylinder, in the same water with the same heat	=441,68
To this add the weight of the common scale as before	=413,40
And the whole weight of the scale for the cylinder in water becomes - - -	=855,08
Its weight in air has already been found -	=965,74
The difference of these weights - -	=110,66
Gives for the specific gravity of this brass -	= 8,78
The mean specific gravity of this brass, and brass-wire, may therefore be put about -	= 8,7

N.B. The tables of specific gravity, give that of wrought brass from 8,00 to 8,20. It was necessary to ascertain the specific gravity of the brass wire, to make the correction mentioned in the note to § 10; for as it was highly possible that in experiments with this hydrostatic balance, the scales for the cube and cylinder would occasionally be immersed to different depths in the water, and their weights would be altered as more or less of the wires, by which they were suspended, remained out of the water.

	Grains.
I accordingly found that 80 inches in length of this wire, used in the scales for the cube and cylinder, weighed in air . - -	=6,16

And consequently 1 inch would be =0,077 grains, and four wires of 1 inch =,308 grain, which divided by the specific gravity, viz. $\frac{308}{8,7}$, would give 0,0354 grain for the correction of every inch that the scale was sunk lower in the water; and so in proportion.

(To be continued.)

II.

Letter from Dr. BEDDOES *respecting Cit.* FOURCROY'*s Account of the Discoveries of* MAYOW. *To* MR. NICHOLSON.

SIR, May 10, 1799.

IT would be easy to write a comment upon Mr. Fourcroy's exposition of the merits of Mayow, as long as the exposition itself (see Annales de Chimie, No. 85); I wish, however, to submit only two or three remarks to that author's candour. From unremitting occupations of a more important nature, Mr. F. was, I suppose, prevented from bestowing much time upon Mayow. Marks of haste and of want of information are very apparent. I will not insist upon such an error as translating *taedae ferali—torche cruelle*, or upon the wrong assertion that I republished Mayow in 1790. In general, the French author has bestowed ample praise upon our countryman; but in the estimate of his understanding he has failed most materially. He has neither entered into his views nor rightly conceived the spirit with which Mayow laboured. Mr. F. says, that *the thread which he found soon broke in his hands*; *that he did not suspect the extent of the career which he opened*; *and that he was not sufficiently struck with the singularity and importance of his first discoveries* (p. 89). It would be extraordinary indeed, if a person who outstripped his age in a degree of which there is no other example in the history of science, had not been endowed with superior comprehension of mind. And Mr. F. is directly contradicted by Mayow's dedication. Never was a sense of the importance of a man's writings more fervently expressed, " *Quæ autem* " *de Nitro scripsimus, ea se per universam ferè naturam diffundunt! resque abstrusas explicant* " *quarum plerasque e numerosâ scriptorum turbâ vix quisquam attiget:* and so on. Had Mr. F. known that Mayow died at 27 or 28, he would not perhaps have talked of the thread breaking in his hands, and of his only opening the career. What Mr. F. advances concerning causes of the greater popularity of Boyle, appears to me ill founded. But I will not encroach upon your Journal, by any further observations, unless you or your readers desire it.

I am, Sir, your humble servant,

THOMAS BEDDOES.

III.

Observations and Experiments on various Saponaceous Compounds, particularly the Fish Soap of Sir JOHN DALRYMPLE. *By Mr.* ROBERT JAMESON *F.L.S. &c.*

SIR,

I INCLOSE you a paper on the making of Soap from Fish, a scheme which has been for some time the object of very considerable attention in this part of the island. As many seem to view it in a more favourable light than it deserves, I conceive it to be doing justice to the public, to publish, through the means of your excellent Journal, a fair statement of its plausibility. I am, with respect, Sir, your obedient servant,

ROBERT JAMESON.

Sheriff Bræ, *Leith, May* 13*th*, 1799.

To Mr. NICHOLSON.

On

On Soap, and particularly the Combination of Fish and Potash, called Fish Soap.

THE utility of this substance must have made it an object of early attention, for as mankind advanced in civilization, and associated in villages and towns, saponaceous matter became necessary, not only for the purposes of cleanliness, but for other cotemporary arts. Although this seems to be the probable origin of the manufacture, we have no written record of its use until the time of Pliny, who attributes its invention to the Gauls. Their soap seems to have been a compound of tallow and potash, for Pliny observes, that it was made of tallow and ashes, but the best of goats' tallow and the ashes of the beech-tree. Besides this soap, we have early accounts of the use of different detersive substances, particularly, ashes of vegetables, natron, and urine; several of the ancient writers mention saponaceous vegetables, that appear to have been used for cleansing clothes. Beckman, professor of economy at Gottingen, is led to suppose, on the authority of Strabo, that bran was employed by the Romans, for the purposes of washing such cloth as would be injured by the hard rubbing, and strong detersive power, of an alkali or common soap.

The Romans also used different natural combinations of the argillaceous earth, for the purpose of fulling. These were imported from different countries, as the Chia, Lemnia, Sarda, Samia, &c. They varied much in their detersive power: the best and most valued was named cimolia. This has been considered by several mineralogists, as the same with our fullers-earth; but Mr. Hawkins has lately discovered the true cimolia of Pliny, which is very different from the fullers-earth, upon the island of Cimolo, or what is now denominated Argentiera*.

During the course of ages, several alterations have been made in the art of soap-making; but these, like many other improvements, have occurred accidentally to the manufacturer, who in general was but ill qualified to make the best use of them. Their prejudices, and the vague general knowledge of the man of science, have been the greatest bars to improvement. Happily, circumstances are now altering, the artist and the philosopher are combining their efforts to improve the manufactures, and to establish such a connection among them as will be for their mutual advantage. The great institution now forming in London, will contribute very much to this end, and afford us another proof of the superiority of the British nation, in every thing regarding the improvement of manufactures.

The accounts which have been published of the process for soap-making, are in general vague, and often contradictory. The report in the 19th vol. of the Annales de Chimie, is the most complete treatise that has ever appeared upon this subject, It is a model which deserves to be imitated, not only for the accuracy of the details, but the comprehensive manner in which the whole is treated.

Different Kinds of Soap.

I cannot pretend to enumerate in this paper all the kinds of soap which are now manufactured, but will only mention the most important.

* Plinius hist. nat. lib. 35, cap. 57.—Agricola de naturâ Foss. lib. 2.—Lenz. Mineralogistes Hand-buch. Emmerling, Lehrbuch, vol. 3.—Klaproth. Beitrage.

In France, both hard and foft foaps are manufactured in very confiderable quantity, but the hard foap is the moft generally ufed. The hard foap is principally made from the oil of olives and barilla, and it is found that twelve hundred weight of oil, with a fufficient quantity of barilla ley, forms a ton of foap*. Manufacturers, however, do not always reft contented with fuch a return; but by ufing many unwarrantable practices, increafe the weight of the foap, without adding to its value as a cleanfer. Thus we are told in the French reports, that Quefnot has publifhed a method of increafing the weight of foap in a four-fold proportion. This he does by mixing with it alumen, muriate of foda, ftarch, chalk, foda in powder, oil, tallow, and water; and this ftupid procefs he announces under the title "*De belle augmentation de favon.*"

In France the foft foap is made with potafh and different vegetable oils, as oil of turnip, hempfeed, lintfeed, &c.

In Hungary, foap is made with tallow and natron, according to Townfon, who has given us a vague account of their method of manufacture. In many parts of Germany and Ruffia, tallow foap is manufactured; and in Ruffia a good hard foap is made with butter: the butter which is ufed for foap making, is in general damaged falt butter, which does not make a profitable foap; this is owing to its rancidity, and the quantity of falt and cheefy matter diffufed through it. Weigleb affures us that a foap is manufactured with yellow and white wax, which is hard and firm, with an agreeable fmell like almonds.

As no confiderable quantity of vegetable oil can be procured in Great Britain, our foap is always made with tallow, or fifh-oil; fometimes alfo, the refufe of kitchens and rancid butter have been ufed. The foaps manufactured in the great way are white, mottled, yellow, and foft foaps.

1. *White Soap.* This is made from tallow, with a ley of barilla, or kelp; the tallow is reckoned good if 13*cwt.* with the proper proportion of ley, affords a ton of foap. It is the moft expenfive of the hard foaps, and is principally ufed for the finer kinds of work.

2. *Mottled Soap.* This is the next in value, and is manufactured from tallow, kitchen-ftuff, and barilla. The mottled appearance is given, towards the end of the operations of boiling, by difperfing the ley through the foap. In France, they follow a method, which is more fimple, and furely more fuccefsful. Their method is as follows:—After the foap has been feparated from the wafte ley, it is boiled again with a frefh portion of cauftic ley, then a quantity of the folution of fulphate of iron is added, which by its decompofition, depofits its oxyd through the foap, giving it a bluifh colour. The foap is now allowed to cool flowly, when it rifes above the wafte ley; this ley is then drawn off, and the foap melted by itfelf, before cafting into frames.

3. *Yellow Hard Soap*, is made with tallow and rofin; and thefe are reckoned good, if 10*cwt.* of tallow, $3\frac{1}{2}$*cwt.* of rofin, with the proper quantity of barilla, or kelp ley, afford a ton of foap. The rofin is ufed merely to cheapen the foap, for it certainly does not increafe its power as a cleanfer. Rofin and alkali boiled together, without tallow, form a vifcid tenacious mafs, of a very inferior nature as a cleanfer.

* Annales de Chimie, vol. 19.

4. *Soft*

4. *Soft Soap*, is made with whale or fiſh oil; but I am not acquainted with the proportion uſed. The proceſs for making it differs from that of hard ſoap, in this circumſtance, that none of the ley is drawn off, but the whole is allowed to combine with the ſoapy compound. On this account, it is neceſſary to uſe a pretty pure alkali, particularly that it ſhould be free from ſea-ſalt, which is a ſubſtance of the greateſt detriment, as it cauſes the ſoap to ſeparate from the ley, and riſe to the top of the veſſel. The oil and alkali (which muſt be pot-aſh) are boiled together, until a combination has taken place; then a quantity of tallow is added, which, by particular management, is diſperſed through the ſoap, in the form of white ſpots, giving it a beautiful appearance *.

Having thus curſorily mentioned the principal kinds of ſoap manufactured in different countries, I will now, in as few words as poſſible, ſtate the proceſs followed in this country, for making common white ſoap. This will enable us to judge with more certainty of the plan lately propoſed for making hard ſoap, by mixing the muſcular fibre of fiſh with the tallow.

Proceſs for making White Soap.

The tallow is firſt melted in the veſſel with weak barilla ley, the mixture is then gradually brought to a boiling heat, and we continue adding alkali, until the whole of the tallow has formed a ſaponaceous compound. We know this has taken place, if the compound has a milky appearance, with a conſiderable degree of conſiſtence, and ſeems to be ſeparating from the liquor below. This ſeparation is a very material part of the operation; and to effect it completely, a quantity of ſea-ſalt, or weak kelp ley, is added, the materials are continued boiling for two or three hours, and then the fire is withdrawn. The ſoap will now be found united at the top of the liquor, or what is called the *waſte ley*†, which is of no further uſe, and

* I ſhould be happy to learn the proportions uſed in ſoft ſoap making, and alſo the particular management of the tallow in producing the white ſpots; and if this tallow be of any ſervice.

† The ley which is found below the ſoap, and known under the name of waſte ley, is of a very mixed nature, and the variety is much increaſed, according as potaſh, barilla, or kelp, are uſed in the manufacture. Several ſubſtances always occur, whatever be the nature of the ſaline compound employed. Theſe are,

1. *Gelatine.* A conſiderable quantity of this ſubſtance (which had been previouſly combined with the tallow) is found in ſolution, or ſuſpended in the ley: the cauſtic alkali has ſo ſtrong an action upon it, that a portion is decompoſed, the hydrogen and azote flying off in the form of ammonia, while the carbonaceous matter remains behind, giving a dark blue, or blackiſh colour, to the undecompoſed gelatine.

2. As tallow contains ſebacic acid, it unites with the alkali, and ſometimes with the lime, forming ſebats.

3. *Carbonat, or uncombined alkali.* However accurately the manufacturer works, ſtill a quantity of the alkali eſcapes, and is found in the waſte ley. Beſides the ſubſtances we have already mentioned to be common, other combinations occur. Thus if kelp be uſed alone, in making the ſoap, beſides the gelatine, &c. the ley contains muriate of ſoda, ſulphate of ſoda, ſulphurets of ſoda, and potaſh. As potaſh is never worked by itſelf in making hard ſoap, but generally with kelp (at leaſt in Scotland), we obtain different ſaline combinations; theſe are muriate of potaſh, muriate of ſoda, and ſulphate of ſoda; but this laſt is in ſmall quantity: ſulphuret of ſoda alſo occurs. If barilla is employed, we have muriate of ſoda, ſulphate of ſoda, and ſulphuret of ſoda, in the ley. The nature of the ſaline compounds found in barilla ley, are often conſiderably different from what has been now mentioned; particularly, when the ſoap is made with Catalonia barilla. This is owing

and is therefore to be drawn off by a pump or ſyphon. The ſoap is now melted for the laſt time, with a ſmall quantity of water, or what is better, weak kelp ley; it is then allowed to cool for a ſhort time, and afterwards caſt into frames. The melting with water is of conſiderable importance, as it gives the ſoap a ſmooth and compact appearance, or what the workmen term a ſkin.

Having premiſed this much with regard to the different kinds of ſoap, which are already known, I will now proceed to conſider the plauſibility of a late propoſal, for the manufacturing of ſoap with fiſh and tallow, or by uſing fiſh alone, diſſolved in potaſh.

Attempts have been made at different times to procure a ſubſtitute for tallow; but in no inſtance has it been attended with ſucceſs. Several years ago, when the Greenland fiſhery was more conſiderable than it is at preſent, ſoap manufacturers uſed a conſiderable quantity of oil, in making their hard ſoap; but the ſtrong oily ſmell ſoon deſtroyed its ſale, and ſeveral ſoap-makers were ſubjected to conſiderable loſs. It is to be regretted that this attempt was not attended with ſucceſs, particularly when we conſider the great advantages which would reſult from the increaſe of our Greenland fiſhery, by the diminution of the importation of tallow from Ruſſia*.

During the courſe of the laſt year, Sir John Dalrymple, Bart. one of the barons of exchequer, obtained a patent in the name of John Crooks, for a new method of making ſoap. He propoſed to uſe the muſcular fibre of fiſh in a certain proportion with tallow to make hard ſoap, and to ſubſtitute it for oil in the manufacture of ſoft ſoap†. As this ſcheme promiſed to be of the greateſt national importance, it was taken into conſideration by the Houſe of Commons. Soon afterwards, Sir John Dalrymple was examined by a committee of the

owing to a method they employ of adulterating it, by burning along with the Salſola ſoda different ſpecies of fuci. In London, a barilla is made by mixing together Alicant barilla and kelp; and this is termed Britiſh barilla.

* The importation of tallow from Ruſſia is ſtated to be about one million ſterling yearly.

† The diſcovery of the converſion of the muſcular fibre into a ſubſtance reſembling ſpermaceti, has afforded room for conſiderable ſpeculation, as to the probable advantages mankind may derive from it. The amazing number of animals killed in different countries, for the ſake of their ſkin and tallow, has been viewed as likely to afford a means of procuring this ſubſtance at a cheap rate. A more plauſible method of rendering this intereſting diſcovery of uſe, may be found in employing fiſh in place of the muſcular fibre of land animals. In Scotland particularly, this ſpeculation deſerves attention, as the quantity of fiſh which fills our ſea at different times of the year is very great. Thus the herrings, which yearly arrive upon the coaſts of Scotland, would afford a great ſource of animal matter for this purpoſe; alſo, the dog-fiſh, which are ſo obnoxious to fiſhers, might be taken in immenſe quantities; and after they had afforded their oil, might ſerve for the purpoſe of affording ſpermaceti. The coaſts of Scotland are alſo happily conſtituted for this branch of induſtry, owing to the number of running waters, which we find almoſt in every quarter; theſe might be eaſily turned in different directions, and employed to run over a great quantity of animal matter. The advantages attending a ſucceſsful cultivation of this diſcovery in the Highlands of Scotland, are numerous and important; but they cannot be detailed here. It is an object worthy the attention of the Highland Society of Scotland, which has already done much for the intereſts of that part of the iſland. If premiums were offered with this view, we ſhould ſoon have the whole tried experimentally.

House, when he detailed the value of the supposed improvement in such strong terms, that an order was given by the Lords of Trade and Plantations to have the whole determined by a skilful manufacturer, in the neighbourhood of some fishing station; and, in compliance with this order, the Honourable the Board of Trustees, at Edinburgh, requested my father to make the necessary trials, and report for their satisfaction. These experiments were made as soon as circumstances would permit; and the result has been lately laid before the board. As this report contains a satisfactory series of experiments, I will now lay before the public a few of them, with the view of setting this supposed improvement in a proper point of view.

Previous to this, I shall mention, in as few words as possible, the processes followed by Sir John Dalrymple in making hard and soft soaps *.

To make Soft Soap. The fish are first to be well washed in water, to free them from blood, &c. and are then to be thrown into a boiling solution of caustic potash, in the proportion of from six to ten parts of fish to ten of alkali. The fish being dissolved, tallow is then to be added in the proportion of one part to eight of fish. These being boiled together for some time, turpentine is added in the proportion of one part to sixteen of soap; afterwards, a 36th part of palm-oil is added, to give the soap a better colour. The soap being finished, it is to be spread about two inches thick in a cool place, and kept there for a month before being used; during this time, it must be turned every two or three days with shovels or hoes.

To make Hard Soap. Having dissolved a certain quantity of fish in caustic vegetable alkali, an equal quantity of tallow is to be added, and to be continued boiling, until they are completely united. The vessel is then allowed to cool, and mineral alkali, or mineral and vegetable alkali, and rosin are to be added: the alkaline ley in the proportion of five parts to twenty of fish and tallow. The whole is then to be boiled, and in an hour after the strongest waste ley is to be added in the proportion of two parts in weight to 25 parts. The whole is then to be boiled until the alkali has united with the vegetable and animal matters. The ley is now to be pumped off, and caustic mineral alkali added, in the proportion of three parts in weight to 25 parts; and the whole is to be boiled, until a good soap is formed: lastly, the soap is to be melted in water, and cast into frames.

We need not stop to examine the processes here detailed, as they must appear, from what has been already stated with regard to soap-making, to be very incorrect.

As the report given to the Board of Trustees contains a long series of experiments, it would occupy too much room to insert them at full length; I shall, therefore, only abstract those which appear to be the most important.

Hard Soap from Fish, &c.

Experiment I.

		Cwt.	*qrs.*	*lb.*
Herrings	- - - -	5	2	20
Tallow	- - - -	5	3	8
Rosin	- - - -	1	0	

* This is abridged from his patent.

These

Thefe fubftances were boiled together fo as to form a foap, and, after cafting into frames, afforded 13*cwt.* 2*qrs.* of foap. According to the returns mentioned in a former part of the communication, the tallow and rofin alone fhould have afforded 11*cwt.* fo that the fifh appears only to add 2*cwt.* It might be fuppofed that this 2*cwt.* 2*qrs.* of fifhy matter had combined with the tallow and alkali, forming part of the foap: it was otherwife, the greater part was of a black colour, and adhered to the under part of the foap, immediately above the wafte leys.

Experiment II.

	Cwt.	qrs.	lb.
Herrings - - - -	8	0	0
Tallow - - - -	4	0	0
Rofin - - - -	1	0	0

Thefe materials afforded 13*cwt.* 3*qrs.* 4*lb.* of foap: of this the tallow and rofin gives 9*cwt.* and the fifh 4*cwt.* The greater part of the fifhy matter was ufelefs, being nearly in a gelatinous ftate.

Obfervation.

In the art of hard foap making, it is known, that the addition of fea-falt, or weak kelp ley, is neceffary, to enable the foapy compound to feparate and rife to the top of the wafte ley. If we attempt to follow this practice with the combination of fifh and tallow, inftantly the fifhy matter feparates, and the tallow rifes to the top in the form of light yellowifh coloured foap. This fact fhows us the impoffibility of combining fifh and tallow to make hard foap.

Soft Soap made from Fifh, Tallow, &c.

Experiment I.

	lb.	oz.
Herrings - - - -	36	0
Tallow - - - -	4	11
Rofin - - - -	2	5½
Palm-oil - - - -	1	2¾

This gave 45*lb.* of foap of the confiftence of treacle. According to calculation, the tallow, rofin, and palm-oil gives 18*lb.* 12*oz.* and the fifh 27*lb.* The foap in this ftate is too foft for the market, fo that it requires to be deprived of a portion of its moifture to reduce it to the ftate of common foft foap, when it weighs only 33*lb.* It was found a difficult operation to effect an union of the fifh and tallow, as it required, not only a longer continuance of boiling, but alfo a greater proportion of alkali, than a fimple combination of tallow and rofin. Even after careful boiling, if the alkali contained the fmalleft portion of fea-falt, the fifh feparated from the tallow: this is a circumftance of fuch a nature, as to render working in the great way very precarious.

Many other experiments of the fame nature with the one now mentioned, are related in the report; but it will be fufficiently fatisfactory, to give the following table of expences, which is drawn up upon an average of all the trials.

Expence

Expence of manufacturing a Ton of Sir John Dalrymple's Soft Soap.

	Cwt.	qrs.	lb.	oz.		£.	s.	d.
Fish - -	18	2	0	0	Value	1	1	0
Tallow - -	2	0	10	0		5	17	0
Rosin - -	1	1	1	0		1	0	$1\frac{3}{4}$
Palm-oil - -	0	2	0	$3\frac{1}{2}$		1	11	1
Pot-ash - -	5	3	0	0		15	8	0
Lime - -	3	0	0	0		0	3	6
Expence of manufacturing						2	0	0
Firkins as packages						2	3	0
Duty						16	6	8
Profit						3	0	0
What the manufacturer can afford to sell a ton at						£.47	12	$4\frac{3}{4}$
Common soft soap a ton						48	0	0

From this calculation, we find only a very trifling difference of expence between the fish soap, and common soft soap: and experience has shewn me, that *1cwt.* of common soft soap is equally valuable with *1cwt. 2qrs.* of fish soap. This article, therefore, can never be brought into the market.

Sir John Dalrymple having found that the methods proposed in the patent were not likely to be attended with success, has adopted an old practice of the Swedish peasantry, in forming a soft soap, without oil or tallow, by dissolving fish in caustic potash. In the report, we have a number of trials upon this combination, with a view to ascertain the expence of manufacturing, and the following list of expences is drawn up from these trials.

Expence of manufacturing a Ton of this Soap; and what the Manufacturer can afford to sell it at.

	T.	cwt.	qrs.	lb.		£.	s.	d.
Fish - -	1	10	0	0	Value	4	0	0
Potash - -	0	6	0	0		16	16	0
Lime - -	0	3	2	0		0	3	6
Labour, fuel, &c.						1	0	0
Firkins as packages						2	3	0
Duty						16	16	8
Profit						2	0	0
						£.42	9	2

From the above statement it would appear that the fish soap would supersede the use of common soft soap. Experience however has shewn me that *1cwt.* of common soft soap will clean nearly as much linen as *2cwt* of this fishy combination. We must therefore conclude, that none of the proposed methods will be in any economical point of view of much consequence.

Having thus ftated the refults of the experiments laid before the Board of Truftees by my father, I will conclude this paper, with fome mifcellaneous obfervations upon the combination of fifh and potafh, and the relation of a few experiments on other faponaceous compounds.

When cauftic potafh and fifh are heated together, a decompofition of the animal matter foon commences, as is demonftrated by the feparation of a quantity of ammonia. The quantity of ammonia feparated or formed in this procefs is fo great, that fhould the fifhy compound prove of any ufe, it would be economical to collect the ammoniacal vapours in a chamber with muriatic acid gas, to form muriate of ammoniac. This ammonia is derived not only from the gelatine, but alfo from the pure fibrous matter, as I found by particular trials with it. This fifh foap has a more or lefs brown colour, varies in its confiftence, which depends on the quantity of water it contains; it has a moft difagreeable fmell, and by keeping in a damp place becomes fofter, and acquires a thin whitifh covering. The bony matter of the fifh is not diffolved, but remains fufpended in the foap; fo that when the foap is diffolved in water, the bony matter falls down. When an acid is added, the fifhy matter is feparated of a white colour, and remains diffufed through the water, giving it a milky appearance.

Sir John Dalrymple mentions in his printed letters, that from repeated experience, he has difcovered this foap to be poffeffed of peculiar properties, which he fays, independent of its cheapnefs, will render it an object of great national concern. The following experiments were made to try the truth of Sir John Dalrymple's ftatements, as alfo to obferve if this compound had fimilar properties with common foap.

1. With diftilled water it forms a brown-coloured folution, which being kept for feven or eight weeks, did not alter in its appearance or fmell. It alfo lathered pretty well.

2. With foft water it forms a brown-coloured folution, and lathers well.

3. With hard water it is changed in the fame manner as common foap, a part of the faponaceous compound being decompofed by the earthy falts, which are generally to be found in fuch kinds of water. Sir John Dalrymple, however, afferts, that it wafhes equally well in cold hard, as in cold foft water, and befides, that it wafhes with equal advantage although the water be not heated. The experiment which has been juft mentioned, fhews that it does not differ from common foap when ufed with hard water—that it wafhes equally well in cold as in warm water, does not require refutation.

4. If this foap be agitated in cold fea water, a part of it is decompofed by the earthy falts, contained in the water, and the water acquires a milky appearance. This decompofition is rendered the more evident, if the folution be kept for fome time, for it then acquires a moft difagreeable fmell, owing to the putrefaction of the animal matter, which had been feparated by means of the earthy falts. Sir John Dalrymple affirms, that this foap wafhes well in cold fea water, which he juftly obferves, is a difcovery of great importance. The milky appearance, and imperfect lathering which this foap exhibits when agitated with cold fea water, has probably led Sir John Dalrymple to draw this conclufion. Thefe appearances, however, are no proof of its poffeffing any greater cleanfing power than common foap with falt water.

Accordingly

Accordingly my father found that this assertion of Sir John Dalrymple, was without foundation.

In the 6th volume of the transactions of the London Society of Arts, mention is made of a soap, manufactured in India, which is said to wash with sea water. It would be very interesting to have the truth of this ascertained.

5. Chaptal, in the 21st volume of the Annales de Chimie, has described a method of making soap from wool, and he remarks towards the conclusion of his paper "This soap has been employed in every manner, and under every form, in my manufactory for dying cottons; and I am at present convinced that it may be substituted instead of the saponaceous liquor we make from lixivium of soda and oil, to prepare the cottons. I have constantly observed, that by dissolving a sufficient quantity of this soap in cold water to render the fluid milky, and by working the cotton with the apparatus, which is well known, it is sufficient to pass the cotton three times through, drying it each time, in order that it may be as well disposed to receive the dye, as that which has been passed seven times through the ordinary solution of soap. This will not appear surprising, when it is considered that animal matters are very proper to dispose thread and cotton to receive the dye, and some of the operations of our dye works consist simply in impregnating them with these substances *." In several trials which I have made with this fish soap, I find that it gives a greyish or brownish tinge to cotton cloth, which remains after repeated washing with common soft soap. If this be found to hold true in other trials, it is not improbable that this fish soap may be used in place of wool soap, in the processes mentioned by Chaptal. Several preliminary trials must be made, to determine whether the Gallic acid decomposes this compound, and if the astringent principle will combine with the fishy matter; at least, this is necessary in the process for dying cotton red. (See Chaptal's paper, Annales de Chimie, vol. 26th.)

Wool Soap with Tallow.

Having observed from the preceding experiments, that the muscular fibre of fish does not unite with tallow in making hard soap, I imagined that other animal substances might possess the same property. Accordingly I made several experiments, with the view of uniting the woolly matter with tallow.

Experiment 1. A quantity of white woollen cloth was dissolved in caustic potash, then a considerable portion of tallow was added, and the whole boiled (with a fresh addition of ley) until a brownish coloured soap was formed.

2. A quantity of wool soap which had been made about a year before, and still retained a disagreeable smell, was boiled with tallow, and formed a soap.

3. A quantity of wool soap was boiled with whale oil, and afforded a soap.

4. A quantity of soap made with wool and tallow was boiled with water; sea salt was then added, and the boiling continued for some time; the tallow soon separated, and rose to the

* Philos. Journal, I. 43.

top,

top, in the form of white ſoap : below in the waſte ley was the brown-coloured woolly matter. This experiment ſhews the impoſſibility of combining woolly matter and tallow to form hard ſoap. I next endeavoured to combine glue with tallow, and to form a hard ſoap.

Glue Soap.

I took a quantity of common joiner's glue, and boiled it with cauſtic potaſh, until it was diſſolved; during the boiling, a conſiderable quantity of ammonia was ſeparated, which gave the combination a pretty dark colour. This compound was again boiled with a freſh portion of cauſtic potaſh, along with a quantity of tallow; and the boiling was continued until, upon cooling, a fine yellow-coloured ſoft ſoap was formed. This ſoap had a very ſtrong ſmell, ſomewhat like putrefying fiſh. When it was boiled with ſea ſalt, to form a hard ſoap, a decompoſition was immediately effected; the tallow roſe to the top, in the form of white ſoap, and below in the waſte ley was the glue of a brown colour and curdled.

Peat Soap.

Having found from the preceding trials, that neither the muſcular fibre, glue, or wool, would remain united with tallow in the common proceſs of hard ſoap making; it occurred to me, that different vegetable ſubſtances might be uſeful. Accordingly I endeavoured to combine peat and tallow together, ſo as to form a hard ſoap. I diſſolved a quantity of peat in cauſtic potaſh, and continued the boiling until the compound had acquired the conſiſtence of common ſoft ſoap. In this ſtate it was ſoluble in water, and lathered with it. With tallow it formed a brown-coloured ſoap, which was not decompoſed by boiling with ſea ſalt. I was in hopes, from this circumſtance, that it might enable us to introduce an uſeful manufactury into the Highlands of Scotland, where peat is to be got in great quantity. I found, however, that it gave a brown colour to cloth which was waſhed with it, ſo that my attempt at rendering it uſeful was fruſtrated.

Preſervation of Animal Subſtances by Alkaline Solution.

Sir John Dalrymple, in his examination before the Houſe of Commons, claimed, as a diſcovery of the greateſt importance, the method of preſerving fiſh by immerſing them in a ſolution of alkali. This is ſomewhat ſurpriſing, for we find this fact mentioned long ago, and in different publications. Thus, in one of the early volumes of the Tranſactions of the Royal Society of London, ſeveral experiments are related, on the power of lime water in preſerving animal ſubſtances, and it is remarked, that fiſh immerſed in lime water, were preſerved freſh for a conſiderable time. Sir John Pringle alſo made ſeveral experiments on the antiſeptic power of cauſtic alkalies; and more lately, in one of the volumes of the Tranſactions of the Royal Iriſh Academy, there are ſeries of experiments on the antiſeptic power of cauſtic alkalies.

Sheriff Bræ, Leith,
May 11th, 1799.

ROBERT JAMESON, F.L.S. &c.

IV.

Experiments and Obſervations to prove that Snow does not contain Oxygen, either in Solution or in Combination, and that its fertilizing Quality does not depend on this Cauſe. By Dr. Joachin Carradori de Prato *.

THE generally-received opinion, that ſnow produces fertility, is true, becauſe it is proved by experience; but I do not think it can be admitted in the ſenſe which is commonly received. I am well convinced that ſnow produces this effect, but I conceive, as many others have alſo done, that it acts in a negative manner only, by defending the plants in winter from a degree of cold below that of freezing, and not, as is commonly thought, from its imparting any principle of fertility. But Citizen Haſſenfratz at preſent thinks he has confirmed this laſt opinion, by a diſcovery of the cauſe of the phenomenon, which he thinks muſt be aſcribed to a quantity of oxygen in combination in the ſnow, which it afterwards communicates to the ſeeds which are developed when it is converted into water, and he undertakes to prove by ſeveral experiments, that ſnow water contains much oxygen in the ſtate of combination †.

I ſhall prove by inconteſtable facts, that ſnow water does not contain pure oxygen in the ſtate of ſolution or aggregate, nor in the ſtate of fixity or combination; and conſequently that the ſnow cannot impart fertility by virtue of this principle.

In the month of Auguſt of the preſent year, I took the pureſt ſnow I could find, and after having waſhed it, I filled a ſmall glaſs bottle with a long neck, and when it began to diſſolve, I covered it with very pure oil olive, in order that it might not abſorb any air from the atmoſphere. About ſixteen hours afterwards, I firſt removed the whole of the oil from its ſurface, then threw in a ſmall fiſh, and immediately afterwards covered the water with freſh oil. When the fiſh entered the water it began to ſtruggle, and died almoſt inſtantly. I threw in another with the ſame precautions, and it alſo died. Laſtly, I took a portion of water of the ſame ſnow, which had been kept for the ſame time expoſed to the air, in a receiver with a large mouth, and poured it into a ſmall glaſs bottle equal to the former. Into this I threw a fiſh of the ſame ſize as before, and immediately covered the water with oil olive; but in this water the fiſh gave no ſigns of uneaſineſs, and lived quietly for more than three quarters of an hour. The thermometer ſtood at nineteen degrees of Reaumur during theſe experiments, and the barometer at about 27 inches and a half.

As I have elſewhere proved ‡ that fiſh by their reſpiration in water, have the faculty of abſorbing all the oxygen it contains, and that they immediately die in water which is entirely deprived of oxygen: I inferred from theſe experiments, that ſnow water does not contain oxygen in a ſtate of ſolution.

* Journal de Phyſique, V. 226.

† Journal Politechnique, IV. Cahier; of which ſome account is given in our Journal, I. 144.

‡ Annales de Chimie, et Hiſt. Nat. de Pavie, tome 5 et 14.

In

In order to confirm my deduction, I immediately poured the water, in which the fishes had died, into a receiver, which presented a large surface to the air, and, a few instants afterwards, I threw in a fish of the same species, which lived very well in it, and might have remained as long as I chose. It is, then, an incontestable truth, that the mere want of oxygen was the cause of the death of the two fish thrown into the snow water in the first experiment; for it is very evident, that when the snow water was placed in a situation to re-absorb the oxygen, of which it had been deprived by its congelation, it became as capable of maintaining the life of fishes as any other water.

But it seems certain, from several experiments, that snow water re-absorbs oxygen from the atmosphere more slowly than other waters which were deprived of it. I have before remarked, that snow water after having been exposed during sixteen hours to the air, contained so little oxygen, that it could scarcely support the respiration of a small fish for an hour; whereas I knew from other trials, that waters commonly contain a sufficient quantity of oxygen for the respiration of a small fish in the same circumstances for several hours. It seems proper, therefore, to conclude, that the water by its conversion into snow loses part of its disposition to absorb the oxygen of the atmosphere. But in order to determine this more accurately, I chose to make some trials.

I exhausted all the oxygen contained in two pounds of well water, by the respiration of a fish which was kept in the water included in a narrow-necked bottle until it died. When the fish was dead, I cleared the bottle very exactly of the whole of the oil, and poured the water into a vessel with a large aperture, in which I kept it exposed to the air for sixteen hours. I then returned the water into the same bottle, introduced a fish of the same size and kind, and immediately covered it with oil. At the end of four hours the fish was still alive, but, after half an hour longer, it died in convulsions, in the same manner as fishes ordinarily die for want of air. Well water, therefore, though totally deprived of oxygen like snow water, that is to say, to the point of being incapable of maintaining the respiration of fishes during the same time, did re-absorb more oxygen than snow water.

This cannot be attributed to the quantity of foreign matters usually contained in snow water, on the supposition that they either hinder the water from re-absorbing oxygen, or the fish from extracting it by respiration; for I have found that fishes live for several hours in turbid water exposed to the same circumstances as the snow water: and I have also proved that this water, after having been deprived of oxygen, re-absorbs it in less time than snow water.

Nevertheless, snow water, after a long interval of time, becomes again charged with all the oxygen it can contain, and becomes capable of maintaining the respiration of fish, like every other water. In the month of September, I kept clear snow water for five days in a bottle; and in order that the oxygen might insinuate itself more easily, I first filtered it through a paper, and afterwards agitated it every day in the receiver. This water so treated, absorbed as much oxygen as it was capable of holding; for when it was introduced into a small bottle with a narrow neck, it supported the life of a fish for nine hours, which was introduced and the the wate covered with oil.

But

But ſnow water does not even contain oxygen in a combined ſtate, as Haſſenfratz pretends. I have proved this by the moſt decided experiments. If melted ſnow be oxygenated water, containing oxygen in the ſtate of combination, it will follow that when expoſed to the ſun it will emit the oxygen again in the gaſceous form, by the combination of caloric and light, preciſely in the ſame manner as it eſcapes and ſhews itſelf when the nitric or oxygenated muriatic acids are expoſed to the ſolar light. But certainly ſince ſnow water does not contain oxygen in ſolution, but can abſorb it ſucceſſively as often as it is expoſed to the atmoſphere, it muſt at leaſt, if this principle do exiſt in ſnow, contain it in the aggregate ſtate. I therefore, in the month of September, in the preſent year, put very pure ſnow in ſmall pieces into a ſmall glaſs bottle with a narrow neck, and before it was entirely melted, I poured oil upon its ſurface, in order to prevent the communication with the air, and afterwards expoſed it for three ſucceſſive days to the ſun; ſo that the whole time of expoſure amounted to eighteen hours; and afterwards, having well cleared it of the oil, I introduced a ſmall fiſh: it died inſtantly, as in the ſnow water immediately after liquefaction. And notwithſtanding the long expoſure to the ſun, which rendered it very perceptibly warm, though I obſerved it with every poſſible attention, I never perceived the ſmalleſt bubble or indicatio of oxygen. Conſequently it cannot be admitted that this water was charged with oxygen in combination, becauſe it muſt have eſcaped in the form of gas; and if it had afterwards abſorbed it, the fiſh, which is the ſureſt indication of the ſmalleſt quantity of oxygen in water, would have availed itſelf of it, and lived for ſome time.

The experiments of Haſſenfratz, which ſeem to prove by analyſis the preſence of combined oxygen in ſnow water, are not, in my apprehenſion, concluſive, becauſe they are not confirmed by ſyntheſis. It is not an excluſive property of water ſaturated with pure oxygen in a combined ſtate to alter the tincture of turnſol to a red colour, and to precipitate the ſolution of ſulphate of iron.

It has already been obſerved by Bergman, in his analyſis of waters, that ſnow recently liquefied is abſolutely without air; but his aſſertion is not well proved, becauſe the method he made uſe of to aſcertain the exiſtence of vital air, or oxygen in water, namely, boiling, is abſolutely imperfect. Water retains its oxygen too ſtrongly, to allow its being extracted by boiling, as this experiment would require. I filled a ſmall matraſs with well water, and boiled it for an hour and a half, and upon taking it from the fire, I poured it out beneath oil, in order that no air might enter. When it was cold, I took off the oil and put in a fiſh, immediately after which I covered it again with oil-olive. Notwithſtanding the boiling, the fiſh found a ſufficient quantity of oxygen in this water to enable him to live more than three hours.

Freſh water fiſh are true eudiometers for water, for when they reſpire, they have the faculty of abſorbing all the oxygen it contains. From this property, natural philoſophers may have recourſe to them for meaſuring the quantity of oxygen contained in different waters, by taking fiſh of the ſame ſpecies and ſize, and obſerving the time they live in equal quantities of the water. In this way a ſeries of experiments might be inſtituted, to determine the true proportions of oxygen contained in different waters; as has been done by Fontana, though by the uncertain method of boiling. But it will be neceſſary to take into conſidera-

tion the variations of the barometer and thermometer, becaufe I have obferved that water contains lefs oxygen in fummer than in winter; and when the barometer is high (qu. low?) than when low, the reafon of which is fufficiently evident.

Bergman affirms, that fnow affords an indication of nitrous acid. If this be true, it accounts for its effect in burning leather, and other fubftances plunged therein; as Haffenfratz has remarked, who afcribes it to the combined oxygen, with which he thinks it is faturated. Is it probable that this acid fhould be formed at the moment of the congelation of the water, by the concentration or mechanical approach of the parts, caufed by the cold in the oxygen gas, with which the water is charged, and the azotic gas which may be afforded by the atmofphere?

Let this be as it may, a queftion will prefent itfelf concerning this commonly fuppofed activity of fnow, namely, how it can, if reduced according to the affertion of Haffenfratz, into water poffeffing the fame qualities, prove beneficial for developing the tender embrios of vegetables?

It feems, therefore, that from the whole confideration of the fubject, we are juftified in concluding, that there is no reafon to believe that fnow communicates any pofitive fertility to the earth; its good effects muft be attributed merely to the fimple prefervation of plants from intenfe cold, which, by altering their organization, would deftroy the powers of life.

V.

On certain Properties of Strontian and Barytes. By Citizen VAUQUELIN.

SINCE the method of obtaining barytes and ftrontian in a ftate of perfect purity has been difcovered by chemifts, they have obferved feveral properties in thefe earths, analogous to thofe of the alkalis, fuch as the acrid burning tafte, folubility in water, and feparation in cryftals, together with the change which they produce in the blue colours of vegetables, which they convert into green.

I fhall, on the prefent occafion, exhibit fome other of their properties, which I apprehend may ferve to fhew their ftill more ftriking refemblance to the alkaline fubftances.

About a year and a half ago, upon analyzing a filiceous fulphate of barytes, I found, after having decompofed it with charcoal, that the greateft part of the filex had been diffolved by the acid I made ufe of to decompofe the fulphate which had been formed.

I could not immediately determine the caufe of this unexpected event, but merely fuppofed the barytes to have produced it.

I have fince made fome more direct experiments, to elucidate this queftion, the refult of which fhews that my fuppofition was not without foundation.

Experiments on Strontian.

Experiment 1. Two hundred parts of ftrontian in fine powder, mixed with fixty parts of filex alfo powdered, were fubjected for an hour to a ftrong fire in a crucible of platina; the

the product was a grey fonorous mafs, cracked in various places, the parts of which adhered together with confiderable force. In this ftate, it had no very evident tafte, but when pulverized it was flightly cauftic; when put into water, whether in the lump or in powder, this compound neither produced heat nor fwelled up, as is the cafe with the pure ftrontian. It only became fomewhat whiter.

Experiment 2. The fubftance laft fpoken of was pulverized and boiled in water, which diffolved it much lefs abundantly than pure ftrontian, but the water acquired a flight alkaline tafte, and foon became covered with a white pellicle: it did not afford cryftals. When faturated with nitric acid, this folution afforded, by evaporation, a jelly in confiderable abundance.

Experiment 3. Another portion of the fame matter pulverized and moiftened with a little water, was totally diffolved, by the muriatic acid, and the folution afforded by evaporation a very abundant jelly, which, after wafhing and drying, prefented all the characters of filex. The nitric and acetous acids produced the fame effect on this fubftance.

Experiment 4. Five parts of pure ftrontian in fine powder, and one part of alumine, recently prepared, and ftill humid, were heated together with water, and when the liquor had fully boiled, it was filtered, and contained much undiffolved matter. The filtered liquor had a flight alkaline tafte, but did not cryftalize, though a much greater quantity of the earth had been ufed, than could be diffolved without heat.

This folution was then faturated with muriatic acid, and being afterwards mixed with ammoniac, afforded a fmall quantity of flocculent matter, which was alumine.

Strontian confequently poffeffes the property of favouring the folution of alumine in water; but what is more remarkable is, that the alumine, on the other hand, rendered a great quantity of the ftrontian infoluble; for the water did not, in this cafe, diffolve one-tenth part of what it would elfe have taken up.

The examination of what remained on the filter, proved that the ftrontian became infoluble, by an intimate combination between thefe two earths; and it is probable, that if a greater quantity of alumine had been prefent, there would not have been a particle of mere ftrontian diffolved.

The refidue was, in fact, foluble in acids, with fcarcely any effervefcence; its folution afforded a flocculent precipitate of alumine, by means of ammoniac; and the fupernatant liquor formed a very abundant depofition, when carbonate of potafh was added.

It will not, therefore, be furprifing, if a combination of thefe two earths fhould, hereafter, be found in nature.

Concerning Barytes.

Experiment 1. One hundred and fifty parts of cauftic barytes were mixed, as accurately as poffible, with fifty parts of filex, and the whole was afterwards ftrongly heated for an hour and a half in a crucible of platina. The mafs was in one fingle piece, cracked in various places, but without the cohefion obferved in the experiment with ftrontian. Its colour was a light apple green, its tafte almoft mild, and it did not heat with water, whether it was immerfed in lumps, or in powder. It preferved its green colour in the water.

Experiment 2. The pure nitric, muriatic, and acetous acids totally diſſolve this ſubſtance. Its ſolution, in any one of theſe acids, immediately affords a flocculent precipitate by ammoniac. Theſe ſolutions become gelatinous by evaporation, and when completely dried, the ſilex is again developed in poſſeſſion of all its properties. It cannot, therefore, be doubted, but that barytes, as well as ſtrontian, poſſeſſes the property of combining with ſilex, and of rendering it ſoluble, even in the weakeſt acids.

Experiment 3. Nine parts of cauſtic barytes were mixed with one part of alumine, newly ſeparated from its ſolvent, and ſtill moiſt; and the whole was ſubjected for a quarter of an hour to ebullition, with a ſufficient quantity of water. Much undiſſolved matter remained. The filtered liquor had a ſlightly cauſtic taſte, rather ſtronger than that of ſtrontian treated in the ſame manner. It ſoon became covered with a white cruſt by the abſorption of carbonic acid from the atmoſphere; but it did not cryſtalize, though the quantity of water was not ſufficient to have prevented that effect, if the alumine had not been preſent.

A drop of muriatic acid poured into a glaſs of this ſolution, produced a flocculent cloud, which was rediſſolved by agitation. A ſecond and third drop produced the ſame effect, until the greateſt part of the barytes was ſaturated; but at length the precipitate being no longer diſſolved by the motion impreſſed on the fluid, an exceſs of acid diſſolved it. When the fluid was entirely ſaturated with acid, the flocculent matter was again reproduced by ammoniac; and when this laſt addition ceaſed to produce any effect, a very abundant precipitate was afforded by the carbonate of potaſh.

We ſee, therefore, that barytes, as well as ſtrontian, diſſolves alumine, and even more abundantly; nevertheleſs, there ſtill remained in the reſidue a portion of alumine and barytes, which were not diſſolved, and appeared to be in a ſtate of intimate combination.

Suſpecting that this more abundant ſolution of alumine by barytes, than by ſtrontian, might be owing to a greater quantity of barytes having been uſed in this experiment than of ſtrontian in the other; I made a ſecond, in which I mixed equal parts of barytes and alumine, and boiled them as before. The liquor ſtill gave ſigns of an abundant ſolution of alumine and barytes, and about half the matter remained in the form of a white powder inſoluble in water, in which the acids demonſtrated the preſence of theſe two earths in combination.

The ſame effects take place between ſtrontian and alumine, and it is not ſurpriſing that this property extends itſelf to the alkalis; for when alumine is precipitated by potaſh, and rather too much of this ſubſtance is added, the earth always retains ſome traces of the alkali, however carefully it may be waſhed. Beſides which, we have examples enough of glaſs, which becomes ſoluble, or inſoluble, according to the quantity of alkali; and a ſtill more ſtriking inſtance is afforded of potaſh in hard ſtones, which cannot be ſeparated by any mechanical means.

The truth of what is here advanced, may be very ſimply ſhewn, by pouring a ſaturated hot ſolution of barytes into a ſolution of the muriate of alumine. A precipitate in flakes will firſt be found, which will be rediſſolved by a new quantity of the ſolution of barytes; and if before this precipitate be entirely rediſſolved, it be ſeparated from the fluid, it will be found to be compoſed of alumine and barytes.

Barytes

Barytes and Oil.

Experiment 4. It is known that when a solution of barytes or strontian is poured into a solution of common soap, a very abundant deposition is formed, which is a combination of the earth with the oil of the soap, while the alkali remains pure and caustic in the fluid. In order to ascertain whether these earths could unite directly with oil, I boiled a solution of barytes, made with heat, together with oil olive; the oil soon became consistent, the water lost its alkaline taste, and the new combination exhibited the taste and smell of a true soap, from which it differed merely in its want of solubility in water.

Barytes and Animal Matter.

Experiment 5. I was desirous of knowing whether barytes would act upon animal matters, in the same manner as the alkalies, which last are known to decompose them, and form a kind of soap, while ammoniac is disengaged. I therefore mixed twenty drams of bullocks' liver pounded with ten drams of barytes diffused in water, and boiled the whole in a retort, to which a receiver was applied. In a very short time, I obtained a large quantity of ammoniac, and the animal substance was converted into a sort of clotted magma of a rose colour, insoluble in water, and which, by several experiments, I ascertained to be a combination of fatty matter and barytes. It is, therefore, proved that the alkaline earths exert the same action as the alkalies upon animal matters, though with a slight difference in the result.

Conclusion.

It is evident from the preceding facts, 1. That strontian and barytes possess nearly the same habitudes as the alkalies, with regard to silex and alumine, like which substances they combine with the earths, and divide them in such manner, as to render them afterwards soluble in the weakest acids. 2. That they may be used like the alkalies, in the analysis of hard stones, which are not attacked by acids; a property which may be advantageously applied in such cases as do not allow the exhibition of alkalies. 3. That it is necessary to be careful in decomposing the nitrates of barytes and strontian in crucibles of earth, in order to obtain those bases in a state of purity, not to apply too strong a heat, for fear they should combine with silex and alumine. 4. That it was, no doubt, owing to combination with the earth of the crucibles, that barytes was formerly obtained in the state of a frit insoluble in water, and that the powder of charcoal, at present used in expelling the carbonic acid, has no other effect than that of removing the earth from the contact of the crucible. 5. That barytes and strontian ought henceforth to be separated from the class of earths, and inserted in that of alkalies, with which they have many more properties in common. This has already been done by Citizen Fourcroy, in his new work. 6. That it is very probable, we shall hereafter find combinations of barytes and strontian with silex, and perhaps with alumine.

VI.

Concerning those Perpetual Motions which are producible in Machines, by the Rise and Fall of the Barometer or the Thermometrical Variations in the Dimensions of Bodies.—(W. N.)

IN a former communication, I have given an account of fome of the delufive projects for obtaining a perpetual motion, from an invariable power *. In that paper I remarked that the flow of rivers, the viciffitudes of tides, the variations of winds, the thermometrical expanfions of folids and fluids, the rife and fall of the mercury in the barometer, the hygrometric changes in organized remains, and every other of thofe mutations, which never fail to take place around us, may be applied as firft movers to mills, clocks, and other engines, and keep them going till worn out. Many inftances of this kind of perpetual motion are feen in water-mills, and other common engines, which are neceffarily confined to certain local fituations.—The wind-mill, though lefs confined with refpect to place, is the fubject of a much more variable power; other inftruments, ftill lefs confined with regard to fituation and expofure, have been made, which are capable of continuing their motion without ceafing. Such was the clock, or perpetual motion, in Cox's mufeum, which was fhewn about twenty years ago in London. My former paper was written to fhew the value of the perpetual motion, ftrictly fo called, which has for the moft part been purfued by men of little information. In the prefent memoir, I fhall endeavour to afcertain that of this fecond kind of motion, which, becaufe more promifing, and of nearly the fame apparent practical value, has been followed at fome expence by men of higher claims. For this purpofe, I fhall firft defcribe a few fchemes, and then inveftigate the quantity of power they are likely to afford.

Fig. 1, Plate 6, is a fketch of the firft mover in a clock, which formed part of Cox's mufeum, which was fold by public lottery, about the year 1776, if my recollection be accurate. A B reprefents the furface of the mercury in a barometer, the glafs veffel of which had the form of a bottle or chemical matrafs. The diameter of the upper furface of the mercury was, I think, about twelve inches. C D reprefents the bafon or receptacle, into which the aperture of AB was plunged. I fuppofe, of courfe, that the lower furface of mercury, which was expofed to the preffure of the atmofphere, was nearly the fame as the upper A B, as in fact it appeared to be. From the intervention of the cafe, and other parts of the apparatus, I could only conjecture the manner in which the effect was produced; but this was afterwards explained to me by Mr. Rehe †, who contrived and made it. The bafon C D is fufpended by two chains K L, which pafs over the pullies or wheels H I, and are attached to the frame E F; which laft is fixed to the barometer A B. Let us now fuppofe the apparatus to be at liberty, and it will be clearly feen, that if the two maffes attached to the oppofite ends of the chains K L be not precifely equal, the heavieft will defcend, and caufe the lighteft to rife.—The maffes muft, therefore, be brought nearly to this ftate of equality, by the adjuftment of weight added to one or both of them. In this ftate, fuppofe the preffure of the atmofphere

* Philof. Journal, I. 375.

† This gentleman is at prefent one of the board of infpection of naval works at the Admiralty.

to

to increase, and the consequence will be, that a portion of the mercury being forced from the vessel C D into A B, will render this last heavier, and cause it to descend; while C D at the same time rises. And on the other hand, when, by a diminished pressure of the external air, the mercury subsides in A B, the vessel C D will preponderate, and A B will rise. Now the frame E F, which is interposed between the barometer and the pullies I K, is jointed at the corners and also at the places where it is attached to the chain and the barometer; and the inner edges of the upright pieces E, F are formed into teeth like those of a saw, the slopes of which lie in opposite directions, as is shewn in the figure. The wheel G, which is placed between these bars, is also toothed in the same manner; and its diameter is such, that when the teeth on one side, as for example E, are engaged, those on the other side, F, may be free; but it is too large to admit of both sides being disengaged at once. The wheel G is prevented by a click from moving in the direction opposite to that which may be produced by the action of the bars E and F. Hence the play of the machine is evident. When the pressure of the atmosphere diminishes and the barometer rises from its cistern, the side E of the frame will move the wheel G through a greater or less space, according to the variation; and when, on the contrary, it falls, the teeth E will be drawn out of their bearing, and those of F will be thrown into the wheel, and still produce a motion of the same kind; the joints of the frame E F allowing it to change its figure enough for this purpose. It is hardly necessary to remark, that this wheel G being connected with the clock, serves to wind it up, and that the clock is constructed to go for a much greater number of days than the barometer has ever been known to remain stationary.

The ingenious mechanic will readily form a notion of many other methods, of applying the variations of the barometer to similar objects. The wheel-barometer of Robert Hook, as well as another contrivance, in which the barometer and its cistern are placed at the different extremities of an inclined lever, may likewise be used for this purpose.

Several artists have exerted their industry, in attempts to apply the variations produced by change of temperature in bodies as a first mover. If a thermometer be suspended by its centre of gravity is such a manner, that the tube may lie nearly horizontal; the daily variations in the bulk of the mercury will cause a preponderance on the one side or the other, accordingly as the temperature is higher or lower, than it was at the original fixing of the centre of suspension. The thermometer may contain mercury or any other fluid, or it may consist of air confined by mercury, as in the manometer. In this contrivance, the great and frequent ranges of variation affords much promise of utility. The limits of convenient or practicable power from change of equilibrium in a fluid thermometer, will hereafter be examined. A much greater force seems to offer itself, in the power by which the expansion is produced; but the difficulty of forming a piston or other apparatus for confining fluids, will probably constitute an insurmountable impediment to this method.

The solid thermometer does not present the same difficulty. Fig. 2, represents a series of expansion-bars, each consisting of a plate of brass, soldered to another of steel, and possessing the property of bending by change of temperature, according to the laws already explained in this work.

work *. If the steel face of C A be uppermost, and the end C be fixed to C B, the extremity A will rise from B, when the temperature is elevated; and if the succeeding bars be similarly fixed above each other, as in the figure, the whole system will occupy a greater length, or elevation, above C B, when heated, than when cold. Another more convenient method of disposing the bars is shewn in fig. 3; in this, the bars are fixed together at the middle, with the brass faces turned towards each other. Each bar has a slight curvature (much less than is here shewn), which will be increased by heat, and by that means cause the distance between the middle of two extreme bars to be greater than it would be at a lower temperature.

These causes of action may be applied to machinery by various contrivances, some of which serve to increase the length of range, but add nothing to the power. This last, no doubt, is an object of convenience, according to the effect intended to be produced. The only method of adding to the power will consist in increasing the number of the bars. Fig. 4, represents a system for this purpose, which is the simplest and most convenient that has occurred to me. A C represents the circumference of a barrel, resembling those in which the main springs of clocks are put; the length and diameter of which may be varied, according to the power intended to be gained. To this external part is fixed a ratchet-wheel to receive the click C, which confines its motion to one direction. At A is fixed a plate to receive the action of the expansion-pieces. B D is an internal cylinder of the same kind, which is also confined by a ratchet-wheel and click to move only in the same direction as the outer part A C. It is not necessary to describe the operative arrangements, by which these two cylinders are disposed, so as to move on the same axis, and the ends duly applied, so as to form one box; while the interior and exterior parts allowed to move independent of each other. At B is fixed a plate, by which the action of the expansion-pieces is communicated to the inner cylinder. A series of bars, similar to those delineated in fig. 3, are disposed in the space between the two cylinders, the greatest part of which they occupy, leaving only such an interval between A and B, as may be sufficient to allow for the motion of the bars. In this interval is placed a spring, tending to cause A and B to recede from each other: and lastly, there are side-pins proceeding from the places of junction of every pair of bars, which respectively pass through circular grooves in the caps, and prevent the motion of the bars from being interrupted or impeded by their touching either the inner or the outer cylinders. E represents a wheel, which is supposed to be connected by tooth-work, or otherwise, with the face of the external cylinder, and may be considered as the machinery intended to be moved. Or otherwise, if the clicks C and D, with the teeth they act upon, be reversed, and the interior cylinder be fixed to the axis itself, that axis may be used as the first mover.

* Philos. Journal, I. 62. 576.

(To be concluded in our next.)

Chemical

VII.

Chemical Confiderations on the Ufe of the Oxydes of Iron in the dying of Cotton. By J. A. Chaptal.

(*Concluded from page* 93.)

2. IF the iron be precipitated from a folution rather ftrong by alkaline liquor, marking between five and fix degrees of the areometer of Baumé, the product will be a bluifh green magma. Cotton macerated in this precipitate acquires at firft a dirty irregular green tinge; but fimple expofure to the air converts it to a yellow, in a very fhort time, of a very deep fhade.

It is by a procefs nearly fimilar to this, that the ochre or ruft colour of the fhops is formed.

But thefe colours are attended with various inconveniences. 1. They either corrode the the cloth or injure its durability. 2. This colour is harfh, difagreeable, and cannot eafily be combined with the foft colours afforded by vegetables.

I was defirous of remedying thefe inconveniences, and fucceeded by the following treatment. I work the cotton in a cold folution, marking three degrees (fpecific gravity 1,02), then carefully wring it with the pin, and afterwards plunge it in a folution of potafh at two degrees (1,015), into which fulphate of alumine has been poured to faturation. By this means the colour is brightened, and becomes infinitely finer, fofter, and more agreeable;—the fulphate does not attack the body of the ftuff;—and after having left the cotton in the (fecond) bath for four or five hours, it is taken out to be preffed, wafhed, and dried.

By this procefs, every fhade which can be defired may be obtained by graduating the ftrength of the folutions. The famples which I prefent to the inftitute are prepared according to this method. This fimple procefs, of which the theory will prefent itfelf to the mind of every chemift, poffeffes the advantage of affording a very agreeable, very folid, and particularly cheap, colour. I have ufed it with advantage for nankeens, of which the colour is infinitely more fixed than that of the Englifh nankeens. It has the advantage over them in refifting alkalies, and the only fault I know in it is that of acquiring a brown colour by the action of aftringents.

I was for fome time of opinion, that it might be poffible to combine this yellow with the blue of indigo, to obtain a folid colour. But hitherto my expectations have not been realized, and the refults of the trials I have made are, that there is not a fufficient affinity between the blue of indigo and the oxydes of iron. I obtained only a dirty, earthy, very deep, and unequal green colour.

The oxyde of iron, on the contrary, combines very eafily with the red of madder, and affords a light violet prune colour of very extenfive and advantageous ufe in cotton manufactories.

But if thefe two colours be applied to cotton without employing a mordant capable of fixing the latter, the colour will not only remain dull and difagreeable, from the impoffibility of brightening it, but it will likewife have the very great inconvenience of not refifting alkalies. It is proper, therefore, to begin the operation in the fame manner as when cottons are prepared to receive the Adrianople red; and when the goods are brought as far as to the operation of galling, they are to be paffed into a folution of iron, more or lefs charged, accord-

ing to the nature of the violet which may be defired. The cotton muft be carefully wafhed and maddered twice in fucceffion, after which it is brightened in a bath of foap.

When a true violet of a foft and firm colour is required, the folution of iron is not to be ufed till after the galling. The iron is then precipitated in a bluifh oxyde, which combining with the red of the madder affords a fuperb violet, more or lefs deep according to the ftrength of the galling and of the martial folution.

It is very difficult to obtain an uniform colour by this procefs; and a very even violet is confidered in the manufactories as a mafter-piece of the art. It is generally imagined, that this important problem of the art of dying can only be folved by well-conducted manipulations. But I am well convinced that the great caufe of the unevennefs of this dye is, that the iron depofited upon the cotton receives an oxydation by mere expofure to the air, which varies in the different parts of the cotton. The threads which are at the outfide of the mafs become ftrongly oxyded, while the interior part, being defended from the action of the air, undergoes no change. Whence it follows, that the inner part acquires a faint fhade, while the outer furface prefents a violet almoft black. The only means of remedying this inconvenience, is to wafh the cotton on taking it out of the folution of iron, and to madder it while wet. The cotton is thus rendered more even and velvety.

The folvents for iron are nearly the fame for this colour, as for the yellow of which we have already treated.

I fupprefs all obfervation on the manipulations, in order to confine myfelf to mere chemical report; and on this confideration, I fhall add an obfervation to direct the artift in the brightening of violet upon cotton.

The red of madder, and the oxyde of iron, being depofited on the ftuff, determine the violet colour. This colour inclines to red or blue, accordingly as the one or the other of the two principles predominates. The dyer knows by experience, how difficult it is to obtain a combination which fhall produce any defired tone or colour; particularly, when it is required to be uniform, bright, and folid. It may, neverthelefs, be effected, not only by varying the proportions of the two principles, but alfo by varying the procefs of brightening. It is only neceffary to know the following facts: namely, that foda deftroys the iron, while foap by ftrong boiling attacks the red of the madder in preference. Hence the dye may be made to incline to red or blue, accordingly as the brightening is effected with one or the other of thefe mordants. Thus cotton taken out of the madder, wafhed, and boiled with 30,00 parts of foap, will afford a fuperb violet; though, if foda had been ufed, it would have turned out merely a prune colour.

The oxyde of iron precipitated upon piece-goods, unites ftill more advantageoufly with the yellow afforded by aftringents; and an infinite number of fhades may be obtained by varying the force of the mordants. In this cafe, it is lefs a combination, or folution, of principles, than a fimple mixture, or juxtapofition of the colouring matters upon the ftuff. The oxyde of iron may be more intimately combined with the aftringent principle by a boiling heat, which brings it to the ftate of black oxyde, as our colleague Berthollet has obferved.

It is alfo poffible to render thefe fame colours brown, and give them a variety of tinges, from

from light grey to deep black; ſimply by paſſing the cottons, impregnated with the aſtringent principle, into a ſolution of iron. The oxyde is then precipitated by the principle which is fixed in the ſtuff.

It is an obſervation which may become of high value in the art of dying, that the aſtringent vegetables moſt commonly uſed afford a colour which is not very bright, but of high value for its durability. This yellow colour is brightened in the ſeries of vegetables, in proportion as the aſtringent principle is leſs in quantity, and the livelineſs of the colour is alſo increaſed in the ſame proportion. It is, therefore, difficult to obtain yellow colours which ſhall be at the ſame time ſolid and bright. Theſe two valuable qualities are inverſely proportioned to each other. But it is poſſible to join theſe colouring principles in ſuch a manner as to add brightneſs to ſolidity. Green oak-bark joins perfectly with woad, and ſumach with quercitron. By this mixture, we may ſucceed in combining with the oxyde of iron ſuch vegetable colours as unite durability with brightneſs.

I ſhall conclude theſe obſervations with a remark concerning the uſe of aſtringents in the dying of cotton.

It has been pretended, that by increaſing the proportions of ſumach, or the bark of alder, or oak, it may be practicable to ſupply the place of galls in the red dye for cotton. I ſhould be ſo much the more gratified if this were true, as the galls conſiderably add to the expence of our colours, and ſumach may be had at a low price, becauſe it grows almoſt every where in the dry ſituations of our ſouthern climates; but I can affirm that the ſubſtitution is impoſſible, however large the doſe of this aſtringent may be; for the colour is much paler, with leſs body and durability. I know that this is not the caſe with regard to wool and ſilk, in which this article is uſed with ſucceſs; and in giving an account of this difference, I think it may be aſcribed to the nature of the nut-gall itſelf. 1. The acid, which it contains excluſively of the other aſtringents, as Berthollet has proved, facilitates the decompoſition of the ſoap, with which the colours have been impregnated; and then the oil remains fixed in their texture in a much larger quantity, and more intimately combined. 2. The nut-gall, which owes its developement to animal bodies, retains a character of animalization, which it tranſmits to the vegetable piece; and by that means, increaſes its affinities with the colouring principle of the madder; for it is known how uſeful animal ſubſtances are to facilitate this combination. This animalization becomes uſeleſs in operations upon wool or ſilk.

VIII.

Account of the Experiments of Citizen Clouet, *on the different States of Iron, and its Converſion into Caſt-ſteel*.*

THE learned reporter begins his account, by giving an hiſtorical ſketch of the ſcientific and correct information we poſſeſs, reſpecting the art of ſteel-making. He ſtates, that from the

* From the report of Citizen Guyton, made to the National Inſtitute of France, on 16th Meſſidor in the year VI. (July 4, 1798), inſerted in the Annales de Chimie, XXVIII. 19.

the time when the labours of Reaumur had enlightened the practice of making natural fteel, and fteel by cementation, the theory remained ftationary, notwithftanding the numerous and valuable experiments of Bergman, Rinmann, Prieftley, &c. until the appearance of the excellent memoir of Vandermonde, Berthollet, and Monge, in the Memoirs of the Academy of Sciences for 1786. That the Englifh, who had long fupplied the European market with fteel of cementation, remained alfo in the exclufive poffeffion of the article known by the name of caft-fteel, which, though confined to certain fine works *, is, neverthelefs, a very valuable branch of national induftry; that various experiments have been made with fuccefs, on a confined fcale, in France to imitate this product, fince the time when Jars publifhed an account of the method ufed at Sheffield; but that from a want of precifion in the narrations of thefe proceffes, and the difference which is, with juftice, confidered to fubfift between the experiments of the laboratory, and thofe of the manufacturer in his extenfive operations, the art of making caft-fteel was confidered, by the moft eminent French chemifts, as very far from being publicly known: and Vandermonde, Monge, and Berthollet, notwithftanding their acquaintance with thefe facts, thought fit to declare, in the public inftructions drawn up by order of the committees of fafety, that they could offer nothing but conjectures on the fubject †. In this fituation was the knowledge of the chemifts and manufacturers of France, when Citizen Clouet refumed his experiments on a larger fcale at the houfe of the Confervatory of Arts, and the mineralogical fchool at Paris, on the fufion of various kinds of fteel, and the immediate converfion of iron into caft-fteel.

On this fubject, the author delivered a memoir to the Inftitute of France, which forms the fubject of Guyton's report. He firft treats of the combinations of iron and charcoal. One thirty-fecond part of charcoal is fufficient, as he affirms, to convert the iron into fteel; one-fixth part of the weight of the iron affords a fteelwhich is more fufible, but ftill malleable; and, after this term, it becomes nearer to the ftate of caft-iron, and no longer poffeffes enough of tenacity. By augmenting the dofe of charcoal, the fufibility is increafed; and, at laft, it acquires the ftate of grey caft-iron.

The particular caft-iron, which refults from the combination of iron and glafs, forms the fecond object upon which the attention of Citizen Clouet was fixed. The glafs enters but in a fmall quantity into this compound, notwithftanding which the properties of the mafs are changed. This iron, though very foft to the file, if heated merely to cherry red, flies in pieces under the hammer: the caft ingot contracts greatly in cooling; and when, by careful management, it has been made into bars, the operation of hardening gives them the grain of fteel, and renders them brittle, without adding to their hardnefs.

Charcoal in powder, added to the glafs, changes the refult, and increafes the fufibility; but the nature of the product is greatly influenced by the dofe of thefe ingredients. From one-30th to one-20th part of the iron affords fteel capable of a high degree of hardnefs, which may be forged at a low red heat, and has all the properties of caft-fteel. If more charcoal be employed, the products refemble thofe of the fmelting-furnace.

* It is ufed in a great variety of common tools and works in this country.—N.

† Phil. Journal II. 102.

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The attraction of iron for carbone, continues Citizen Clouet, is such that, at a very high temperature, it will take it even from oxygen. He proves this by the following experiments: Let iron, in small pieces, be put into a crucible with a mixture of equal parts of carbonate of lime and clay; let the heat be urged to the degree necessary to weld iron, and kept at that elevation for an hour, or more, according to the size of the crucible: the metal being then poured into an ingot mould, will prove to be steel of the same quality as cast-steel.

The oxydes of iron are equally susceptible of passing through the states of soft iron, steel, and fusible or cast-iron, according to the proportions of coal made use of. The black oxyde of iron, of which the state appears to be the most constant, becomes iron when heated in the crucible with an equal bulk of charcoal powder: a double quantity affords steel. A progressive augmentation imparts the characters of the white and the grey cast-iron.

Lastly, Citizen Clouet observed the same transitions dependant on the respective quantities, by heating cast-iron, and the oxyde of iron; cast-iron and forged-iron; the oxyde of iron and iron; the oxyde of iron and steel. No more then one-fifth of cast-iron is necessary to convert bar-iron into steel.

Iron and its oxyde do not intimately unite together. The black oxyde, mixed with half the quantity of charcoal which would be necessary for its reduction, affords iron, which is soft, possessing little tenacity, of a black colour, and indistinct fracture.

One-sixth of oxyde restores common steel to the state of iron, by heating them together in the forge, or in the way of cementation.

At the end of his memoir, Citizen Clouet has given observations on the manner of producing cast-steel, and the furnances proper for this effect.

He determines the nature of the fluxes, the degree of heat, the quality of the crucibles, the precautions for casting the ingot, the method of forging this kind of steel, the processes to be followed in experiments at the forge upon two kilogrammes of the materials, and the proportions to be given to a reverberatory furnace capable of heating four crucibles, each containing 12 or 13 kilogrammes of steel (about 28 pounds avoirdupois each crucible).

He remarks, that the mere ingredients of saline glass cannot be directly used in this process; that glasses, which are too fusible, render the steel difficult to forge; that steel, kept for a long time in fusion, takes up more glass than is proper; and, lastly, that the melted matter must be stirred, and the glass carefully taken off before casting, in order to prevent its mixing with the steel.

The Commissaries of the Institute proceeded to repeat and verify the experiments of Citizen Clouet. These operations, which are related at length, were as follows. 1. Six hectogrammes (about 21 oz. avoirdupois) of filings of farrier's nails, and four of a mixture of equal parts of white marble, or carbonate of lime, and baked clay of an Hessian crucible, both reduced to powder, were well blended together, and exposed to the heat of a forge-furnace urged by three bellows-pipes for an hour and a half. The crucible failed at the first experiment; but, on repetition, a bar of steel was afforded. 2. Upon making the experiment with Macquer's furnace, the fusion was not complete, though the fire had been urged to 151° of Wedgwood. 3. In another excellent wind-furnace, 367 grammes (about 13 oz. avoirdupois) of small-drawn iron

iron nails, and 245 grammes (about 8½ oz. avoirdupois) of a mixture of carbonate of lime and baked clay, were expofed to ftrong heat for an hour, when the fufion was judged to be complete; and after removing the vitreous matter the ingot was poured out. From the effect produced on two pyrometric pieces, it was judged that the fteel had undergone an heat of 150 degrees. This fteel had all the properties of caft-fteel, and was made into razors by Citizen Lepetitwalle, who found it of a good quality, eafy to be worked, and capable of bearing a comparifon with the caft-fteel marked *Marfhal* and *B. Huntfman.*

Upon thefe facts, the reporter obferves, that fince iron does not become fteel but by taking up about 0,2013 of its weight of carbone *, and in the prefent procefs it exifts only in the form of carbonic acid, this acid muft confequently be decompofed; which happens, as the reporter obferves, by means of a combination between its principles refpectively and certain adequate portions of the iron, that is to fay, the oxygen of the acid combines with part of the iron, and forms an oxyde with which the vitreous flux becomes charged, and the carbone combines with the reft of the iron, and forms fteel. Hence it may be inferred, that this new procefs muft be attended with a lofs of fo much more confequence, as it is neceffary to ufe iron of the beft quality for making fteel. But, on this head, the reporter takes notice that the lofs in the experiment with the wind-furnace was not quite one-twelfth part; and, in another experiment, by Vauquelin, the lofs was lefs than one twenty-fecond part; a lofs, which he obferves, will be well repaid by the increafed value of the product, and may reafonably be expected to be ftill lefs in operations on a large fcale. He thinks, moreover, that this new method may probably turn out of high value for producing fteel of uniform quality with regard to the dofe of carbone. For he thinks that this quantity, or proportion, is likely to be determined by the equilibrium of the forces of affinity which caufe the decompofition of the carbonic acid. Or, in other words, if we fuppofe an indefinite quantity of carbonic acid to be prefented at an elevated temperature to a mafs of iron not greater than could be converted by the dofe of carbone contained in that acid, the iron will form two combinations, the oxyde and the fteel; and it is conceived that the equilibrium between the attraction which tends to preferve the union of principles in carbonic acid, and thofe which are exerted between the iron and thofe principles will prove to be fuch that the carburet of iron will be formed precifely in thofe proportions which conftitute good caft-fteel. This fubject, which certainly fhews the acutenefs of Citizen Guyton with regard to the doctrines of chemical attraction, muft be decided by the teft of experiment.

The report is concluded with a fummary of the facts and obfervations it contains, together with an inference, that the immediate converfion of iron into fteel, without ufing charcoal, is a great and valuable difcovery with regard to the increafe of national induftry; that there is no doubt but the procefs will fucceed in the large way, and that Citizen Clouet is entitled to a public recompence for his liberal, and unreferved communications.

That all the facts are of high value to fcience, and that the obfervation refpecting the combi-

* This quantity (upwards of one-fifth) fo much exceeds any addition which iron is ftated to gain by converfion into fteel, that I fuppofe it to be 0.2013 in the centenary or hundred parts of iron. Iron is reckoned to gain about a little more than half a pound in the hundred weight by cementation.—N.

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nation of vitreous matter with iron, as well as that which ſhews that carbonic acid can produce the ſteel converſion, are new and important, cannot be queſtioned; but whether the uſe of carbonate of lime and clay, which is attended with ſome loſs, may be preferable to vitreous flux and coal, which afford ſome ſmall additional weight, is not yet, as it ſhould appear, decided by the actual operations.

SCIENTIFIC NEWS.

Accounts of Books, &c.

Letters to a Merchant on the Improvement of the Port of London, demonſtrating its Practicability without Wet Docks, or any additional Burdens being laid on Shipping, and at a a leſs Expence of Time and Money than any other Plan propoſed. By C. Dodd, Engineer, 1799.

THIS octavo pamphlet, of nineteen pages, has been given away by the publiſher of the author's former Reports (Taylor in Holborn). The plan conſiſts in building a new bridge, inſtead of, or upon, the preſent London-bridge, which ſhall conſiſt of one principal centre arch, formed of iron, one hundred feet from low water to the crown of the arch, deſcribing a ſpan of 300 feet, with two large ſhore arches of 80 feet ſpan, as near to the butment on each ſhore as adviſeable, for keeping deep water alongſide the preſent below-bridge quays. Among the advantages of the ſtructure, the principal is, that it will admit ſhipping to paſs through, and arrive at the ſpace between London and Blackfriars-bridge, which, as the engineer obſerves, would contain above double the number of ſhips propoſed to be held in the new London docks, which have been long in agitation; at the ſame time that it would be near the centre of commerce, more than twice as cheap in time and money, and require no houſes to be pulled down, nor eſtabliſhed courſe of induſtry to be diſturbed.

The ſlope on each ſide, which muſt neceſſarily be added to this ſtructure, is propoſed to have an inclination anſwering to three inches in the yard, and will then, as Mr. Dodd aſſerts, terminate, on the city ſide, at Monument-yard, and on the Borough ſide, near St. Thomas's Hoſpital. And this may be proportionally diminiſhed, either in inclination, or in length, by making the main arch, only 60 or 70 feet high, which, it is obſerved, will afford the ſame advantages, provided the ſhipping ſtrike their yards and topmaſts. The evident advantages of this plan are detailed at length, with conſiderable animation, in theſe letters; in the ſecond of which, the ſubject is conſidered, as to, 1. Practicability with regard to the erection. 2. Deepening the river above-bridge. 3. The erection of quays, wharfs, &c. 4. Facility of accommodation to the commerce of the port of London. 5. Advantages of commercial tranſactions in rivers, beyond thoſe in docks, and, in particular, of this plan, beyond the plan of the London docks; and, 6. The poſſibility of completing this undertaking, without any expence being laid upon the ſhipping.

In conſequence of this diſplay and elucidation of the ſubject, a petition has lately been preſented

ſented to the Houſe of Commons, in favour of the plan, and the City of London directed George Dance, eſq. architect to the corporation, to examine and verify the local circumſtances relating to the ſame. By the help of ſome notes I have taken from a ſection made by this architect, which I have ſeen at the Right Honourable Sir Joſeph Banks's, I ſhall be enabled to make a few obſervations.

From ſome experiments made by the Society for the Encouragement of Arts, under the direction of their able ſecretary, Mr. Moore, with an appropriate inſtrument, for meaſuring the reaction, I underſtand that a horſe, moving at the rate of three miles an hour, can only exert a force equal to 70 or 80*lb.* and from the general eſtimate of the work of horſes, deduced at page 466 of the ſecond vol. of our Journal, the reaction againſt a common horſe, moving at the ſame rate, will prove ſomewhat leſs than 60*lb.** and the half of this, namely 30*lb.* will turn out to be the ſteady force exerted by a horſe in a poſt-chaiſe, or carriage, which in London is ſeldom driven quicker than at the rate of about ſix miles and a half an hour; though, on the roads near London, they go at the rate of eight, and ſometimes near nine miles an hour. I think it likely that there might be ſomewhat of temporary effort in the deduction of Mr. Moore, and that 70*lb.* may be a good eſtimate for the ſtout cart-horſes in London; and, from various circumſtances, particularly the over-rated deductions of ſeveral reſpectable authors, we may infer that the animal can double his efforts for a ſhort time, ſuch as ten minutes, without receiving any injury from the exertion. Theſe ſpeculations are not only of general utillty, but are particularly applicable to the ſubject before us. If the ſlope of Mr. Dodd's bridge ſhould be ſo conſiderable as to require additional horſes to ſurmount it,—or if the length of the inclined plane, required to afford an eaſy aſcent, ſhould be ſuch as to exceed every reaſonable computation, with regard to the expences of conſtruction, and the purchaſe of lands,—either of theſe reſults would afford the moſt ſerious objections to the plan itſelf. Mr. Dance's ſurvey appears to have theſe particulars in view.

In his drawing, the new bridge is traced 100 feet high in the clear, *above high water*, and a thickneſs of rather more than ten feet is allowed between the interior curve of the crown of the arch, and the pavement. Hence the inclination of a line drawn from Monument-yard to the higheſt part of the bridge pavement gives 3,9 inches riſe per yard, or very nearly one ninth. If we ſuppoſe, therefore, an horſe to draw half a ton up this ſlope, the weight will re-act with about 120 pounds, and from the length of the line of inclination, it appears that the horſe would have this pull to make for about three minutes. But I muſt not overlook that Mr. Dance's arch is taken from the top of high water, whereas, that of the projector is ſtated, in two places of his pamphlet, to be taken from low-water line; which makes a difference (as I gather from the two lines in Mr. Dance's ſection) of 22 feet, or more than one-fifth in the whole reaction, and reduces it to 85 pounds, or very little more than Mr. Moore's horſes at plough did ſteadily overcome. If this reaſoning be well founded, it will conſequently follow, that the ſudden aſcent of Mr. Dodd's ſlope will not occaſion any inconvenience, or

* That is to ſay, the horſe raiſes (or moves againſt a re-action of) $2\frac{1}{2}$ hogſheads of water, or 1375lb. (at the rate of or) through ten feet in a minute. But three miles per hour, give the rate of 264 feet per minute; whence 264 : 10 :: 1375 : 60.

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render it neceſſary to take his ſmaller arch, which would diminiſh it by one-third more, and bring the reſiſtance within the loweſt eſtimate of re-action given in the foregoing paragraph.

Mr. Dance has ſhewn in his ſurvey, that the mean inclination of Holborn-hill, from Field-lane to Bartlett's-buildings, gives a riſe of 2,06 per yard, and that a ſlope of this kind would reach about half way between Lombard-ſtreet and Eaſtcheap; the latter of which ſtreets would be nearly half ſhut in, while Thames-ſtreet would be entirely ſurmounted, or covered up;—that the inclination of Ludgate-hill from the obeliſk in Bridge-ſtreet to the rails in St. Paul's Church-yard gives a riſe of 0,96 inches per yard, and that a ſlope of this kind would reach beyond Thread-needle-ſtreet on the city ſide, paſſing at double the height of the houſes on the ſouth-ſide of Thames-ſtreet; compleatly covering Eaſtcheap; intercepting the ſecond-floor windows at Lombard-ſtreet; croſſing Cornhill at the height of twelve feet, or above the tops of the ſhop-windows; and nearly four feet high at the end of Thread-needle-ſtreet. The ſame ſlope on the Borough ſide would reach completely through that town, and meet the ground on the road beyond the ſtones' end: the whole length of both ſlopes being above one mile and a quarter. It ſeems unneceſſary to enter into the conſequences deducible from theſe facts, or to obſerve, how much the length of theſe inclined roads would be diminiſhed by rectifying the drawing with regard to the water-line, or by adopting the loweſt of Mr. Dodd's arches; the latter of which would make a difference of nearly two to one. For if the extreme declivity, or ſhorteſt inclined plane, in Mr. Dance's plan can be eaſily ſurmounted by a loaded horſe, for the time required to paſs over its length, it would follow, of courſe, that ſpeculations reſpecting other planes will be ſcarcely applicable to the ſubject *.

I have received a letter from the Rev. Mr. Richardſon, of Bath, incloſing the ſecond letter ſtated by Dr. Gibbes (ſee p. 96 of our laſt number) to be ſtrongly in his favour; and alſo a letter from Dr. Beddoes, on the ſame ſubject; which letters I hope will terminate the diſcuſſion. Mr. Richardſon (April 20) quotes an aſſertion of Mr. Notcutt, made in converſation ſince the diſpute, that he did not know what the ſpecimen was, but conjectured it might be ſulphate of ſtrontian; at the ſame time that Mr. N. informed Mr. R. that he did not yet know its compoſition, as neither he, nor Mr. Clayfield had analyſed it. The ſubſequent part of Mr. R.'s letter, contains general inference and obſervation of that kind, which, in ſelecting the mere facts, I have avoided relating; and a requeſt that Dr. Beddoes would tranſmit to me the facts contained in his letter. From Dr. B.'s letter, I find that he did not think it incumbent on him to publiſh Mr. R.'s report of Mr. Notcutt's teſtimony, becauſe he was in poſſeſſion of the teſtimony itſelf; and he therefore returned this ſecond letter in time, with an invitation to Mr. R. to ſend it to me, if he, or Dr. G. thought it to the purpoſe. With regard to the general ſubject, Dr. B. aſks me, whether I ought not to diſtinguiſh between the

* Since the above was compoſed, I have been informed that in Mr. Dodd's plan, firſt publiſhed in a ſingle ſheet, the main arch was propoſed to be elevated one hundred feet above high water, and that this was the plan ſubmitted to Mr. Dance.

notoriety of a discovery, and of the discoverer? Whether the name of the latter could be said to be before the public? Whether I, or one in many hundreds, even of my English readers, knew it? And whether Dr. G.'s paper will not make him pass for the discoverer abroad, till the succeeding numbers of the Journal shall correct this error? And he adds, that there can be no doubt of the similarity of the Sodbury mineral, to some varieties of that nearer Bristol. Mr. Clayfield suspects the Red-land, Ham-green, and Sodbury strontian to come from the same vein.

With regard to the questions, there is but one which seems to call on me for an answer; and to this I reply, that I knew nothing of Mr. Clayfield, and should probably have mentioned him in a note, or modification of the title at page 535, of the second volume of this Journal, if I had.

Extract of a Letter from Mr. H. Davy.

"When I mentioned to Mr. Clayfield the existence of silex in the epidermis of vegetables, he requested me to examine the equisetum hyemale, or Dutch rush, used in manufactories for polishing brass, &c. A short time after, Mr. Notcutt informed me that he had succeeded in obtaining a globule of glass from it by the blow-pipe. I have examined this plant, and find that the epidermis of it is almost wholly composed of silex, which is reticularly disposed like that of the bonnet cane, or calamus rotang.

"Since my last letter to you, I have seen a paper of the ingenious Vauquelin, in the 88 No. of Annales de Chimie, *On the Excrement of Fowls compared with their Nourishment; with some Observations on the Formation of the Egg.* He found in the ashes of the oat-grain, silex and phosphate of lime. I have not yet had time to examine any grains or seeds, but it is more than probable that the silex exists in the epidermis of the oat-grain, and not in the farina. The husks of wheat and barley, as well as the seeds of grasses, and hard shells and capsules of fruit, will, I have no doubt, be found to contain flint."

The following printed address was transmitted to me by the penny-post, under cover of a letter from Mr. Pepys, jun. to acquaint me that the Society had ordered it to be sent to the Scientific Journals. In return, I sent a letter requesting information concerning the constitution and funds of the Society, particularly with a view to inform the public of the regulations under which they might contribute to the desirable object held forth, as well as of the degree of confidence this body might be entitled to. To these inquiries I received only a verbal answer, that the Society is not yet organized, and that a prospectus will be sent to me as soon as published.

British Mineralogical Society.

The benefits which have resulted to our manufactures, domestic economy, and convenience, from the great progress made in other branches of science, have, for a considerable time, called for the establishment of a Society whose attention should be directed to the analysis and reduction of our native ores and minerals.

The

The principal obſtacle that hindered our mineralogical treaſures from being explored has, in ſome meaſure, been happily removed by the advances that have been made in chemical knowledge; and there is now every reaſon to hope that, at no very diſtant period, by the gradual removal of other impediments, an accurate knowledge will be obtained of the compoſition, nature and properties, not only of ſuch minerals as are generally found in cabinets, but of the various hitherto neglected ſtrata of the different parts of this kingdom.

There are, no doubt, in many of our mines, other valuable metals and minerals (at preſent imported from abroad) beſides thoſe they are worked for; which want of chemical knowledge reſpecting the compound ſtate of ſuch productions have cauſed to be hitherto entirely neglected. Nor indeed could it be expected that the miner, whoſe buſineſs is to produce a given quantity of a known metal in a ſhort time, ſhould ſo far wander from his track as to attempt to examine what he has always been taught to conſider as waſte. When he meets with any other ſubſtance than the one he is in queſt of, it excites no attention, and in many inſtances is turned out of the mine, and ſcattered like rubbiſh to repair the roads.

To aſſiſt in preventing a waſte of the bounties afforded by Providence to this country, THE BRITISH MINERALOGICAL SOCIETY has been inſtituted; but the landholders and proprietors of mines can alone overcome the obſtacles which ariſe from the prejudices of the miners, by cauſing them to produce ſamples of the various ſtrata they paſs through, and by giving them premiums for every new ſubſtance of which they may be able to produce ſpecimens.—The country will be benefited exactly in the proportion that ſuch a practice ſhall become general.

The Britiſh Mineralogical Society will analyſe, *free of expence*, package and carriage excepted, for the proprietors of mines or landed eſtates, whatever ſubſtance they may meet with in ſufficient quantity to render a knowledge of the component parts a deſirable object. In return, the Society expects that ſingular or curious ſpecimens of the mineral productions, which may be met with in working the Britiſh mines, will be ſent for the improvement of the Society's Cabinet, which will thus in time become a national ornament.

It muſt be obvious, that, in purſuits of this nature, a Society poſſeſſes many advantages over an individual; for, in every caſe where the component parts of a mineral are very intricate, the knowledge and experience of all the members can be concentrated for the analyſis.

When the analyſis of any mineral ſent for examinination is completed, the reſult, with an account of the methods employed, will be ſent to the proprietor of the ſpecimen; and if it ſhall contain any new ſubſtance, the probable uſes to which it may be applied will be ſuggeſted.

Such is the outline of the Plan of the Society, which, having nothing mercenary in view, encourages its members to expect that their labours for the mineralogical improvement of the Britiſh nation will be ſeconded, not only by thoſe who have an immediate intereſt in obtaining an accurate knowledge of the nature and properties of the various mineral productions of their eſtates, but by every individual who has the proſperity of his country at heart.

Specimens ſent for analyſis are requeſted to be packed up in ſuch a manner as to prevent, as much as poſſible, their being rubbed or fretted by the carriage; as the external ſtructure and character

character of minerals often aſſiſt in leading to a knowledge of their compoſition. It is alſo requeſted that an account be ſent of the circumſtances connected with the ſpecimen, as the depth below the ſurface of the earth, and the nature and thickneſs of the ſtratum in which it is found, and alſo of the incumbent and ſubcumbent ſtrata, &c. and, where neceſſary, ſpecimens of theſe with their provincial technical names.—Addreſs the package (carriage paid) to the Society's Secretary, Mr. W. H. Pepys, junior, No. 24, Poultry, London.

By Order of the Society.

W. H. PEPYS, jun. *Secretary.*

A Practical Introduction to Spherics and Nautical Aſtronomy, being an attempt to ſimplify thoſe uſeful Sciences,—containing, among other original Matter, the Diſcovery of a Projection for clearing the Lunar Diſtances in order to find the Longitude at Sea; with a new Method of calculating that important Problem. By P. Kelly, Maſter of Finſbury-ſquare Academy, London. Johnſon and Robinſons. Price 6s. p. 210. pl. 17.

This work is divided into two parts; the firſt contains the principles of ſtereographic projection, exemplified by numerous figures drawn upon a large ſcale; theſe are followed by right-angled and oblique ſpheric trigonometry, in which all the problems are ſolved both by projection and calculation: this part concludes with improved ſolutions of certain caſes of ſpheric triangles.

The ſecond part treats of aſtromony, particularly thoſe parts which are uſeful at ſea; ſuch as finding azimuths, amplitudes, time, latitude, and longitude. Theſe problems are ſolved by the globes, and likewiſe by projection and calculation.

The method of finding the longitude by lunar obſervation is here explained at ſome length, in a clear and elementary manner. The principles of this problem are explained by ſtereographic projections, whence rules are deduced for eſtimating the true diſtance beforehand.

A new projection here given for clearing the diſtance from parallax and refraction, is performed by drawing four right lines from the plan ſcale. The author does not inſiſt upon this method as affording a perfect ſolution of the problem, but he obſerves that it will be found ſufficiently exact for the general purpoſes of navigation, and where perfect correctneſs is required, it will even be found uſeful as a guide or check to calculation.

The new method given for working the lunar obſervation, conſiſts in the regular ſolution of two ſpherical triangles, and taking the zenith diſtances inſtead of the altitudes, by which the operation is performed by ſines only: this is certainly an advantage, eſpecially to learners, who are apt to confound ſines with coſines, &c. but however ſimple and accurate this method may be, it cannot be conveniently applied except by ſuch as are in poſſeſſion of Taylor's Logarithms, or ſome other tables which readily afford portions of the quadrant to ſeconds.

The figures throughout this work are on a large ſcale, and appear to be very correct.

Fig. 2.

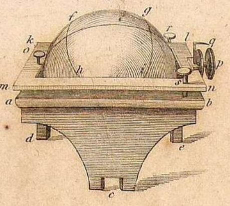


Fig. 4.

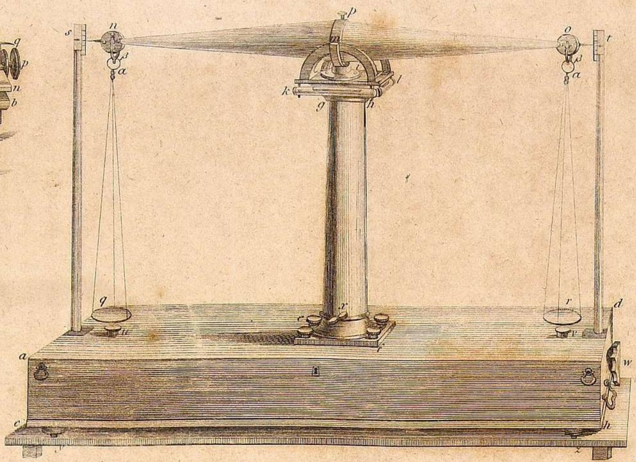


Fig. 6.

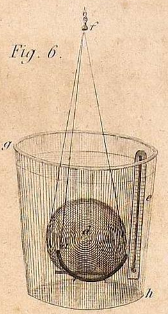


Fig. 7.

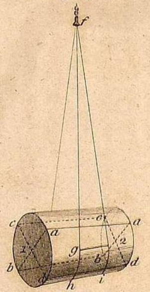


Fig. 5.

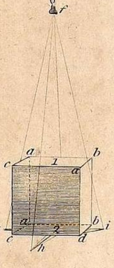


Fig. 3.

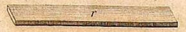
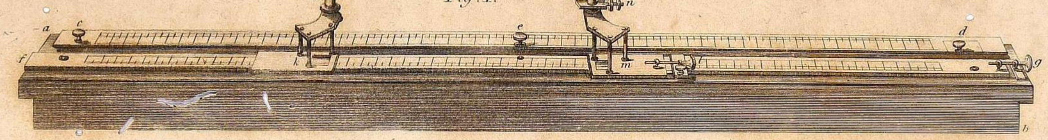


Fig. 1.



Barlow sculp.

Barometric & thermometric first movers.

Fig. 1.

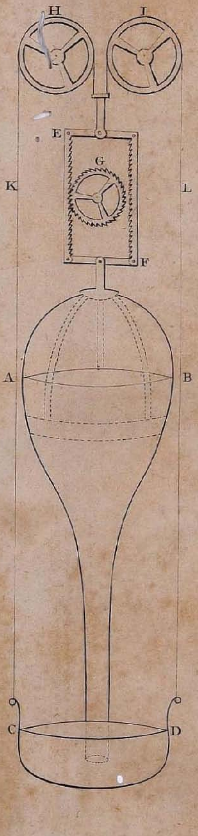


Fig. 2.

Fig. 3.

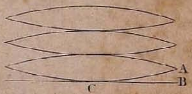


Fig. 4.

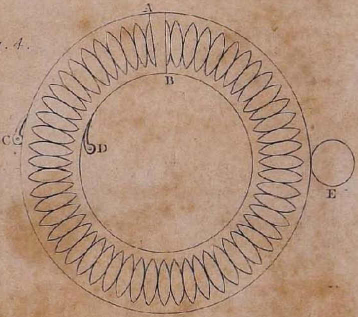
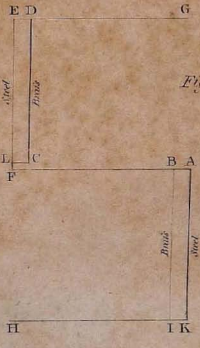


Fig. 5.



Barlow sculp.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY 1799.

ARTICLE I.

Obſervations upon an unuſual Horizontal Refraction of the Air, with Remarks on the Variations to which the lower Parts of the Atmoſphere are ſometimes ſubject. By the Rev. S. VINCE, A.M. F.R.S. and Plumian Profeſſor of Aſtronomy and Experimental Philoſophy in the Univerſity of Cambridge.*

THE uncertainty of the refraction of the air, near the horizon, has long been known to aſtronomers, the mean refraction varying by quantities which cannot be accounted for, from the variations of the barometer and thermometer; on which account, altitudes of the heavenly bodies, which are not more than 5° or 6°, ought never to be made uſe of, when any conſequences are to be deduced from them. The cauſe of this uncertainty is probably the great quantities of groſs vapours and exhalations of various kinds which are ſuſpended in the air, near to the earth's ſurface, and the variations to which they are ſubject; cauſes of which we have no inſtruments to meaſure the effects which they produce in refracting the rays of light. In general, the courſe of a ray paſſing through the atmoſphere, is that of a curve, which is concave towards the earth: the effect of which is, to give an apparent elevation to the object; and thus the heavenly bodies appear above the horizon, when they are actually below it: but it

* Philoſ. Tranſ. 1799, p. 13.

will not alter the position of their parts, in respect to the horizon; that is, the image of the highest part of the object will be uppermost, and the image of the lowest part will be undermost. The figures, however, of the sun and moon, when near the horizon, will suffer a change in consequence of the refraction of the under limb being greater than that of the upper, from which they assume an eliptical form; the minor axis of which is perpendicular to the horizon, and the major axis parallel to it. But a perpendicular object, situated upon the surface of the earth, will not have its length altered by refraction; the refraction of the bottom being the same as that of the top*. These are the effects which are produced upon bodies at, or near, the horizon, in the common state of the atmosphere, by what I shall call the *usual* refraction.

But besides the usual refraction which affects the rays of light, the atmosphere over the sea is sometimes found to be in a state which refracts the rays in such a manner, as to produce other images of the object, which we will call an effect from an *unusual* refraction. In the Phil. Trans. for 1797, Mr. Huddart has described some effects of this kind, which he has accounted for, by supposing, that from the evaporation of the water, the refractive power of the air is not greatest at the surface of the sea, but at some distance above it; and this will solve in a very satisfactory manner, all the phenomena which he has observed: but effects very different from those described by Mr. Huddart, are sometimes found to take place. These I had an opportunity of observing at Ramsgate, last Summer, on August the first, from about half an hour after four o'clock in the afternoon, till between seven and eight. The day had been extremely hot, and the evening was very sultry; the sky was clear, with a very few flying clouds. I shall describe the phenomena as I observed them with a terrestrial telescope, which magnified between thirty and forty times; they were visible, however, to the naked eye. The height of the eye above the surface of the water, at which most of the observations were made, was about twenty-five feet; some of them, however, were made at about eighty feet from the surface; and it did not appear that any of the phenomena were altered, from varying the height of the eye, the general effect remaining the same.

The first unusual appearance which I observed, was that which is represented in Plate VII. fig. 1. Directing my telescope at random, to examine any objects which might happen to be in view, I saw the top of the masts of a ship, A, above the horizon, x y, of the sea, as shewn in the figure. At the same time also, I discovered in the field of view two complete images, B, C, of the ship in the air, vertical to the ship itself. B being inverted, and C erect, having their hulks joined. The phenomenon was so strange, that I requested a person present to look into the telescope, and examine what was to be seen in it, who immediately described the two images as observed by myself; indeed they were so perfect, that it was impossible we could differ in our description. Upon this, I immediately took a drawing of the relative magnitudes, and distances of the ship, and its images, which at that time were as represented in the figure, as near as it was possible for the eye to judge; and it was very easy to estimate them to a very considerable degree of accuracy. As the ship was receding from the shore, less and less of its

* See my Complete System of Astronomy, art. 194.

masts

masts became visible; and continuing my observations, in order to discover whether any or what variations might take place, I found, that as the ship descended, the images, B, C, ascended; but as the ship did not sink below the horizon, I had not an opportunity of observing at what time, and in what order the image would have vanished, if the ship had so disappeared.

Being desirous of seeing whether the same effect was produced upon the other ships which were visible, I directed my telescope to another ship, A (fig. 2), whose hulk was just in the horizon, *x y*; when I observed a complete inverted image, B, the main-mast of which just touched that of the ship itself. In this case, there was no second image as before. The ship, A, moving upon the horizon, B continued to move with it without any variation in its appearance.

The next ship which I directed my telescope to, was so far on the other side of the horizon, *x y*, as just to prevent its hulk from being seen, as is represented by A (fig. 3). And here I observed only an inverted image of part of the ship; the image, *y*, of the topsail, with the mast joining that of the ship, the image *x* of the top, *a* of the other mast, and the image *z* of the end *c* of the bowsprit only appearing at that time. These images would suddenly appear and disappear very quickly after each other; first appearing below, and running up very rapidly, shewing more and less of the masts at different times as they broke out; resembling in the swiftness of their breaking out, the shooting out of a beam of the aurora borealis. As the ship was descending on the other side the horizon, I continued my observations upon it, in order to discover what changes might take place; when I found, that as it continued to descend more of the image gradually appeared, till at last the image of the whole ship was completed, with their main-masts touching each other; and upon the ship descending lower, the image and the ship separated; but I observed no second image as in the first case: a second image, however, might probably have appeared if the ship had continued to descend.

Upon moving my telescope along the horizon, in order to examine any other ships which might be in sight, I observed, just at the horizon, *x y* (fig. 4), the top, *a*, of the mast of a ship; and here an effect was observed which had not been discovered; for there was an inverted image, B, vertical to, *a*, an erect image, C, both of them very perfect and well defined; and an image, *v w*, of the sea between them, the water appearing very distinctly. As the ship was coming up towards the horizon, I continued to observe it, in order to discover the variations which might follow, and found that, as the ship approached the horizon, the image C gradually disappeared, and at last it vanished; and after that, the image *v w* of the sea disappeared; and during this time the image B descended; but the ship did not rise so near to the horizon as to bring the main-masts together. Had I directed my telescope to the same point of the horizon a little sooner, I should have seen the two images before the ship itself was visible; in fact, the images were visible when the whole ship was actually below the horizon; for from the very small part of the mast which was at first visible, that part must then have been below the horizon, and appeared above it by the usual refraction; the altitude of *a* above the horizon having then been much less than the increase of altitude which arises from the common horizontal refractions. The discovery of ships in this manner, might in some cases be of great importance;

importance; and on ſuch occaſions, it might be worth while to appoint proper perſons to make obſervations for that purpoſe.

The cliffs of Calais being very viſible, I directed my teleſcope towards them, in order to examine whether there were any thing unuſual in their appearance; when I obſerved an image of the cliffs above the cliffs themſelves, together with an image of the ſea ſeparating them, as is repreſented in fig. 5, in which *x y* repreſents the horizon of the ſea, A B the cliffs, *a b* their image, and *v w* the image of the ſea between them; the depth of *a b* was much leſs than that of A B. It is probable, however, that *v w* might not be the image of the ſea immediately adjoining to the cliffs, but a partial elevation of the ſea at ſome diſtance from them; and that the image *v w* might intercept ſome part of the image *a b*, which would otherwiſe have been viſible; we muſt not, therefore, conclude that the image *a b*, ſo far as it appeared, was leſs than the correſponding part of the object. From the memorandums which I made at the time of obſervation, I do not find that I examined the appearance of the cliff, A B, and its images *a b*, which had there, at that time, been any ſtriking marks in them, would have determined whether the object and its image were of the ſame magnitude. The image *a b* was, however, erect; the boundaries on the top of A B and *a b* agreeing together. Having examined this for ſome time, and taken a drawing of the appearance, during which I could diſcover no variation, I directed my teleſcope to other objects; and upon turning it again to the ſame cliffs, after the ſpace of about ſix or ſeven minutes, the images *a b* and *v w* were vaniſhed: but examining them again ſoon after, the images were again viſible, and in every reſpect the ſame as they appeared before. A ſhort time after they diſappeared, and did not appear any more.

Soon after the above appearances, I obſerved a ſhip, C, with the hulk below the horizon, *x y*, paſſing by the ſame cliffs, A B; an inverted image, D, of which appeared againſt the cliffs, as repreſented in fig. 6. The ſhip was in motion, and remained at the ſame diſtance on the other ſide of the horizon. I continued my obſervations upon it, till it had paſſed the cliffs for a conſiderable diſtance, but there was no change of appearance. The cliffs were illuminated by the ſun, and appeared very diſtinctly; but there was no image above, as in the laſt caſe.

Continuing to obſerve the ſame cliffs, A B, fig. 7, I ſoon after diſcovered two partial elevations, *m n*, of the ſea, by the unuſual refraction; they changed their figures a little, and diſappeared in the place where they firſt appeared, and were equally diſtinct in every part.

About this time, I diſcovered a very thick fog coming upon the horizon from the other ſide, rolling upon it with a prodigious velocity, curling as it went along, like volumes of ſmoke ſometimes out of a chimney. This appeared ſeveral times: I conclude, therefore, that there was a conſiderable fog on the other ſide of the horizon.

The laſt phenomenon which I obſerved, was that which is repreſented in fig. 8, where *x y* repreſents the horizon, *a b* two partial elevations of the ſea meeting at *c*, and continued to *d*; *e*, another partial elevation of the ſea, of which kind I obſerved ſeveral, ſome of which moved parallel to the horizon with a very great velocity. I conjecture, therefore, that theſe appearances were, in part at leaſt, cauſed by the fog on the other ſide the horizon; for though I did

did not at the fame time fee the motion of thefe images, and that of the fog, yet from memory, I judged the motions to be equal; and they were alfo in the fame direction. A fog, which by producing an unufual refraction, might form thefe images, would, by its motion, produce a correfponding motion of the images.

I have here defcribed all the different phenomena which I obferved from the unufual refraction, of moft of which I faw a great many inftances. Every fhip which I obferved on the other fide of the horizon of the fea, exhibited phenomena of the kind here defcribed, but not in the fame degree. Of two fhips, which in different parts were equally funk below the horizon, the inverted image of one would but juft begin to appear, whilft that of the other would reprefent nearly the whole of the fhip. But this I obferved, in general, that as the fhip gradually defcended below the horizon, more of the image gradually appeared, and it afcended; and the contrary, when the fhips were afcending upon the horizon in different parts, one fhip would have a complete inverted image; another would have only a partial image; and a third would have no image at all. The images were in general extremely well defined, and frequently appeared as clear and fharp as the fhips themfelves, and of the fame magnitude. Of the fhips on this fide of the horizon, no phenomena of this kind appeared; there was no fog upon our coaft, and the fhips in the Downs, and the South Foreland, exhibited no uncommon appearances. The ufual refraction at the fame time was uncommonly great, for the tide was high, and at the very edge of the water I could fee the cliffs at Calais a very confiderable height above the horizon; whereas they are frequently not to be feen, in clear weather, from the high lands about the place. The French coaft alfo appeared both ways to a much greater diftance than I ever obferved it at any other time, particularly towards the eaft, on which part alfo the unufual refraction was the ftrongeft.

During the remainder of my ftay at Ramfgate, which was about five weeks, I continued daily to examine all the fhips in fight; but I difcovered no phenomena fimilar to thofe which I have here given a defcription of. The phenomenon of the fhip, obferved by Mr. Huddart, differed altogether from thofe above defcribed, as the inverted image which he obferved was below the fhip itfelf. An appearance of this kind I obferved on Auguft the 17th, about half an hour after three o'clock in the afternoon, of which fig. 9 is a reprefentation. The real fhip is reprefented by A, and the image by B; *e r*, *m v*, the hulks; *s t*, the flag; and *w x*, its image, juft touching it, with the fea, *x y*, below. Between the two hulks, fome faint dark fpots, and lines appeared, but I could not difcover what they were. The reprefentatives of the veffel, at the time of this appearance, was not quite come up to the horizon; and as it approached it, the image gradually diminifhed, and totally difappeared when the fhip arrived at the horizon.

It remains now, that we enquire into the caufes which might produce the very extraordinary effects which have been above related. From the phenomena, we are immediately led to the nature of the path of the rays of light to produce them; and we may conceive that the air may poffibly be in fuch a ftate as will account for the unufual tract which they muft have

described. For let *b* (fig. 10), be the surface of the sea; *a b z*, an object; E, the place of the eye; *a r* E, *b s* E, the progress of two rays by the usual refraction from the extreme parts of the object to the eye; to these curves, draw the tangents E *a'*, E *b'*, and *a' b'* will be the image of the object, as usually formed. Now if we take the case represented in fig. 4. let *a'' b''* represent the inverted image, and *a''' b'''* the erect image. Join *a''* E, *a'''* E, and *b''* E, *b'''* E, and these lines must respectively be the directions of the rays entering the eye, from *a b*, in order to produce the images *a'' b''*, and *a''' b'''*, hence these lines must be tangents at E, to the curves which are described by the rays of light; let, therefore, *a n* E, *a m* E, *b v* E, *b w* E, be the curves described. We have, therefore, to assign a cause which may bring rays passing above the rays *a r* E, *b s* E, to the eye at E. Now if there were no variation of the refractive power of the air, a ray of light passing through it would describe a straight line; therefore the curvature of a ray of light passing through the atmosphere depends upon the variation of the refractive power of the air. If, therefore, we suppose the air lying above *a r* E to vary quicker in its refractive power than the air through which *a r* E passes, the curvature of a ray proceeding above that of *a r* E will be greater than the curvature of *a r* E; and upon this principle we may conceive that a ray may describe the curve *a n* E, and in like manner, if a quicker variation of refractive power should take place above the curve *a n* E, than in that curve, a third ray may describe the curve *a m* E. The same may be said for the rays *b v* E, *b w* E, diverging from *b*. The alterations of the refractive power may arise partly from the variations of its density, and partly from the variations of its moisture; and the passage of the rays through the boundary of the fog, may there suffer a very considerable refraction; for from the motion of the fog, and that of the images above mentioned, I have no doubt that the fog was a very considerable agent in producing the phenomena. When all the causes co-operate, I can easily conceive that they may produce the effects which I have described. If the cause should not operate in the tract of air through which the curves *a n* E, *b v* E pass, but should operate in the tract, through which *a m* E, *b w* E pass, an erect image which would be visible, but there would be no inverted image; and should it operate in the latter case, but not in the former, there would be only an inverted image.

As the phenomena are very curious and extraordinary in their nature, and have not, that I know of, been before observed, I have thought proper to lay a description of them, with all the attending circumstances, before the Royal Society. They appear to be of considerable importance; as they lead us to a knowledge of those changes, to which the lower parts of the atmosphere are sometimes subject. If when these phenomena appear, a vessel furnished with a barometer, thermometer, and hygrometer, below, and also at the top of the mast, were sent out to pass below the horizon, and return again, and an observer at land, having like instruments, were to note, at certain intervals, the situation and figure of the images, it might throw further light upon this subject, and lead to useful discoveries respecting the state of the atmosphere, from a conjunction of the causes which affect these instruments.

An

II.

An Account of ſome Endeavours to aſcertain a Standard of Weight and Meaſure. By Sir George Shuckburgh Evelyn, Bart. F.R.S. and A.S.

(Continued from p. 107, of the preſent volume.)

(§. 20.) *Experiment of the Cube of Braſs weighed in Air.*

THE cube was ſuſpended to the right arm of the beam, by the ſcale belonging to it, and the left ſcale pan, with the mark x, was hung at the other end of the beam, in which were placed the following weights*, made by E. Troughton.

		grains.
viz. No.	15 of	16384
	14 -	8192
	13 -	4096
	12 -	2048
	11 -	1024
	9 -	256
		84,82

The total weight of the cube in air = 32084,82 { the barom. being at 29,0 inch. / the therm. - - - at 62°,0 }

(§. 21.) *Experiment of the Weight of the Cylinder in Air.*

		grains.
No.	15 of	16384
	13 -	4096
	11 -	1024
		53,37
		21,557,37

But a counterpoiſe of 555,02 grains, having been uſed, by miſtake, inſtead of - - 552,34 — Add this exceſs = 2,68 } = + 2,68

And the total weight of the cylinder is = 21,560,05 { the barometer at - - 29,0 inch. / the thermometer at - 62°,0 }

(§. 22.) *The Cube weighed in diſtilled Water.*

Sept. 5. Put into the left ſcale, the counterpoiſe for the water ſcale { 300 / 100 } = - - 400,00 grains.

The cube, with its ſcale, was then immerſed in the water.

* This ſcale contained alſo 555,02 grains, being the weight or counterpoiſe to the ſcale for the cube.

I then

		grains.
I then restored the equilibrium, by putting into the opposite or left-hand common scale, Mr. Troughton's weights, No. 10. - - -		= 512,00
(The barom. standing at 29,47 inches,		200,
the therm. at 60°,2.)		30,
		3,70
	grains.	
But a counterpoise of - - - -	400	745,70
having been taken, by mistake, instead of -	442,75	= − 42,75
Deduct the difference, which was so left out - -	= 42,75	
The apparent weight of the cube in water becomes - - -		= 702,95
Add the correction * for the loss of weight of the 4 wires, by immersion $2\frac{1}{4}$ inches deeper than when the counterpoise was adjusted - - - - -	= +	,08
And the true corrected weight of the cube in water, with 60°,2 of heat becomes - - - - -		703,03

(§. 23.) *Experiment of the Cylinder in distilled Water.*

Sept. 5. The thermometer being at from 60°,2 to 60°,5, and the barometer 29,47 inches,

	grains.	
Put into the left scale pan, the counterpoise to the water-scale for the cylinder - - - - - - - -	300 100 41,7	= 441,7

The cylinder, with its water-scale, was immersed in water. I then restored the equilibrium, by putting into the left scale,

	grains.
Mr. Troughton's weights, No. 12 - - - -	= 2048
No. 9 - - - -	= 256
	200
	30
	10
	4
	1,10
Weight of the cylinder in water - - - - -	= 2549,10

* When the cube was immersed, the water in the glass jar stood $2\frac{1}{4}$ inches higher than when the counterpoise for this water-scale was adjusted, and found to be 442,75 grains (see §. 18.); and 1 inch of alteration in the height of the water having appeared to be = 0,0354 grain in weight (§. 19.), $2\frac{1}{4}$ inches will be = 0,078 grain; and so much must be added, to correct for the loss of weight in the four wires that suspended the scale and cube in water. When the cube was immersed, the surface of the water stood 1,5 inch below the top of the glass jar, and 9,7 inch below the centre of the beam, or index point.

When the cube was in the water, the beam was clearly sensible with $\frac{1}{10}$ of a grain.

Add

Add the correction for the loss of weight of the four wires, by being $1\frac{1}{2}$ inch deeper immersed in the water, than when the counterpoise was adjusted - - - - - - } = + 0,05*

Corrected weight of the cylinder in water - - - - = 2549,15

After this experiment, I discovered that some small bubbles of air had insinuated themselves between the cylinder and the scale in which it hung; these therefore were removed, and the experiment repeated, as follows:

* In order that *this* and some other corrections may be the more easily applied, I have computed the three following tables, to be used whenever great accuracy is required.

Table I. Shewing the expansion of cast brass, both in length and solidity, and also of water, in solidity, by the effect of heat: the former is derived from Mr. Smeaton's experiments (Phil. Transf. vol. XLVIII.); and the latter from some of my own, when I was a resident member of the university of Oxford.

Degrees of Heat.	Expansion of Brass. In Length.	Expansion of Brass. In Solidity.	Expansion of Water. In Solidity.
o	Millionth Parts.	Millionth Parts.	Millionth Parts.
1	1	3	165
2	2	6	330
3	3	9	495
4	4	12	660
5	5,2	16	825
6	6	19	990
7	7	22	1155
8	8	25	1320
9	9	28	1485
10	10,4	31	1650

Table II. Shewing the correction for the wires, or the diminution of the weight of the water-scales, by immersion in water.

By Immersion in Water.	The 4 Wires of the Cube, or Cylinder.	The 3 Wires of the Sphere lose
Inches.	Grains.	Grains.
1	— 0,035	— 0,078
2	— 0,071	— 0,157
3	— 0,106	— 0,235
4	— 0,142	— 0,314
5	— 0,177	— 0,392
6	— 0,212	— 0,471
7	— 0,248	— 0,549
8	— 0,283	— 0,628
9	— 0,319	— 0,706
10	— 0,354	— 0,785
20	— 0,708	— 1,570

N. B. 80 inches in length, of the wires for the scales for the cube and cylinder weigh - - } grs. 6,16

therefore 1 inch will be ,077 grain, and 4 wires of 1 inch - } = 0,308

Also, 91 inches of the wire for the sphere weigh - } = 20,71

and 1 inch = 0,227, and 3 wires of 1 inch - - } = 0,683

and the specific gravity of the wire is = 8,7

Table III. Shewing the correction of the weight of the sphere in air, on account of the weight, or heat, of the atmosphere.

Barometer.	Correction.	Therm.	Correction.
Inch. $\frac{1}{10}$.	Grains.	o	Grains.
29,5	0,00	50	0,00
1	— ,12	1	+ 0,10
2	,23	2	0,20
3	,35	3	0,30
4	,47	4	0,40
5	,58	5	0,50
6	,70	6	0,60
7	,82	7	0,70
8	,94	8	0,80
9	1,05	9	0,90
10	1,17	10	1,00

N.B. If the barometer is below $29\frac{1}{2}$ inches, or the thermometer below 50°, use the contrary signs.

Water being taken as heavier than air, as 836 : 1 (see Observations in Savoy, Phil. Transf. for 1777), the barometer being at 29,27, and thermometer 51°, a sphere of air equal in bulk to the brass sphere, viz. = $113\frac{1}{2}$ cubic inches, would weigh, when the barometer was 29,5 inches, and the thermometer 50° = 34,57 grains; and 1 cubic inch of such air = 0,304

This correction will serve for any other body whose bulk is known.

		grains.
Weights as before	No. 12 =	2048
	No. 9 =	256
		200
		30
		10
		4
		1,10
The buoyancy of the air bubbles being removed	= +	3
		1,07
		2553,17
Add the correction for the loss of weight in the wires, as before	= +	0,05

And the more exact weight of the cylinder in water becomes = 2553,22 { with the temperature 60°,5 and the barometer 29,47 inches.

Note, when { the cube / the cylinder } { was weighed in water, its centre was below the surface of the water } = 2,5 / 3,7 inches.

that is, the cylinder was the deepest by - - - = 1,2

The repetition of this experiment shews how necessary it is to attend to the most trifling circumstances: there were not more than three or four of these particles of air, and those not larger than a small pin's head. Moreover, it may be noted, the distilled water in which these experiments were made, being afterwards examined with my (Martin's) hydrometer, in the heat of $60^\circ\frac{1}{2}$, weighed on that scale = 1,0005; so that I see no reason for diffidence in the quality of the water.

(§. 24.) *A Synopsis of the preceding Experiments.*

	Cube.	Therm.	Cylinder.	Therm.	Barom.
Contents (true to $\frac{2}{100000}$) in inches	Inches. 124,18917	° 61	74,94826	° 62	Inches.
Weight in air, true to 0,02 grain	grains. 32084,82	62	21560,05	62	29,00
Weight in water, true to 0,10 grain	703,03	60,2	2553,22	60,5	29,47
Weight of an equal bulk of water, true to 0,12 grain, or $\frac{1}{190000}$	31381,79		19006,83		
Weight of a cubic inch of water, from these experiments	252,694		253,600*		

* The weight of a cubic inch of common or rain water has been reckoned about 253 grains, sometimes = 253,33 grains, at others 253,18. But authors do not seem to have agreed in what they meant by *common* water, *rain* water, *pump* water, *spring* water, and *distilled* water; for occasionally they are all confounded, and made to pass for each other; and sufficient notice seems not to have been taken of the temperature to which these weights were assigned. See Martin's *Philosophia Britannica*. Lewis's Philosophical Commerce of Arts. Chambers's Dictionary, by Dr. Rees, &c. &c.

The

The diverfity in the refult of thefe two experiments is deferving of notice, and muft be explained. It may proceed from two caufes, which we will now inquire into. But firft it may be obferved, that the accuracy in meafuring the dimenfions of thefe two bodies, as well as the precifion in weighing them, has, I think, been fuch as to put out of all doubt this part of the experiment. From whence then does this difference arife? Either of two caufes may be fufpected; *viz.* the preffure of the water againft the fides of thefe two bodies altering their volumes, which, it may be prefumed, would have a greater effect on the cube, from its figure, than on the cylinder, and in a direction agreeable to this difference; that is, it would diminifh the capacity of the cube more than that of the cylinder, and thus make the apparent weight of a cubic inch lefs in the experiment of the cube. But alfo we fee, that the cylinder was weighed at a greater depth, by 1,2 inch, than the cube, below the furface of the water. Now, if it be true that water is compreffible *, it will become denfer, from its weight, at different depths, and this circumftance would act in the fame way with that juft mentioned; *viz.* would make the apparent weight of a cubic inch lefs from the experiment of the cube than the cylinder, which we fee is the fact.

(§. 25.) In order to diffipate thefe doubts, I caufed a very accurate hollow brafs fphere to be made, of about fix inches diameter, and of fuch thicknefs of metal, *viz.* 0,13 inch, as to be very little heavier than water, and yet of fuch ftrength as, together with its form, to refift any probable change of bulk by the preffure of water.

This fphere, which has already been mentioned (§. 10.), was examined in the following manner. The fix-inch moveable bar *r* (Plate V. fig. 3.) of the guage, was compared with the divided fcale of inches, fig. 1. The microfcopes being adjufted to exactly fix inches, or the interval between 26 inches and 32 inches, and the bar placed under them, the excefs above 6 inches was found to be as follows, by the micrometer, *n o*.

1ft trial. inches.		2d trial, after re-adjuftment. inches.	
6+,0055	thermom. 64°,0	6+,0055	therm. 64°,0
,0053		,0052	
,0056		,0055	
,0054		,0054	
,0057		,0052	

Mean of the 1ft trial = 6 ,00550 — 6 ,00536
Mean of the 2d trial = 6 ,00536

Mean of both, or length of bar } = 6 ,00543 } in the temperature of 64°.

(§. 26.) The bar was then placed in the rectangular *gauge k l m n*, fig. 2. in the direction *p o*; and the end of the micrometer fcrew brought to bear againft it repeatedly, fo as to touch without force, or confiderable preffure: and the divifions † cut by the index, on the micrometer plate of the gauge, were as follow:

* See Mr. Canton's experiment in the Phil. Tranf. vol. LII.

† Each thread of this fcrew is = $\frac{1}{103}$ inch, and each revolution of the fcrew is divided into 100; fo that every divifion on the micrometer plate is = $\frac{1}{10300}$ inch.

Trial 1st. Division on the micrometer.		Trial 2d. Division on the micrometer.		Trial 3d. Division on the micrometer.	
65		64		$64\frac{1}{2}$	
63		62		65	
66	therm. 62°,0	65	therm. 62°,0	66	therm. 62°,3
70		63		63	
66		$62\frac{1}{2}$		$62\frac{1}{2}$	
Mean = 66		63,3 *		64,2	

The mean of these three means is 64,5 with the temperature 62°,1.

(§. 27.) The bar was now removed from the gauge, and the sphere put there in its place; and, by means of the three great circles, each of which was divided into 8 equal parts, nine several diameters of the sphere were taken, as follow:

	Div. of microm.			Div. of microm.			Div. of microm.			Div. of microm.	
	40			40			41			44	
	50			42			49			46	
diam. AB	47	therm. $62^\circ\frac{1}{2}$	diam. GH	45	therm. 62°,5	diam. CD	43	therm. 62°,4	diam. IK	47	therm. 62°,5
	42			42			42			45	
	46			44			$44\frac{1}{2}$			46	
Mean =	45			42,6			43,9			45,6	

The above four mean dimensions may be called *equatorial*, viz. - - 45; 42,6; 43,9; 45,6

The mean of which is - - - - - = 44,3

	Div. of micr.			Div. of micr.			Div. of micr.	
	44			42			40	
	46			45			44	
diam. EF	44	therm. 62°,5	diam. 1, 2	45	therm. 62°,6	diam. 3, 4.	41	therm. 62°,8
	45			40			40	
	45			41			41	
Mean	44,8			42,6			41,1	

These three last dimensions, together with the 1st of the preceding set, may be called *meridional*, being in a circle at right angles to the former, viz. AB = 45; EF = 44,8; 1,2 = 42,6; 3,4 = 41,1

The mean of which is - - - - - = 43,5

and differs from the former not quite $\frac{1}{10000}$ inch.

* In all these experiments with the gauge, the figures on the micrometer plate increase as the screw goes *forward*; viz. the higher numbers indicate a less interval or diameter.

In

In another great circle, 90° from the preceding, comprising the diameters already taken, EF and CD, at the intersection of the two former circles, were taken:

	Div. of micr.			Div. of micr.	
The diameter $\alpha\beta$	41, 44, 44, 41, 40	therm. 63°	diameter $\gamma\delta$	40, 41, 42, 40, 42	therm. 63°,1

The diameters

EF	taken as before	44,8
CD		43,9
$\alpha\beta$	- -	42,5
$\gamma\delta$	- -	41,0

Mean - - = 43,0, which is that of another great circle or meridian, at right angles to the former; from whence it will be seen, that not one of the three circles differs from another more than about $\frac{1}{10000}$ inch.

The preceding 9 mean dimensions of the diameter, collected, are

AB = 45,
CD = 43,9
GH = 42,6
IK = 45,6
EF = 44,8
1,2 = 42,6
3,4 = 41,1
$\alpha\beta$ = 42,5
$\gamma\delta$ = 41,0

the mean of which is = 43,7 in the temperature - 62°,6

Now the import of the foregoing experiments is this, that when the mean diameter of the sphere is holden between the points of contact of the gauge, near *o* and *p*, the index of the micrometer shews - - - - - - = 43,7 divis.
but, when the bar *r* is placed there, it shews - - - = 64,5

the difference is - - - - - = 20,8
and by so much is the bar shorter than the diameter of the sphere.

	inches.
These divisions, 20,8, are equal to (§. 26.) - - -	0,00202
and the length of the bar has already (§. 25.) been found - -	= 6,00543
therefore the true diameter of the sphere becomes - - -	= 6,00745

which quantity I think must be true to within $\frac{1}{10000}$ inch.

(§. 28.) The cube of this diameter, 6,00745 inches × ,5236, as is well known, will give the contents of the sphere in cubic inches, viz. = 113,5194 inches, which must be very near the truth: for, if not, let it be supposed that the inaccuracy in the measurement, or the irregularities in the figure of this sphere, should be such as to amount to $,\frac{1}{000}$ inch, and these so many, without balancing each other, as to produce a spheroidical form, one of whose diame-

ters

ters should exceed the other by $\frac{1}{1000}$ inch; in that case, the error in the assumed solid would not exceed $\frac{1}{3000}$ part of the whole: and this is a position infinitely too extravagant to be admitted, when we recollect, that this diameter has been probably taken to within a tenth part of that error.

(§. 29.) The weight of this sphere, in air and in water, comes next under our consideration; the experiments for which were as follow, made June 12, 1797; the barometer being at 29,74 inches, and the thermometer, in air, at 67°.

Experiment the 1st.

	Troy grains.
The weight of the sphere in air, the counterpoise, or weight of the scale or cradle, *a b c f* (Plate V. fig. 3.), in which the sphere hung, being allowed for *, so that *this* was the net weight - - - - -	=28722,64
	grains.
The sphere and scale suspended in water, with it centre 5,6 inches below the surface, and the heat 66° - - -	= 303,17
Deduct the counterpoise, or weight of the scale, in water, with the same heat of 66°, and same depth † below the surface -	=—253,32
The difference is the net weight of the sphere in water of the temperature 66°, which, deducted from its weight in air - - - -	— 49,85
Leaves the weight of a bulk of water = the sphere, in the temperature 66°, and 5,6 inches below the surface - - - -	=28672,79

Experiment the 2d. June 16, 1797.

The barometer being at 30,13 inches, and the thermometer at 68°.

	grains.
Weight of the sphere, together with the scale, in air - -	29265,91
Deduct the weight of the scale, or counterpoise, in air - -	=— 544,03
Remains the total net weight of the sphere in air - - -	= 28721,88
And, to reduce this to the same state of the atmosphere as the preceding observation, viz. 29,74 inches of the barometer, add the correction for 0,39 inch (see table, §. 23.) - - -	= + ,46
Also the correction for 1° of the thermometer - - -	= + ,08
And the net weight of the sphere, in an atmosphere of 29,74 inches, and heat of 67°, becomes - - - - -	= 28722,42
	grains.
Weight of the sphere, with its scale, in water, 3,7 inches below the surface, and the thermometer at 66°,1	= 484,70

* The weight of this scale, with its 3 wires, in air, was = 276,10 grains.

† The sphere having been weighed in the same depth of water that the counterpoise to the scale was determined in, no correction for the greater or less immersion of the scale-wires was here necessary; which however will sometimes be the case. See §. 29. and table II. of correction, §. 23.

From

	grains.	grains.
From thence deduct the weight of the scale in water	= 435,09	
The net weight of the scale in water becomes -	= 49,61	
To which, add the correction for the wires of the scale being immersed 2,53 inches deeper now, than when its weight in water was determined (see table, §. 23.)	= 0,20	
And the corrected net weight, in water, is - - -		49,81
Which, deducted from its weight in air, leaves the weight of a bulk of water = the sphere, in temperature 66°,1 - -		= 28672,61
Correction for 0°,1 of heat *. - - - -		+ ,45
And the true corrected weight of a bulk of water equal to the sphere, reduced to the barometer = 29,74, and thermometer 66°,0, becomes		= 28673,06

Experiment the 3d. June 16, 1797.

	grains.	grains.
The true net weight of the sphere in air, reduced to a state of the barometer of 29,74 inches, and thermometer 67°, as in last experiment -		= 28722,42
Weight of the sphere, together with its scale, in water, 6,8 inches below the surface; the thermometer at 66°,1 - - - -	= 484,20	
Deduct the weight of the scale in water - -	435,09	
The difference is the net weight of the sphere in water, of the temperature 66°,4 - -	= 49,11	
To which, add the correction for the wires of the scale being immersed 5,5 inches deeper now, than when its weight in water was determined (see table, §. 23.)	+ ,44	
The corrected net weight, in water, becomes - - -		= 49,55
Which, deducted from its net weight in air, leaves the weight of a bulk of water = the sphere, and 6 inches below the surface, with the heat of 66°,4 - - - - -		= 28672,87
Correction for 0,°4 of heat (see table, §. 23.) - -		= + 1,81
The true corrected weight of a bulk of water = the sphere, in the heat of 66°,0, and with a pressure of the barometer of 29,74 inches, and 6 inches below the surface - - - -		= 28674,68

* One degree difference of heat in the water will alter the weight of the sphere in water, or the weight of the bulk of water equal to it, = 4,54 grains; so that, by far the greatest source of error, in these experiments, lies in the difficulty of exactly knowing, and preserving, the temperature of the water.

Results

(§. 30.) *Reſults of the Obſervations of the Sphere collected.*

Correct weight of a bulk of water = ſphere, the barometer being at 29,74 inches, thermometer 66°,0.	grains.	At a depth below the ſurface of the water. inches.
By the 1ſt obſervation - - - -	28672,79	5,6
2d obſervation - - - -	28673,06	3,7
3d obſervation - - - -	28674,68	6,8
Mean of all - - - - -	28673,51	5,37

Which, I think, may fairly be preſumed to be within 1 part in 50,000 of the truth.

(§. 31.) Now the contents of this ſphere having already (§. 28.) been found to be = 113,519 cubic inches; $\frac{28673{,}51}{113{,}519} = 252{,}587$ grains, will be the weight of a cubic inch of diſtilled water, under the circumſtances above mentioned, by Mr. Troughton's weights *.

I think it may now be concluded, that the variety in the experiments of the cylinder and the cube, (§. 24.) does not proceed from the different depths † in the water, at which they were made; at leaſt, that the preſſure of 3 inches, in perpendicular height of water, does not render that fluid more denſe by $\frac{1}{20000}$ part, which may be reckoned an inſenſible quantity; but that this variety *did* proceed from a difference in the yielding of the ſides of the cube and the cylinder. And laſtly, I hope it may be truſted, that the weight of a bulk of water = the ſphere, has been determined to within $\frac{1}{30000}$ of the whole, and probably to within half that quantity.

(§. 32.) Having then, through the means of Mr. Whitehurſt's obſervations, and of his own inſtrument, aſcertained the length of his propoſed ſtandard, in the latitude of London, 113 feet above the level of the ſea ‡, under a denſity of the atmoſphere correſponding to 30

* But, as will appear hereafter (§. 41.), theſe weights are too light, when compared with the ſtandard in the houſe of commons, by about 1 in 1523,92; the correction, therefore, for this difference, would be = 0,165 grain, to be deducted from - - - - - 252,587 grains.
— ,165

And the weight of a cubic inch of diſtilled water, in grains of the parliamentary ſtandard, will be - - - - - - - - = 252,422

† By means of an alteration and addition to my apparatus, ſince the experiment above mentioned was made, I have been able to repeat it at greater depths below the ſurface of the water, viz. when the centre of the ſphere was 5 inches, 13 inches, and 21 inches, below, without any appearance of water having a ſenſible difference of denſity at different depths. The veſſel I uſed for this purpoſe was of wood, 32 inches high, and 12 ſquare, containing 16 gallons, with two ſides of plate-glaſs, to admit the light; and the wires by which the ſphere were ſuſpended were 45 inches long, and ſtronger than before, viz. 100 inches of the ſingle wire weighed 24,14 grains; and due allowance was made for the different weight of the ſcale and wires, in air and water, from actual experiment.

‡ The height, as I have been informed, of the room of Mr. Whitehurſt's obſervations.

inches

inches of the barometer, and 60° of the thermometer, which is full as ſatisfactory, for all practical purpoſes, as if it had been done *in vacuo* *, which I conceive to be nearly impoſſible; and, having determined the weight of any given bulk of water, compared with this common meaſure, I believe it now only remains, to aſcertain the proportion of this common meaſure and weight, to the commonly-received meaſures and weights of this kingdom.

(§. 33.) It is perfectly true, that if I choſe to indulge in fanciful ſpeculation, I might neglect theſe compariſons, as an unphiloſophical condeſcenſion to modern convenience, or to ancient practice, and might adopt ſome more magnificent integer than the *Engliſh pound* or *fathom*; ſuch as the *diameter* or *circumference* of the world, *&c. &c.* and, without much ſkill in the learned languages, and with little difficulty, I might ape the barbariſms of the preſent day. But in truth, with much inconvenience, I ſee no poſſible good in changing the quantities, the diviſions, or the names of things of ſuch conſtant recurrence in common life; I ſhould, therefore, humbly ſubmit it to the good ſenſe of the people of *theſe* kingdoms at leaſt, to preſerve, with the meaſures, the language of their forefathers. I would call a yard a yard, and a pound a pound, without any other alteration than what the preciſion of our own artiſts may obtain for us, or what the lapſe of ages, or the teeth of time, may have required.

(§. 34.) The difference of the length of the two pendulums, from Mr. Whitehurſt's obſervations, appearing to be 59,89358 inches, on Mr. Troughton's ſcale; and a cubic inch of diſtilled water, in a known ſtate of the atmoſphere, having been found to weigh 252,587 troy grains according to the weights of the ſame ariſts, it remains only to determine the proportions of theſe weights and meaſures, to thoſe that have been uſually, or may be fitly, conſidered as the ſtandards of this kingdom; and herein a ſmall diſcrepancy between themſelves, in theſe authoritative ſtandards, will have no influence on the general concluſion I propoſe to draw; which is, not ſo much to ſay what *has* been the ſtandard of Great Britain, as what it *ſhall* be henceforward, and may be immutably *ſo*; and which ſhall differ but a very ſmall quantity, and that an aſſignable one, from thoſe that have been in uſe for two or three hundred years paſt. By theſe means, no inconvenience would be produced from change of terms, or ſubdiviſions of parts, or from ſenſible deviation from ancient practice: all that will be done, will be to render that certain and permanent, which has hitherto been fluctuating, or liable to fluctuation. To give effect and energy to theſe ſuggeſtions, is the province of another power.

* It is perfectly true, that this ſuppoſes the experiment to be made with a pendulum ſimilar to Mr. Whitehurſt's.

(To be concluded in a future Number.)

III.

On certain uſeful Properties of the Oxygenated Muriatic Acid. By the Rev. ALEXANDER JOHN FORSYTH*.

THE oxygenated muriatic acid has of late been employed with much ſucceſs in the art of bleaching and diſcharging colour; but I do not know that any one has hitherto obſerved that its combinations with alkalies and earths, not only poſſeſſes the property of diſcharging colour, but alſo of brightening and rendering many colours much more intenſe.

As I hope that information on this ſubject may be acceptable to ſome of your readers, I ſhall give you an account of a few experiments made with the oxymuriate of carbonated pot-aſh with an intention not to diſcharge, but to brighten colours. I preferred this ſalt, having a conſiderable quantity of it ready made, and from a few experiments tried with the oxymuriates of carbonate of ſoda, ſoda, and lime, I thought they were more apt to diſcharge the colour, and ſeldom produced it ſo clear as the former. Until very lately, I did not think of making any experiments with the oxymuriate of ammoniac, but from theſe I am led to ſuppoſe that it will have a ſtill better effect than the oxymuriate of carbonated potaſh.

I diſcovered this property of the oxymuriate of carbonated potaſh by accident; having dropped a little of the aqueous ſolution of this ſalt upon clean writing-paper, when the paper became dry, I was ſurpriſed to ſee my ink write much blacker where the ſaline ſolution had ſpread, than any where elſe. I then ſpread ſome of the oxygenated muriatic acid upon the ſame paper, but here the ink became lighter coloured than uſual. I mixed a little ink in a phial with clear water, and dropped into it a ſmall quantity of the above ſaline ſolution; at firſt, the ink became remarkably black, but whenever it had attained its greateſt intenſity, the very next drop, in ſome degree, diſcharged the colour. After many trials, being perſuaded that this ſaline ſolution when properly uſed would improve ink, I filled a quart bottle with the thinneſt part of common ink poured off from the ſediment, and made it as black as poſſible with this ſaline ſolution. It has now ſtood without being moved for two months, has let fall no precipitate, writes remarkably black (which to many people would be a very great recommendation), flows eaſily from the pen, and continues upon the paper much blacker than any patent ink I have ever ſeen of the ſame thickneſs: ink with little gum in it anſwers beſt, becauſe the ſaline ſolution precipitates from gum a white ſubſtance, inſoluble in water. It is likewiſe proper, not to add ſo much of the ſaline ſolution as to make the ink perfectly black all at once, for it improves by ſtanding in the bottle, and there is leſs danger of any of the colour being diſcharged. Of the ſaline ſolution which I uſed, two drachms and forty grains was ſufficient for a quart bottle of ink, or one-fifth part of the weight of the ſulphates of iron and copper uſed in the compoſition of the ink to be improved: this proportion occaſions no precipitation of the metallic oxids.

* Communicated by the Author.

Having

Having remarked the good effect which the oxygenated muriate of carbonated potash produced upon ink, I next tried if it would improve the colour of the substances commonly used in dying. The aqueous extract of galls by a small addition of this saline solution at first becomes a little white and turbid, a little more immediately renders it as dark coloured as it becomes by long exposure to the action of the air and light; still more of the saline solution discharges the dark colour, and changes it to a kind of yellow: but galls require a much greater quantity of the salt to discharge their colour, than any vegetable colouring substance I have tried. The colours of logwood and weld, are rendered much brighter and more intense by the saline solution: cochineal and archil, are improved: the colours of brazil wood and madder, are little altered. But if a solution of alum be poured into the aqueous extract from any of the abovementioned substances, and suffered to remain at rest until no more precipitate falls down, and if the clear liquor be then poured off, and a little of the saline solution mixed with it, a very abundant and better coloured precipitate immediately subsides. By adding, alternately, solutions of alum and oxygenated muriate of carbonated potash, the whole of the colouring particles may be precipitated from the liquor, and thus a much more abundant and better coloured precipitate procured, than could have been got by using alum alone. Great care must be taken not to add too much of the saline solution at one time, for if any of the colour is discharged, it cannot be recovered. It is likewise proper, to add little of the alum at one time, for too much of it injures the colour, and the first precipitates are generally the best. No heat must be applied after the addition of the saline solution: the colour of all the preparations I could make of indigo, was injured or discharged by being treated with this saline solution.

As yet, I have tried the effect the solution of this salt would have upon the dying of cotton with weld only, and in every instance it has been an improvement where the small pieces of cotton were of the same quality, immersed in the same mordants and colouring baths, for the same length of time, with this difference only, that before one of them was taken out, a little of the saline solution was added to the colouring bath, the colour was brightened by the addition, and about one-third part less weld, produced the same shade as when none of the saline solution was used. The bath must be cold before the saline solution is added, otherwise the colour will be injured; and I imagine that this salt will be employed to most advantage, where the stuff to be dyed is immersed in a cold colouring bath. It certainly has a tendency to extract the colouring particles from the dye, and to brighten them when cold, but in every case injures, and, in some cases, entirely discharges their colour when warm.

I cannot precisely say, to what degree the carbonated potash ought to be saturated with the oxygenated muriatic acid, so as to produce the best possible effect upon colours; but I have found, that the degree of saturation has a powerful influence, and, in many cases, changes the shade of colour completely. Different colours also require different degrees of saturation in the salt, so that a great many experiments must be tried before it can be exactly ascertained what will answer best in every case: for ink, it should be saturated to such a degree, as that

 bubbles

bubbles of air are just beginning to rise in the saline liquor in the operation of making it: or if it is saturated above that point, a little more alkali may be added, or the phial in which it is preserved, may be left open until the air-bubbles cease to rise.

I have observed in all the experiments made with this salt, that it produces the same effect upon most colours as exposure to the air and sun, with this difference only, that its operation is much more rapid and complete: therefore, I have accounted for its effects, by supposing that when a proper quantity of the salt was added, the colouring particles attracted as much of oxygen from the salt, as was sufficient to give them their greatest degree of intensity and brightness; but when too much of the salt was added, so much oxygen united with the colouring particles as to oxydate them completely, and destroy their colour: probably the indigo (the colour of which is more or less discharged by all proportions of this saline solution) is sufficiently oxygenated in the operation of making it.

As in all the experiments tried upon colours, the oxygenated muriate of carbonated potash produced a better effect than the oxygenated muriate of potash; it is probable that the carbon, or carbonic acid, acts a very important part. What renders this more probable is, that charcoal, when used for purifying any substance, but particularly ardent spirits, extract of galls, or water become putrid, by standing too long in glasses, with flowers, &c. if left on the filter, exposed to the air and sun, and not suffered to dry too quickly, assumes a variety of beautiful colours, principally blue, yellow, and purple. When the colour, therefore, is improved by the oxymuriate of carbonated pot-ash, both the oxygen and carbon may unite with the colouring particles, and produce a joint effect.

The oxymuriate of carbonated potash, when not used in excess, precipitates many of the metals from their solvents, with a great deal of colour, and, in many cases, may be advantageously used to detect the presence of a metallic oxyde dissolved in water.

Soap is improved by mixing it with a small proportion of this saline solution; common soap, if well mixed with it in a mortar till it becomes white, answers better for washing the hands, or shaving, than any of the kinds of soap most strongly recommended for these purposes.

All the fat and expressed oils are rendered as white as milk, and partly soluble in water, by mixing them with this saline solution; and they may be restored to their former state, by a small addition of any of the mineral acids, excepting a small quantity of a mucilaginous substance, which remains for a while suspended in the water, and at last collects upon its surface: common whale-oil treated in this way, loses much of its offensive smell, and becomes better.

These are a few of the observations I have lately made upon the effects produced by oxygenated muriate of carbonated potash. Though I have not had an opportunity of making so many experiments, or of considering their results with all that attention which they seem to deserve, yet as many of them are entirely new to me, and promise to be useful to society, I have presumed to lay them before the public, even in their present imperfect state, through the medium of your very excellent publication.

Belhelvie, by Aberdeen, June 2, 1799. A. I. F.

On

IV.

On the Management of Fire, particularly with Regard to the Construction of Boilers. By BENJAMIN, COUNT OF RUMFORD*.

THE sixth of Count Rumford's experimental essays is divided into six chapters. In the first, the curious and interesting nature of the subject is displayed, and the importance of œconomy in fuel, is strongly impressed. It is stated, in general terms, that no less than seven-eighths of the heat which might be produced and usefully applied from any given quantity of fuel is lost in the common methods of application. But not to leave the subject to rest upon general induction, the author proceeds to relate various interesting experiments which shew the difference of effect with the same vessel. Accordingly as the fire was applied in the common method without confining it, or according to the improved methods of the Count, the proportions of fuel required to raise the temperature of equal quantities of water, differed incomparably more than common observers might have been led to expect. In this first chapter, the Count enters into the midst of his subject, by giving a description of the arrangements in the kitchen of the house of industry at Munich; the construction and judicious provisions of the fire-place, the form of the vessels and their covers, and the saving of heat, by causing the steam, and even the smoke, to perform useful offices. He states the advantages of wooden boilers in many cases, over those of metal, and after an interesting detail of particulars, we arrive at the valuable fact, that the quantity of fuel consumed in this establishment is only one-tenth part of what is required to produce like proportional effects in private families. This narration is followed by accounts of some of the provisions in the construction of the best fire-places, and the figure of the boilers, and is concluded by a confession, which does honour to the understanding and the principles of the Author: namely, that the want of method in this chapter, is intentional; that he was desirous rather of writing a useful book, than a splendid performance; for which purpose he rather chose to decoy the reader into a situation where he should have an inviting view of the whole prospect, than to present a regular series of elementary principles to the multitude, who have neither time nor patience to labour through so abstruse a subject.

The second chapter accordingly presents us with a popular and perspicuous account of the generation of heat in the combustion of fuel. Whether heat be matter or motion, forms no part of the present discussion; the laws of its action constitute the object of practical research. Whether the heat be afforded exclusively by the air, which is decomposed in combustion, or not, it is evident that the quantity will be greater or less, accordingly as the combustion or

* In consequence of the letter received from my correspondent W. see p. 86 of the present volume, I applied to Count Rumford, for permission to copy the engravings which constitute plate VIII. of the present number, which he granted with the utmost readiness. As the whole of the present article consists of abridged matter from his sixth essay, or such remarks and facts as I have received from him in conversation, I have thought it most proper, to subjoin his name to the title, though I have, for the sake of conciseness, used my own language, and have spoken of him in the third person.

decomposition

decompoſition of the fuel is more complete. This greatly depends on the ſtream of air, which paſſes through the burning fuel; that is to ſay, upon the velocity of this ſtream, which muſt be definite, in order to produce the greateſt poſſible effect with fuel of any particular kind. Without air there is no combuſtion; a ſmall portion of air will maintain the combuſtion but feebly; a greater quantity will give intenſity to the heat, and rapidly decompoſe the air and the fuel; a ſtill greater quantity will carry off more heat than it can generate, that is to ſay, it will blow the fire out *. The current of air, by which a fire is excited, may be made to flow by mechanical means, ſuch as bellows; or by the ſtatical effect of its own expanſion. This laſt is the common proceſs operated by chimnies; and it is of great practical advantage to be able to regulate this current. If we ſuppoſe the dimenſions of the fire-place, and the height of the chimney, to be ſuch as are beſt calculated to produce a rapid combuſtion of the fuel, it may, in many inſtances, be deſirable to moderate that combuſtion, by diminiſhing the quantity of air; this is done by perfectly cloſing the door or aperture, through which fuel is conveyed to the fire-place, and by ſecuring a command of the other two paſſages, namely, that of the aſh-hole under the grate and that of the chimney, by proper regiſters, which may be opened to any required extent, or cloſed at pleaſure. By theſe arrangements, the combuſtion is governed by the operator, and may be entirely ſtopped by cloſing both regiſters. The Author obſerves, that nearly the ſame effects as thoſe of the chimney regiſter may be produced, by cauſing the ſmoke to deſcend ſeveral feet after it has quitted the fire-place before it aſcends again; and from a late converſation with him, I underſtand that this deſcent is of the utmoſt importance and value in its effect. As the cooled air or vapour always occupies the loweſt place, it muſt follow, that no part of the ſtream can paſs up the chimney, till it has imparted ſo much of its heat, as to cauſe it to deſcend to the bottom; whereas, in the ordinary form of chimnies, it is the moſt heated part which flies immediately and rapidly up the chimney. Many fire-places have been greatly improved by this ſimple addition. The ſize of the fire-place, as well as of the fuel, are both of conſiderable importance. The air, which in common conſtructions is ſuffered to paſs over the fire, is a thief which carries off heat without aſſiſting the combuſtion: a large grate, partly covered with fuel, produces a ſimilar effect by admitting air through the vicinities, which ſimply becomes heated by robbing the boiler, and the reſt of the apparatus with which it may come in contact. The Count recommends a grate in the form of a ſegment of a ſphere, which affords the advantage of the fuel rolling to the loweſt part, as it becomes ſmaller; and he gives the form of a cone to the paſſage beneath the grate, converging downwards, till the diameter becomes only one third of that of the grate. Inſtead of the grate itſelf, he has introduced earthen pans perforated with holes to admit the air, and thinks they anſwer even better than the grates themſelves.

The heat generated in the combuſtion of fuel, manifeſts itſelf in two ways; namely, in the hot vapour which riſes from the fire, and in the rays which paſs off in ſtraight lines in all directions. It is not known what proportion this radiant heat, as it has been called, bears to that

* Philoſophical Journal, I. 515

which

which paſſes off in flame and heated vapour. It ſeems probable that it may vary according to the volatility of the combuſtible matter and other circumſtances; but, in general, it ſeems that the quantity of the former is much leſs than that of the latter. Open fires warm apartments by their radiant heat only, all the heat of the vapour being entirely loſt up the chimney. It is a remarkable fact, that radiant heat is emitted in great abundance by all bodies which are capable of ignition, whether they be ſolid or fluid, combuſtible or incombuſtible.

The means of confining heat and directing its operations, conſtitute the object of the third chapter. It is well known that heat paſſes more ſpeedily through ſome bodies than others. The uſeful applications of this property are ſeen in a variety of inſtances; among which, the wooden handle of a tea-pot, and the cloth made uſe of in handling hot iron, are familiar examples. The Author mentions ſome others, and remarks that the conducting powers of no two bodies, with regard to heat, are exactly alike.

To confine heat, is nothing more than to prevent its eſcape out of the heated body, by ſurrounding that body with a covering, through which it cannot readily paſs. If a covering could be found perfectly impervious to heat, there ſeems no reaſon why the heated body, thus defended, ſhould not continue hot for ever; but it is not probable that any ſuch body exiſts.

All metals are remarkably good conductors. Wood, and, in general, all light ſpongy bodies, or ſuch as contain fluids in their interſtices, are bad conductors *. Mercury, water, and all fluids, are conductors by circulation, but not perceptibly ſo in any other way. Air and the elaſtic fluids are very ſlow conductors, even when permitted to circulate; probably on account of their very ſmall maſs: pulveriſed bodies conduct worſe than the ſame bodies in the aggregate. Very dry powder of charcoal is one of the beſt we know; but common air is the ſubſtance employed by nature to confine heat, and is certainly the beſt which can be uſed.

The warmth of the fur of beaſts, is undoubtedly owing to the air entangled or confined in their interſtices †. Double windows and walls, ſo uſeful in cold countries to confine heat, and in hot climates to prevent its admiſſion, operate in the ſame way ‡. The Author proceeds to give a ſhort account of his experiments formerly made on theſe ſubjects, and ſhews by a curious experiment, that ſteam is not a conductor of heat in any reſpect, except by the inteſtine motion of it parts. The ſame experiment is applied to ſhew that heated air and vapour, are alſo non-conductors, and may be uſed with the greateſt advantage in our operations for confining heat.

Theſe intereſting ſpeculations lead in the fourth chapter to an enquiry how the heat is communicated from flame to other bodies. If the conducting quality of bodies be not altered by any increaſe of temperature which does not change their chemical combination, it will be proper to conſider flame as a hot wind. It is neceſſary, in order that a fluid ſhould receive or impart heat, that all its parts ſhould ſeverally come into contact with the body which gives or receives. Hence a hot body immerſed in air is not cooled except ſo far as the air has motion; and the

* Philoſophical Journal. I. 289-295

† Philoſophical Tranſactions, 1792.

‡ Double walls and windows are ſcarcely uſed in hot countries. I have ſeen light double ſtructures of bamboo in India. N.

effects

effects of wind, or a blaft, becomes active in carrying off the heat. If flame be merely a hot wind, it will follow that the ftream of a blow-pipe, when ufed to impel the flame and not to excite the fuel, muft probably act by impelling the ignited vapour with greater force and quantity againft bodies, and no otherwife; and, confequently, that carbonic acid gas, or any other elaftic fluid, will have the fame effect under thefe circumftances as oxygen. The Count found this to be the cafe with thofe very fluids, as well as with atmofpheric air, urging the flame by the blow-pipe, and ufed to fufe the end of a ftick of glafs. It is probable that this refult may require to be modified, and the experiments varied, in order to reconcile it with the effects produced by the ufe of oxygen, in fufing filex and other incombuftible bodies by Lavoifier, Erhman, and others; but there is no doubt that it is ftrikingly conclufive with regard to the practical object, to which our author applies his reafoning. *The boiler to be heated muft not only expofe as large a furface as poffible to the flame, and hot vapour; but it muft be of fuch a form as to caufe the flame which embraces it to impinge againft it with force, to break againft it, and to play over its furface in eddies and whirlpools.* It is therefore againft the bottom, and not the fides, that the principal efforts muft be directed.

The next chapter, V. prefents an account of experiments with fire-places and boilers of various forms and dimenfions: a philofophical and practical courfe of unparalleled felicity, whether we confider the ability and conduct of the director, or the means afforded to perform them. In order to obtain a refult capable of comparifon, it is taken for granted that equal quantities of fuel fimilarly expended, will raife the temperature of water through the fame number of degrees. Whence, by knowing the original temperature and quantity of water, together with that of the fuel expended to raife it to the boiling temperature, the refult may be expreffed by ftating the quantity of water at 32°, which would have been raifed 180° degrees by 1lb. of the fuel in that furnace *. The Count calls this the *precife refult*, and fometimes adds the quantity of water which might have been kept boiling one hour by 1lb. of the fuel; which laft, as he remarks, cannot be very exact.

From the indifpenfable motive of brevity, it becomes neceffary to refer the reader to the effay itfelf for the particulars of the refpective conftructions, and the ufeful confequences to which they point. I fhall, therefore, mention fome of the leading facts, and then give the defcription of the furnace and boiler delineated in the plate.

The fuel made ufe of was wood; and it is a fact of no inconfiderable value, that pine wood, which, weight for weight, cofts, in moft places, only half the price of beech, affords more heat in its combuftion. It was not found, as might be expected, that the faving of fuel was greater the larger the fcale of operation; but, on the contrary, the experiments fhewed that there is a maximum of effect with fingle fire-places, which will be departed from when the quantities of liquid either exceed or fall fhort of a certain definite quantity. The caufes of this are pointed

* In the form of a rule. Multiply the quantity of water by the number expreffing the degrees actually raifed; multiply the number of pounds of fuel expended by 180. Divide the firft product by the latter, and the quotient will exprefs the water which would have been raifed 180° by 1 lb. of the fuel. N.

out,

out, namely, that flame will act more effectually the greater the surface; and this is proportionally greatest in small furnaces: and, secondly, that the cooling effect of the masonry and apparatus will be greater the greater its mass; and this also is proportionally greater in small furnaces. The exact size to produce the greatest effect is not easy to be ascertained; but supposing this to be done, it will follow, as the Count has remarked to me, that in all establishments of considerable magnitude, it will be better on this and many other accounts, to heat the vessel by a number of smaller fires, instead of one large one.

In the construction hereafter to be described, the flame was at first made to circulate round the sides of the boiler; but by subsequent experiments, in which the circulation through those side flues was prevented (the hot vapour being still suffered to enter them), it was found that the effect was accelerated by the speedier draft of the chimney, without any greater expence of fuel.

In every furnace there seems to be a rate of working, at which the heat is more cheaply produced than at any other; that is to say, if there be too little fuel in the fire-place at once, the process will be longer, and the loss, by the conducting power of the apparatus, more considerable: if there be too much, the business will be performed more speedily, but at a greater expence, chiefly from the escape of heated vapour by the chimney. In some cases, it may be most profitable to save time; in others, fuel. In the experiments, no. 31 and 32, it was found, that to abridge the time one-third, there was an additional consumption of about one-eighth more fuel.

The progressive improvements in the saving of fuel, as summed up at page 121 of the essay, are very curious. With a common open fire, carefully managed, the precise result was 1.11. or 1lb. of fuel raised $1\frac{1}{10}$ lb. of water 180°. But in the experiment, no. 20. with the improved fire-place and boiler, the result was 20.1; or $20\frac{1}{10}$lbs of water, were boiled with 1lb. of wood. So that it appears that the latter process was eighteen times more economical than the former, and that seventeen parts out of eighteen of the fuel were consumed to no purpose in the first experiment. With regard to the absolute quantity of heat, the author deduces from the experiments related in Dr. Crawford's treatise, that 1lb. of charcoal would cause 57.6 lbs. of water to boil, if none of the heat was suffered to escape. And by Lavoisier's experiments, the quantities of heat generated by equal weights of charcoal and dry oak are as 1089 to 600; and the Count takes it to be nearly true, that equal weights of oak and of dry pine wood will afford equal quantities of heat. These positions afford the result, that 31,74 lbs. of water would have been brought from the freezing to the boiling heat by the combustion of 1lb. of his fuel. It follows, therefore, that in the method of boiling over common open fire, nearly 28 parts out of 29 of the heat are lost; and that in the Count's experiments the absolute loss amounted to about one-third. When the generation and loss of steam, the conducting power of the materials, and the quantity of heat which must necessarily be suffered to pass up the chimney in order to produce a draft, are considered, it may be doubted whether the economy of fuel can be carried much farther.

As the fuel of this country consists of coal and coke, and not wood, it seems requisite to state their comparative powers of affording heat by combustion. The experiments of Lavoisier,

 quoted

quoted by the author, give the following proportions *. Equal quantities of heat are produced by the combuftion of

403 lbs of cokes	or in meafure	17 of cokes.
600 — of pitcoal		10 of pitcoal.
600 — of charcoal		40 of charcoal.
1089 — of oak		33 of oak.

If pitcoal had been ufed inftead of pine-wood, the number of pounds of water made to boil by 1lb. of the fuel would have been 36,3 inftead of 20,1; and as, from the experiments of Watt, it is eftablifhed, that nearly five and a quarter times the quantity of heat is required to convert boiling water into fteam as would have raifed the fame water from the freezing to the boiling point, it will follow, that the heat generated in the combuftion of one pound of pit-coal ought to afford very nearly 7lbs. of fteam.

In all the experiments made on a very large fcale, with brew-houfe boilers, rather more than one-half the heat actually produced found its way up the chimney and was loft.

The fixth chapter of this effay contains a fhort account of a number of kitchens, public and private, and fire-places for various ufes, which have been conftructed under the direction of the author, in different places: of thefe I muft neceffarily forbear to fpeak at prefent.

Fig. 1. Plate VIII. is a front view of the new boiler of the brew-houfe called Neuheufel (belonging to the Elector of Bavaria), or rather of its fire-place and cover (the boiler being concealed in the brick-work). The infide door of the fire-place is here reprefented fhut; and, in order that it might appear, the outfide door is taken off its hinges, and is not fhown. The two vaulted galleries, A, B, in the folid mafs of the brick-work, on the right and left of the fire-place (which were made to fave bricks), ferve for holding fire-wood. The partition walls of the fire-place and the different flues, as alfo a fection of the boiler, are reprefented by dotted lines. The fmall circular hole on the left of the fire-place door is the glazed window opening into the fire-place, by which the burning fuel may be moft advantageoufly feen, without difturbing the current by any opening of doors. *a b* is the wooden curb of the boiler: *c d*, a platform on which the men ftand when they work in emptying the boiler, &c.: *e f* is a platform which ferves as a paffage from one fide of the boiler to the other. This platform, which is about 18 inches wide, is 12 inches higher than the other platforms, in order that the openings *g* and *h*, into the flues, may remain free. Thefe openings, which are opened only occafionally, that is to fay, when the flues want cleaning, are kept clofed by double brick-walls. Thefe walls are expreffed in the following figure.

Fig. 2. is an horizontal fection of the fire-place at a level with the bottom of the boiler. *a*, *a*, *a*, *a*, are four openings, by which the flues, which in the firft arrangement of this fire-place went round the outfide of the boiler, were occafionally cleaned: *b* is the canal by which the fmoke went off into the chimney. The entrance into the fire-place, and the conical perforation in the wall

* Thefe experiments, which were communicated to Count Rumford by Mr. Kirwan, are to be found in the Memoirs of the Paris Academy; I think for the year 1781.

of

of the fire-place, which ferves as a window for obferving the fire, are marked by dotted lines. The pofition of the outfide door of the fire-place is marked by a dotted line, *c d.* The circular difhing-grate is feen in its place; and the walls of the flues under the boiler are all feen. The crooked arrows in the flues fhow the direction of the flame.

Fig. 3. is a vertical fection of the boiler reprefented in the foregoing fig. 1. This fection is taken through the middle of the boiler of the fire-place, and of the cover of the boiler. A is the afh-pit, with a fection of its regifter door. B is the fire-place, and its circular difhing-grate. C is the entrance by which the fuel is introduced, with fections of its two doors. D is a fpace left void to fave bricks. E is the boiler, and F its wooden cover. *m* is the fteam chimney, which is furnifhed with a damper. R R is the vertical wall of the houfe againft which the brickwork in which the boiler is fixed is placed. *a b* is the curb of timber in which the boiler is fet. The manner in which the cover of the boiler is conftructed, as well as its form, and the door and windows which belong to it, are all feen diftinctly in this figure.

Fig. 4. is an horizontal fection of this fire-place taken on a level with the bottom of the flue which goes round the outfide of the boiler, in which flue, before the fire-place was altered, the flame circulated. The flues under the boiler are, in this figure, indicated by dotted lines.

Fig. 5. is an horizontal fection of the fire-place of the brew-houfe boiler, at a level with the top of the flues under the boiler, *after the flue round the outfide of the boiler had been ftopped up*, or rather the flame prevented from circulating in it. This figure fhows the actual ftate of the fire-place at the prefent time. The crooked arrows fhow the direction of the flame in the flues.—*a*, *b*, are the two canals (each of which is furnifhed with a damper) by which the fmoke goes off into the chimney;—and *c*, *c*, *c*, *c*, *c*, *c*, are fix fmall openings communicating with the flues, by which the flame and hot vapour can pafs up into the cavity on the outfide of the boiler, which formerly ferved as a flue.

Fig. 6. is a front view of the afh-pit door of this brew-houfe fire-place, with its regifter. This door is clofed by means of a latch of a particular conftruction, which is fhown in the figure.

Fig. 7. is the door without its regifter;—and

Fig. 24. the circular plate of the regifter reprefented alone.

In conftructing thefe regifter doors, and in general all iron doors for fire-places, great and fmall, the door fhould never be fhut in a rabbet, or groove, in the frame, but fhould merely *fhut down on the front edge of the frame*, which edge, by grinding it on the flat furface of a large flat ftone, fhould be made quite level to receive it. If this be done, and if the plate of iron which conftitutes the door be made quite flat, and if it be properly fixed on its hinges, the door will always fhut with facility and clofe the opening with precifion, notwithftanding the effects of the expanfion of the metal by heat; but this cannot be the cafe when the doors of fire-places are fitted in grooves and rabbets. Where the heat is very intenfe, the frame of the door fhould be made of fire-ftone; and that part of the door which is expofed naked to the fire fhould be covered either with a fit piece of fire-ftone, faftened to it with clamps of iron, or a fufficient number of ftrong nails, with long necks and flat heads, or of ftaples, being driven into that fide of the plate of iron which forms the door which is expofed, fhould be covered with a body about

 two

two inches thick of ſtrong clay, mixed with a due portion of coarſe powder of broken crucibles, which maſs will be held in its place by the heads of the nails, and by the projecting ſtaples. This maſs being put on wet, and gently dried, the cracks being carefully filled up as they appear, and the whole well beaten together into a ſolid maſs, will, when properly burned on by the heat of the fire, form a covering for the door, which will effectually defend it from all injury from the fire; and the door, ſo defended, will laſt ten times longer than it would laſt without this defence. The inſide doors of the two brew-houſe fire-places which the Count fitted up at Munich are both defended from the heat in this manner; and the contrivance, which has anſwered perfectly all that was expected from it, has not been found to be attended with any inconvenience whatever.

I have lately ſeen the patterns at Count Rumford's for caſting a *regulating door* of his invention, which poſſeſſes the advantages of ſimplicity, cheapneſs, and effect, in a very ſtriking degree. It has neither hinges nor ſliding groove: it cloſes with utmoſt preciſion; and from the very ſmall quantity of work it requires to finiſh it, the price ought not to exceed two or three ſhillings. I am not at liberty to anticipate the inventor, who will communicate it to the public as ſoon as this can be moſt effectually done, namely, by directing them to a place where they may ſee what they are to imitate.

V.

Extraction of Sugar from Carrots, &c.—Experiments on Barytes and Strontites.

Extract of a Letter from Mr. William Henry of Mancheſter, to the Editor, dated June 20th, 1799.

MY friend, Doctor Peſchier of Geneva, who is now at Vienna, ſends me the following information reſpecting the extraction of ſugar from carrots, &c. I ſubjoin alſo ſome experiments of my own, on the ſulphate and Pruſſiate of barytic and ſtrontian earths.

" Profeſſor Jacquin raſped and expreſſed 40lbs. of roots of carrots (daucus carota). The juice, which was pretty clear, and of an agreeable taſte, was evaporated to the conſiſtence of ſyrup; clarified with ſome albumen of egg, after being mixed with ſome ounces of quick-lime; and filtrated. After cooling a few days, it contained, in the bottom of the veſſel, ſmall cryſtals, which, when waſhed in cold water, were white and diaphanous. To theſe ſucceeded others of a yellow colour. The whole of the cryſtals amounted to 12 oz.; they taſted exactly like the pureſt ſugar, and preſerve their dryneſs to this day. There remained about 4lb. of a brown ſugar perfectly agreeable to the taſte.

" Second experiment. Three roots of beta cycla altiſſima, weighing, after having been peeled, 4lb. were pounded and expreſſed. The juice was thick, mucilaginous, and ſweet, but diſagreeably mixed with the taſte of turnips. It was evaporated, according to the method of Mr. Achard of Berlin, without any addition, and ſkimmed at intervals. This ſcum was produced

duced by the albumen, which, you know, is to be found in moſt vegetables, and coagulates at a certain degree of heat. When thus cleared, it had completely loſt its ſharpneſs, but after ſtanding at reſt for ſome days, in a cool place, it ſhewed no tendency to cryſtallize. It was then evaporated in a water-bath, and afforded a paſte of a light brown colour; that deliqueſced in a ſhort time, and reſumed the conſiſtence of a thick ſyrup, to the weight of ½lb.

" Third experiment. 11lb. of the ſame roots were expreſſed, as in the ſecond experiment, but were purified as in the firſt. They gave 3lb of a clear ſyrup, which afforded 3 oz. of ſmall cryſtals of ſugar, of a pleaſant taſte. The remaining ſyrup was brown and very ſweet. It is to be obſerved, that as theſe experiments were made in the ſpring, the roots had already begun to ſhoot, and contained rather more mucilage than ſugar. It is neceſſary, alſo, to inform you, that the little plate, affixed to the work of M. Achard, repreſents the beta vulgaris, while the deſcription is that of beta cycla abtiſſima, which is very different. On the whole, the extraction of ſugar from the carrot and turnip, on a larger ſcale, promiſes to become an object of profitable ſpeculation.

" It has been diſcovered lately, that the water, remaining after obtaining ſtarch, contains a large quantity of ſugar."

I know not whether you will think the following experiments on barytes and ſtrontites worthy of a place in your journal.

The analyſis of the native ſulphate of ſtrontites from the neighbourhood of Briſtol, which has been ſo well executed by Mr. Clayfield, eſtabliſhes, beyond all controverſy, the nature of that compound. If any teſtimony were required to the accuracy of Mr. C.'s experiments, I might ſtate, that early laſt winter I read to the Literary and Philoſophical Society of this town, an analyſis of the ſame ſubſtance, from which I aſſigned as its components, in 100 grains, 41½ of ſulphuric acid, 58¼ pure ſtrontites, and one-fourth of a grain of water, beſides a ſcarcely notable portion of iron. Theſe proportions do not differ more than a quarter of a grain from thoſe of Mr. C.

If Mr. Clayfield's analyſis be imperfect in any reſpect, I think it is in the deficiency of proof that the mineral under examination contains no portion whatſoever of barytes. To aſcertain the preſence of the laſtmentioned earth, Mr. C. poured on the precipitate obtained by boiling the native ſulphate of ſtrontites with carbonate of potaſh, " a quantity of muriatic acid, ſufficient to diſſolve only a few grains of the earth. Had any barytes been preſent, it would have been taken up in preference to the ſtrontites, from its ſuperior affinity for the acid; the ſolution, however, after digeſtion for ſeveral hours, ſtill cryſtallized in needles, and afforded a copious precipitate to barytic lime-water." (Phil. Journ. III. 38.) It will be found, however, that on the addition of dilute muriatic acid to a mixture of carbonates of barytes and ſtrontites, a portion of each earth is diſſolved, as might indeed be expected from ſeveral known facts reſpecting chemical affinity, which ſhew that facility of combination and ſtrength of affinity by no means keep pace together. At a certain point of concentration, the muriatic acid even diſſolves ſtrontites in preference to barytes, on account of the greater ſolubility of the reſulting compound.

compound.—The method by which I examined whether the mineral under examination contained barytes, was as follows.

1. The ſpar was decompoſed by digeſtion with carbonate of potaſh, as in Mr. Clayfield's experiments. The ſubſtance thus obtained, conſiſting of the earthy part of the mineral combined with carbonic acid, I ſhall call the precipitated earth.

2. Nitric acid was gradually diluted with water, till it became capable of diſſolving ſtrontites but not barytes; for to diſſolve the latter, much further dilution is neceſſary. The precipitated earth was wholly diſſolved by this acid, which would have left undiſſolved any portion of barytes.

3. Barytes, in a pure and cauſtic ſtate, according to Dr. Hope (Edinb. Tranſact. vol. iv.), precipitates ſtrontites and all other earths from muriatic acid. I expoſed, therefore, to an intenſely ſtrong fire, a portion of the precipitated earth; and thus deprived it of its carbonic acid ſo completely, that it readily diſſolved in hot water, and cryſtallized on cooling. Now, if the peculiar mineral under examination contained any barytes, this watery ſolution of its pure earthy part, on being added to a ſolution of the precipitated earth in muriatic acid, would occaſion a ſeparation of ſtrontites. On making the experiment, however, no precipitation took place, which ſhows that the watery ſolution contained no barytes, but conſiſted of pure ſtrontites only. The ſame fact alſo proves that the muriatic ſolution contained neither lime, magneſia, alumine, nor any of the metals, any one of which would have been precipitated by the ſtrontitic water.

The following experiments are, perhaps, worth relating, as they furniſh additional proof that barytes and ſtrontites are really diſtinct and peculiar earths.

Dr. Hope, and I believe the generality of writers on this ſubject, aſſert that barytes (in contradiſtinction to ſtrontites) is precipitated from muriatic acid by the Pruſſiate of potaſh. But Mr. Kirwan, on grounds which he has not ſtated, teſtifies the contrary. "Moſt earths," he obſerves (Elements of Min. I. 3.), "are ſoluble in ſome acid or another, and many in all acids; the Pruſſian alkali can precipitate none of them from theſe ſolutions; whereas it precipitates all metallic ſubſtances except platina from their acid ſolvents. This forms a diſtinct line of ſeparation between earths and metals. The exception, formerly made in favour of barytic earth, is now found to have ariſen from a miſtake." As the deciſion of this queſtion is of ſome importance, from its influence on mineral analyſis, I determined to ſatisfy myſelf of the truth by the proper experiments; the relation of barytes and ſtrontites to the Pruſſic acid, not having been an object of attention either with Dr. Hope, M. Klaproth, or Meſſ. Pelletier, Fourcroy, and Vauquelin.

1. *Relation of Barytes to the Pruſſic Acid.*

To a ſolution of cauſtic cryſtallized barytes in hot water, Pruſſiate of iron, which had been well waſhed with boiling diſtilled water, was gradually added, till it ceaſed to be diſcoloured. The ſolution of barytes acquired a yellow tinge, reſembling that of Pruſſiated potaſh. It was gently evaporated, and when cold, a number of cryſtals had formed, which were ſlightly tinged by iron, and appeared to be very minute rhomboidal parallelopipeds.

These

Thefe cryftals were fparingly foluble in water, 4 oz. of which, at 65° Fahrenheit, took up barely one grain; and of boiling water, each ounce diffolved only between five and fix grains. The watery folution gave a precipitate of fulphate of barytes, on adding fulphuric acid, and of Pruffiated iron, with the folutions of that metal. In a low red heat, the falt was foon changed into a black mafs, confifting of carbonate of barytes and charcoal. The cryftals diffolved readily in diluted nitric and muriatic acids, apparently without decompofition, for the folutions precipitated falts of iron. They were decompofed by carbonate and fulphate of potafh, a double exchange of principles enfuing.

Part of the precipitate, obferved on adding the Pruffiate of potafh to muriated barytes, is doubtlefs often occafioned by the prefence of fulphate of potafh in the Pruffiated alkali; an adulteration, which it is difficult entirely to avoid; but that a double elective affinity is exerted between the two falts, clearly appears on examining the precipitate. After being repeatedly wafhed with diftilled water, muriatic acid will be found to diffolve the greater part of it; and the folution of the precipitate, in this acid, betrays the prefence of Pruffiate of barytes, on applying the proper tefts. It may, therefore, be received as an eftablifhed fact, that an infoluble, or difficultly foluble Pruffiate of barytes is formed, on adding Pruffiate of potafh to the muriated earth, a property in which barytes differs from all other earthy bodies, and refembles the metals: the fimple affinity of potafh for Pruffic acid is fuperior to that of barytes; for cauftic barytes does not precipitate a perfectly pure Pruffiate of potafh. Barytes attracts Pruffic acid more powerfully than lime, for Pruffiate of lime is decompofed by pure barytes; the order of thefe affinities, I believe, has not before been afcertained.

2. *Relation of Strontites to the Pruffic Acid.*

A folution of pure cryftallifed ftrontites deprives Pruffiate of iron of its tinging acid; but the folution of Pruffiated ftrontites is much lefs difpofed to cryftallife than the homologous barytic falt. Indeed I have repeatedly evaporated folutions of Pruffiated ftrontites very low, without having been able to obtain any cryftals. I therefore expelled from one portion the whole of the water by a gentle heat; the dry mafs thus obtained diffolved readily in water at 65°, an ounce of which took up 120 grains, and even then did not appear to be faturated: but having no more of the falt, I was prevented from afcertaining its exact folubility. The dry falt does not deliquiate: in its other properties, it refembles Pruffiated barytes.

A folution of cauftic ftrontites does not precipitate Pruffiated lime, as barytes does; ftill, however, a decompofition probably takes place, but is prevented from being apparent by the folubility of the new compound. This may, alfo, be the cafe, when muriated ftrontites is added to Pruffiate of potafh, or of lime.

Pure barytes, when added to Pruffiated ftrontites, occafions a precipitate, but a much lefs abundant one, than might be expected; hence it is probable that the affinities of thefe two earths for Pruffic acid are nearly equal in ftrength, as Dr. Hope has fhewn they are, with refpect to carbonic acid.

Dr. Pefchier acquaints me, that the fulphate of ftrontites abounds in Syria and in Hungary, but not the carbonate.

Concerning

VI.

Concerning those perpetual Motions which are producible in Machines, by the Rise and Fall of the Barometer, or the Thermometrical Variations of the Dimensions of Bodies. W. N.

(Concluded from p. 128.)

SUPPOSE this apparatus to be put together at a certain temperature in the day-time; and that in the night the temperature becomes colder; in this case, the curvature of all the bars will diminish, and the distance between A and B will be increased by the action of the intermediate spring: but as the plate A is prevented by the click C from receding, the plate B will be pushed forward, and the interior cylinder will gather a certain number of its teeth upon the click D. The next day, when the temperature again rises, all the expansion bars will bend, and the space between A and B will be diminished; this, however, cannot happen by the motion of B, which is held fast by the click D. The external part will, consequently, be now carried forward, and will act upon the apparatus E: a second lowering of the temperature, by whatever cause, will occasion the interior part again to advance; and, in this manner, the accumulations of force may be incessantly reiterated.

Experience must determine how far the properties of these compound bars may be changed in the course of time*. It seems probable, that the mere changes communicated by the atmosphere, could scarcely produce any sensible effect; and whether this effect would be detrimental to the general result, may also be questioned. Considerations of this nature, lead to the enquiry, whether our object may not, with equal facility, be obtained by the direct push or pull of bars of metal, as in the gridiron pendulum, or that of Ellicott†.

If a succession of bars of brass were disposed round a cylindrical face of less expansible metal, so as to form an helical line from the one end to the other; or, otherwise, if we suppose a brass clock chain, with a right-lined edge, to be wrapped round such a cylinder; or again, if, instead of the cylinder, we suppose the chain to pass over a succession of rollers, whether disposed in a cylindrical system, or according to the form of a pully, the result will be the same; that is to say, the chain will contract and expand about the ten-thousandth part of its length, for every ten degrees of Fahrenheit's thermometer. In the way of rough estimate, let us, therefore, assume a cylinder of cast-iron, one foot in diameter, and one foot in length, having a groove turned in its surface, like a screw, with twelve turns in the inch, for the purpose of lodging a system of friction rollers to receive the brass chain, wrapped round it. Such a chain‡, consisting of 144 turns, would measure 450 feet §, and would contract nearly 0,54 inches for every 10 degrees, or one-twentieth of an inch for each degree of change of temperature; but as the cylinder itself contracts, the whole effect will be somewhat less than half that quantity; that is to say, each degree of the thermometer will be one-fortieth of an inch.

* Philos. Journal, I. 62.

† Ibid, p. 59, 60.

‡ And would cost about 25l.

§ For pyrometrical data, see Philos. Journal, I. 58.

The

The philosophical world is aware, that hygrometers have been made, on this principle, with cat-gut, hair, whalebone, and other materials. It seems probable, that the first of these substances would exert considerable force as a first-mover, but it would scarcely prove durable; and what is still worse, the variations of moisture in the atmosphere are little suited to operate upon machinery preserved in a case in an apartment.

If the increase of the space moved through by the expansion of metals upon the principle of Ellicott's pendulum, should be adopted instead of the compound bars in fig. 4, the project of fig. 5, may be followed. A bar of steel, A K, is fixed beside a bar of brass B I, the joinings, I K, being inflexible; but those at A, B, in the lever A F, being jointed, the difference of expansion between the two metals will be magnified at F, in the proportion of A F to A B. The lines, L C, G D E, represent a second combination of the same kind, in which the point G will have a similar and equal motion to that of F, but the bar E L, being prolonged to F, so as to bear upon the lever E F, the whole of the second combination will be pushed forward by the expansion of the first; on account of which, the motion of G will, in fact, be double that of F; by the addition of a third combination, the motion will be tripled, &c. A sufficient number of these, properly placed in the cavity, between the two cylinders, fig. 4, would afford the same consequences; but it may be doubted, whether any contrivance of this last kind could afford the same power in as small a space as that occupied by the compound bars.

I shall now proceed to form an estimate of the quantities of force communicated by these several contrivances.

The apparatus, fig. 1, plate VI. or barometrical clock, is driven by a force which may be estimated in its annual quantity from the sum of the deviations of the barometer taken from a Meteorological Journal, such as that in the Philosophical Transactions; together with the quantity of mercury so moved, which may be derived from the dimensions of the surfaces in the tube and bason. From these variations, severally, must be taken the quantity of what mechanics call lost time, or the ineffectual movement between the direct and retrograde actions on the machinery. The whole power will be measured by the entire column of variation, supposed to descend through half its height; for this will be the case when a perpendicular tube empties itself by the descent of any fluid contained in it. I have not taken the trouble to collect these elements; but it may easily be imagined, that the sum of all the variations during the year, would amount to no great quantity. I understand, from the ingenious constructor of this apparatus, that the accumulated power was not sufficient to allow the clock to be maintained by a force equal to that which drives a common watch, namely, six ounces, with the daily fall of twelve inches.

In the investigation of the power of expansion, in fig. 4, a variety of curious objects of physical and mathematical research offer themselves to our consideration. In a former part of this Journal*, it has been shewn, that the curvature assumed by a straight compound bar, having each of its parts uniformly thick, will be circular; whence it follows, from the nature

* I. 62.

of versed sines of small arcs, that the distance A B, fig. 2, will, *ceteris paribus*, be as the square of C A; and it should seem as if a considerable advantage might be derived from using the whole length of the bar, as in that figure, 2, instead of the two half-lengths in fig. 3. But it must be considered, that the effect of hammer-hardening the lower part, and wire-drawing the upper, of the compound bar C A, is twice as great at C, fig. 2, as it is at C, in fig. 3, and is shewn in the greater spring, or yielding of the parts; and that the action at A, in this figure, is doubled at the opposite extremity of the bar:—so that upon the whole, the action at A, on account of the short lever A C, fig. 3, being twice as powerful as that at A, in fig. 2, and being exerted through the space A B, fig. 3, of one-fourth part of the space A B, in fig. 2, will be half the action at the end of fig. 2. But as both extremities of the bar are made to act in fig. 3, the whole of its action will be precisely equal to that in fig. 2. The combination, fig. 3, appears, therefore, to be preferable on account of its convenient figure only.

It may also be questioned, whether these bars should be made extremely thin, or the contrary. If they be very thin, the effect of the reaction being equivalent to a pull, or push endways upon the bar, which is greater than the reaction itself, in the proportion of the length of the bar to its half thickness, it may easily be imagined, that the texture and cohesion will be most strongly affected; but, on the contrary, if the bar be very thick, the effect from change of temperature may resolve itself intirely into an action upon the parts near the contiguous surfaces without producing any flexure at all. It appears, therefore, that there is a thickness which is practically better than any other; but what this thickness may be, remains to be determined by trials. As the quantity of motion is inversely as the thickness (Philos. Journal, I. 576), and the force directly as that thickness, it must follow, that the quantity of mechanic effect in all similar bars, neither extremely thick, nor extremely thin, will be the same upon equal changes of temperature. I should give the preference to thin bars, not so much reduced as to have any perceptible spring.

If the fig. 4. be supposed of such dimensions, as that the circular arc struck through the middle parts of all the bars might be three feet in length, and the bars were each six inches long in the radial direction, with a thickness nearly equal to that of the second experiment related at the last-quoted article of our Journal; the space moved through, by each bar, upon an alteration of 146 degrees, would be about 0,05 inch, or half a tenth: but 300 of these bars might, with ease, be contained in the circular space of three feet, and these would produce a motion of fifteen inches by the same change of temperature, or one-tenth of an inch for every degree of Fahrenheit. In order to determine the force with which this change of position would be affected, we are in want of some experiments on the expansions of metals. It is generally supposed that a rod, or wire, will contract or dilate, by change of temperature, in the same manner, whether it be at perfect liberty to move horizontally, or be made to support a weight hung from its extremity, or placed on its upper end. This is, in fact, supposed to be the case in the estimates for constructing gridiron pendulums, and if it were strictly true, the power of this wheel would be constantly equal to the reaction against which it should be exerted. But it would be to little purpose to institute a calculation upon data assumed

at random: I ſhall therefore only remark, that the power of this wheel is very conſiderable, and may be encreaſed almoſt at pleaſure, by enlarging the dimenſions of the bars, in the direction parallel to the axis of the cylinder.

Whatever queſtion there may be, with regard to the force and durability of this ſyſtem of bars, neither of which I am diſpoſed to doubt, there can be ſcarcely any with regard to the ſpiral chain round the cylinder. The direct expanſion and contraction of metals is certainly very powerful, and was ſhewn in a ſtriking manner by the Rev. Mr. Jones in an experiment related, if my memory be correct, in George Adams's Philoſophical Lectures. He hung a very heavy weight to the longer end of a lever, the ſhorter arm of which preſſed upwards againſt the longer arm of another lever, and the ſhorter arm of this laſt was ſupported by a rod of metal. By this mechanical arrangement, it will be underſtood, without difficulty, that a very ſlight motion of the arm which bore upon the metal, might be attended with a very conſiderable motion of that extremity which ſupported the weight; and the dimenſions were ſuch, that this was in fact the caſe. The flame of a candle applied to the bar of metal cauſed it to expand, and carried up the load without difficulty.

Hygrometric contractions and dilatations are known to be performed with immenſe force; but want of durability in the materials, and the difficulty with regard to expoſure, which has been already mentioned, ſeem to forbid the practical uſe of this power for the purpoſes to which our attention is now directed.

The contrivance, fig. 5, may be conſidered as effectual; but the objections which have been made to Ellicott's pendulum, are ſtill more ſtrongly applicable to this, namely, that the friction, the wear, and the irregular action of the joints, muſt be hurtful to the general effect.

VII.

Eſſays on the Art of Dying, by Means of the Solutions of Tin, and the coloured Oxides of that Metal. By J. M. HAUSSMAN.*

THIS memoir of Citizen Hauſſman contains the reſults of his numerous experiments on the ſolutions of tin, and the oxydes of that metal; reſults which, as well as the obſervations of the author, are very intereſting, not only with regard to the improvement of the art of dying, but likewiſe with regard to our ſcientific knowledge of the different degrees of the oxygenation of metals, the union of their oxydes with their ſolvents, and the ſurcompoſitions of which thoſe oxydes are ſuſceptible.

The author commences his paper by announcing, that ſince the time of his reſearches concerning the Turkey red, of which a deſcription is inſerted in the twelfth volume of the Annales de Chimie, he has diſcovered a red which is no leſs ſimple than beautiful and ſolid, the proceſs of which he intends ſpeedily to publiſh.

* Abſtract from the memoir of the author bearing the ſame title, by C. A. Prieur, in the Annales de Chimie, XXX, 15, of which the preſent article is a tranſlation.

He then proceeds to give an account of his experiments, concerning the means of combining the colouring matter of madder, cochineal, and all the ſeveral dying drugs, with tin, in order to fix them immediately on piece-goods, in the way of ſolution and precipitation.

His experiments were made on the ſolutions of tin, by the nitro-muriatic, muriatic, ſulphuric, and acetous acids, and by potaſh; and he mixed the colouring decoctions of plants, in ſome inſtances, with the acid ſolutions; in others, with the oxydes precipitated from them; or, otherwiſe, he re-diſſolved in alkali the oxydes already coloured. The following is a ſummary of the principal circumſtances and effects of theſe operations.

When the fluids do not immediately afford a precipitate, he cauſes it to take place by immerſing a piece of ſilk, cotton, or woollen. The precipitation, in ſome inſtances, takes place by the ſimple mixture of the two liquids, and the reſult conſiſts of oxydes, coloured according to the nature of the vegetable ſubſtance. Theſe coloured oxydes appear likely to be uſeful in painting. In other experiments he forms theſe coloured powders, by firſt precipitating the oxyde of tin from its acid ſolution, by the mere addition of water; and the edulcorated oxyde ſeizes part of the colouring matter from the tincture in which it is plunged. Laſtly, theſe coloured oxydes being diſſolved by potaſh, and the ſtuffs impregnated with the ſolution, the colouring matter afterwards become fixed, either by ſimple expoſure to the air or by immerſion in an acid ſolution of tin or of alumine, according to the object propoſed by the operator.

The colours which are produced differ in kind, in ſhade, in brightneſs, and in ſolidity, from various cauſes, namely, the ſpecies of vegetable, the kind of acid, and the oxydation of the tin; the portion of acid, or alkali, which may exiſt in the precipitate, according to the choice of the operator; the greater or leſs proportion of water in the ſolutions; the other circumſtances of manipulation; or the ſeveral ingredients which may have been employed.

Each acid differs principally by the degree of oxydation to which it is diſpoſed to carry the tin. The nitro-muriatic acid being capable of variation in its doſe of oxygen, is, on this account, more or leſs proper to advance the oxydation of the metal. The oxydation is alſo more perfect, if the ſolvent be more concentrated, or if the action of heat be added, or if the ſolution be expedited, by adding a conſiderable quantity of the metal at one time. The oppoſite conditions are attended with the contrary effect. In order to regulate, at pleaſure, the greater or leſs oxydation of the metal, and in ſome meaſure at the will of the operator, it will be ſufficient, if, to a nitro-muriatic ſolution of tin, there be added a certain quantity of the muriatic ſolution of the ſame metal; for this laſt may eaſily be obtained at the leaſt poſſible degree of oxydation.

It is a well-known fact, that the ſpontaneous progreſs of the oxydation of the ſolutions of tin, or the addition of water, in ſufficient quantity, cauſe the ſeparation of a precipitate. This effect may be prevented when neceſſary, by leaving or adding an exceſs of acid to the ſolution; or by adding a quantity of the muriatic ſolution of tin, which has the property of ſupporting a much greater quantity of water without precipitation; or, laſtly, by adding muriate of ſoda,

or

or any other muriatic ſalt, the effect of which ſeems to be, that they ſaturate the exceſs of water which would elſe have occaſioned precipitation.

To haſten the muriatic ſolution of our metal, it is advantageous to ſprinkle it with the concentrated acid, and then leave it expoſed to the air. The ſurface of the metal becomes oxyded with ſuch rapidity, that a ſenſible heat may be produced; ariſing, no doubts from the caloric taken from the oxygen gas abſorbed from the atmoſphere.

The muriatic ſolution of tin, when concentrated, affords many cryſtals at the temperature of ice, and theſe cryſtals are permanent in a cold place. The heat of thirty or forty degrees above zero (q. of Reaumur?) liquifies them. When diſſolved in water, they afford a ſolution of tin, which may be uſed as occaſion requires. This ſolution becomes oxyded to the maximum (and will, conſequently, produce different effects on the colouring ſubſtances) if it be left expoſed for a certain time to the air, the contact being favoured by an extenſive ſurface.

The oxyde of tin, precipitated from its muriatic, or nitro-muriatic ſolution, by an alkaline carbonate, may, if not too much oxyded, be very eaſily, and without decompoſition, diſſolved in the diluted nitric or ſulphuric acids, and alſo in the acetous acid. Theſe ſolutions produce, with colouring ſubſtances, ſhades, which are more eſpecially governed by the oxydation of the tin, and are diſpoſed to change more or leſs ſpeedily in the air, by a ſuperoxydation of the metal, and according to the nature of the ſolvent. The ſulphuric acid accelerates this effect more than the nitric, and the acetous acid the moſt rapidly of any; which circumſtance ſhews that the acetic ſolution of tin, if it be required to be kept without ſpontaneous precipitation, muſt be preſerved in well-cloſed bottles; and that, on this account, it would be preferable to the muriatic ſolution for eudiometric eſſays.

The diſpoſition of the coloured oxydes of tin to change their ſhade by ſuperoxygenation from the air, appears to depend on the circumſtance that they retain a portion of the acid ſolvent. If, on the contrary, they ſhould retain a portion of alkali, the ſhade is different, and continues fixed in its firſt ſtate. Theſe precipitates do not, however, appear to unite with the carbonic acid, whether it be that this acid has very little attraction for the oxyde of tin, or whether, as is more probable, that the colouring ſubſtances exclude it.

A very remarkable phenomenon takes place in theſe changes of ſhade, by the degree of oxydation, when the infuſion of cochineal is made uſe of: the leaſt-oxyded precipitates are of a violet colour, which by expoſure to the air becomes more or leſs ſpeedily converted into beautiful carmines. Ammoniac cauſes them to approach ſomewhat towards crimſon; but they preſerve their primitive colour, and are even more beautiful when the ammoniac has evaporated. If this alkali were employed upon the carmine red produced by precipitation from the nitro-muriatic acid, the new ſhade would be brown, and would not recover its original luſtre.

In general this kind of colours depends on ſeveral complicated circumſtances: when applied on piece-goods, they reſiſt the action of acids better than that of the air and the ſun. They become ſtill more unchangeable by ſubſtituting, inſtead of the nitro-muriatic ſolution of tin, that made with muriatic acid, or a mixture of both. The coloured oxydes appear likewiſe to gain both in ſolidity and brilliancy when they are prepared by a ſolution of which the acid is not capable

pable of decompofition, and in which the oxygen is afforded to the metal by the decompofition of water. The muriatic acid, and, which is ftill better, the acetous acid, afford this advantage.

The phofphoric, oxalic, gallic, and tartareous, acids prefented no valuable properties with regard to the production of thefe colours. The precipitates formed by the union of thefe acids with the oxyde of tin do not difcolour the infufion of cochineal.

The quantity of water which may be ufed contributes likewife to the greater or lefs intimate combination of the oxyde of tin with colouring matters, and modifies the fhade which is afforded. For example, in the production of the carmine oxyde by the infufion of cochineal, too little water affords a dull colour, and too much prevents the metallic oxyde from acquiring colour enough ; fo that it may even be completely difcoloured by repeated wafhing.

After having given a variety of interefting details refpecting the coloured oxydes of tin, and carefully related the effential conditions for producing each of thefe oxydes in particular, the author purpofes to ufe, as a mordant in dying, the folution of the acetite of tin, which he prefers obtaining by the cold mixture of the aqueous folutions of the acetite of lead, and the muriate of tin in cryftals, in order that the acetous folutions of tin thus obtained may be very concentrated, and loaded with the metal. We cannot do better in this place than copy the words of the author, which will explain the nature of the operation, its importance, and the precautions it requires.

" In order to apply the acetic folution of tin upon cotton or linen, either by the block or the " pencil, it is neceffary that it fhould contain gum, and be left at reft for feveral days (upon " the piece), during which time it quits its acid folvent, and depofits its oxyde of tin, which " by affinity of adhefion will remain fixed on the piece by attracting the oxygen of the atmo- " fphere. Before the piece is expofed to the dying action of any fubftance whatever with " heat, it muft be boiled a few minutes in bran and water, or with cow-dung, after which it " muft be wafhed in running water. The colours to be produced will be different, according " to the dying drugs made ufe of. Madder, vegetable kermes, cochineal, and fermambouc, will " afford reds of various degrees of beauty ; the wood St. Martin affords a brown ; logwood " various fhades of violet ; yellow wood, quercitron, yellow berries (graines d'Avignon), " &c. &c. afford yellows. By mixing all thefe ingredients in different proportions, by weak- " ening the acetic folution of tin more or lefs with the gummed water, or by adding the acetic " folution of iron, an infinity of fhades may be produced. It is effential to gum this mordant " before it is applied on the linen or cotton. If this circumftance be forgotten, the colours " will be lefs bright; the fame precaution muft be attended to when the acetic folution of alu- " mine is added. But, on the contrary, filks and woollens impregnated with an ungummed " folution, then dried for feveral days, and dyed with the beforementioned ingredients, exhi- " bit very bright colours. Wool more particularly, dyed with cochineal, then paffed through " boiling water, and afterwards wafhed, prefents the moft beautiful purple.

" The intenfity of thefe colours will be much increafed by ufing the muriatic folution of tin,

" inftead

" instead of the acetic. But this solution, which becomes more and more oxygenated as it " dries, and successively disengages a portion of acid, weakens the cotton or the linen, which are " impregnated with it; but wool and silk suffer less. To avoid this inconvenience, it is ne- " cessary to defend the piece by a solution of one part of Marseilles soap, and sixteen of " water *. The article, after drying, is to be plunged in the muriatic solution of tin diluted " with water, and when well washed, it may be dyed. The colours obtained by means of " soap are more lively †."

The author here makes a very important remark with regard to the play of the affinities; namely, that in order to have the coloured oxydes of tin beautiful and solid, it is necessary that there should remain a small quantity of combined acid; and that nevertheless the same colours are obtained upon goods dyed by the intermedium of the solutions of tin, though these goods may have been previously passed through boiling water, which must necessarily carry off the salt or acid which might remain, and prove an impediment to the attraction of the colouring parts. The author inclines to attribute this effect in linens and cottons to part of the gum, with which they are impregnated, remaining combined with the oxyde, and serving instead of the acid, while in wool and silk the animal part acts in a similar manner.

Passing afterwards to the operations made with the alkaline solutions of the coloured oxydes, which he succeeded in obtaining, deprived of all causticity, he gives the following description. " I commonly use for these solutions, when the coloured oxyde is in the form of powder, a solution of potash made with one part of the carbonate of potash in crystals, one part of lime, and eight parts of water. After decantation, and reducing the liquid to one-half, I pound and stir well the powder with this liquor. The reduction of the fluid is to be made to one-fourth, when the oxyde is intended to be dissolved simply as it comes from the filter without drying. It is thus put into a vessel, and shaken with the fluid.

" To avoid an excess of potash, the solution must be poured a little at a time upon the oxyde, and some oxyde must be kept in reserve, in case too much alkali should have been added. This may be easily known by applying a small drop to the tongue.

" When these solutions do not possess sufficient consistence, they must be gummed. By this means they become applicable by the block or the pencil. They fix upon the cotton or linen the coloured oxydes, which, by some weeks repose, attract the carbonic acid of the atmosphere, is with which the alkali becomes saturated by degrees.

" The precipitation of these oxydes may likewise be made speedily, and immediately after the piece is dried, in which method the colours are also more vivid. For this purpose nothing more is requisite but to steep the piece for fifteen minutes in a muriatic solution of tin, diluted with twenty parts of water, or, which is better, in a solution of sulphate of alumine made with eight parts of

* I have given in the Annales de Chemie preparations of soap, which may be used instead of that of Marseilles. Note of the author.

† Journale de Physique Pluvoise. Annual Register, vol. v. p. 114, where the entire memoir is to be found.

water,

water, the exceſs of acid being abſorbed by adding one-eighth part of carbonate of lime, while the liquor is hot. Either of theſe proceſſes with the alkaline ſolution of the carmine red oxyde, afforded by muriate of tin and an infuſion of cochineal, produce nearly the ſame ſhade of carmine, inclining to crimſon. The ſhade, on the contrary, will be poppy colour, if, inſtead of the muriatic ſolution, the nitro-muriatic ſolution of tin be added, diluted with the ſame quantity of water; to which, in order to prevent ſpontaneous precipitation, one-fourth of the muriate of ſoda has been added. Theſe colours are as ſolid as the oxydes from which they ariſe."

Citizen Hauſſman then gives a detail reſpecting the ſhades of colour, of infinite variety, which may be obtained from the various mixtures of the alkaline ſolutions of coloured oxydes: very beautiful ſcarlet reds are afforded by a mixture of the ſolution of the carmine oxyde, with that of the oxyde coloured by yellow wood. The ſolutions of indigo made by means of arſenic, antimony, or muriate of tin, may be uſed, but not thoſe made by the ſulphurets of the ſame metals, becauſe the immerſion of the piece-goods, which muſt afterwards be made in the muriatic ſolution of tin, or in that of the ſulphate of alumine, would precipitate the ſulphurets, and render the colours obſcure.

The ſhades of green, of violet, and of prune monſieur, are particularly remarkable; the blue grounds can ſcarcely be obtained of an even tinge but in the vats. The ſolutions of indigo, gummed, and applied either alone or mixed with other coloured ſolutions, being uſually unequal; too much exceſs of alkali in the blue ſolutions would render the colours dull.

A fine blue may likewiſe be produced without the ſolution of tin, by that of gummed indigo, dried on the piece, and afterwards ſteeped in the ſolution of ſulphate of alumine: an ingredient affording a yellow colour will complete the deſired ſhade of green.

Some of the coloured oxydes of tin, preſent ſingular facts; that which is coloured by fernambouc becomes clear and pale as it dries on the piece, but the colour becomes of a deep red by immerſion in a ſolution of ſulphate of alumine: a ſimilar operation cauſes the colour from campeachy wood to become a deep violet, from a bright reddiſh grey.

The alkaline ſolution of the oxyde of tin, coloured by madder, requires more precautions for its ſucceſs than the foregoing, and perceptibly differs from them in its properties. The author deſcribes the moſt proper method of obtaining the infuſion of madder, to which, a proportionate quantity of carbonate of potaſh is added; the coloured oxyde is obtained by mixing it with the muriatic ſolution of tin: but in order to obtain an intenſe colour, it is abſolutely neceſſary to filter and dry the oxyde before it is diſſolved in the potaſh. By treating this ſolution in the ſame manner as was directed for that of the carmine oxyde, by means of the ſolution of the ſulphate of alumine, and not that of the muriate of tin, which is not applicable to the preſent experiment, a ſhade of ſuperfine pulverized madder is obtained; by boiling with bran and water, the ſhade, known in manufactories by the name of the ſecond red, will be obtained, which will become brighter by the addition of gum-water.

The alkaline ſolution of the oxyde, coloured by madder, when it is well made, is ſufficiently conſiſtent to be uſed without gum.

It

It is to be regretted, that goods coloured by these alkaline solutions, retain the excess of colour and of gum so strongly, that it is necessary to rub them, in order to clear it off; this inconvenience is considerable in deep shades, but less so in those which are bright.

Lastly, the author briefly exhibits the properties of zinc, by way of comparing its application with that of tin, as a colouring agent. If, like this last metal, zinc be susceptible of forming lakes with vegetable decoctions, or infusions, its properties differ much in other respects from those of tin; for, on the one hand, the acetite of zinc cannot be used as a mordant, because its acid does not become disengaged by drying, and the oxyde requires an alkaline carbonate to fix it on the piece; on the other hand, the alkaline solution of the oxyde of zinc not being capable of superoxydation, is not proper to co-operate with the solution of indigo: and the alkali not losing its causticity, cannot be advantageously used in the solution of the coloured oxydes of zinc.

Here concludes the task I have undertaken of giving an account of the labours of Citizen Hauffman. His paper abounds with so many facts, and excellent views, that it is with regret that I have abridged his recital. I have been careful to omit nothing of importance. Some conjectural ideas of the author, respecting the production of phosphorus, during the solution of tin by the muriatic acid, and the composition of metals, presumed, from their analogy with alumine, are passed over in silence, because the author himself does not seem to attach any considerable value to these notions, and has offered them with that modesty which ought always to be shewn with regard to assertions, not yet supported by decisive experiments.

C. A. PRIEUR.

VIII.

Observations on the Manners, Habits, and Natural History of the Elephant. By JOHN CORSE, *Esq**.

SINCE the remotest ages, the elephant, on account of his size, his sagacity, and his wonderful docility, has attracted the notice, and excited the admiration of philosophers and naturalists, both ancient and modern; and few travellers into Asia, or Africa, have omitted giving some account of him.

A residence, however, of more than ten years in Tiperah, a province of Bengal, situated at the eastern extremity of the British dominions in Asia, where herds of elephants are taken every season, afforded me frequent opportunities of observing not only the methods of taking them, but also the habits and manners of this noble animal.

From the year 1792 till 1797, the elephant hunters were entirely under my direction; so that I had it in my power to institute such experiments as I thought likely to discover any par-

* Philos. Trans. 1799. p. 31.

 ticulars

ticulars, not formerly known, in the natural hiſtory of the elephant. Soon after my arrival at Tiperah, while informing myſelf of the methods of taking wild elephants, I had occaſion to obſerve, that many errors, relative to the habits and manners of that uſeful animal, had been ſtated in the writings of European authors, and countenanced by ſome of the moſt approved writers.

The elephant has been declared to poſſeſs the ſentiment of modeſty in a high degree; and, by ſome, his ſagacity was ſuppoſed to excite feelings for the loſs of liberty, ſo acute as to cauſe him to refuſe to propagate his ſpecies while in ſlavery, leſt he ſhould entail on his progeny a fate ſimilar to his own; whilſt others have aſſerted, that he loſt the power of procreation in the domeſtic ſtate.

So circumſtanced, I was deſirous of taking advantage of my ſituation, and of making ſuch experiments and obſervations, as might tend to render more perfect the natural hiſtory of this uſeful animal.

Early in the year 1789, I gave an account of the methods then uſed for taking and training wild elephants, to the Aſiatic Society in Calcutta, which was publiſhed in vol. iii. of their Reſearches: and the following experiments and obſervations, made ſince that period, on the natural hiſtory of the elephant, will not, I hope, prove unworthy the attention of the Royal Society.

The young of the elephant, at its birth, is about 35 inches high; and, as a knowledge of its progreſſive growth forms the beſt criterion by which we can judge of the age of this animal, I ſhall here note down ſome obſervations made on this ſubject, till the elephant has attained its full ſize; for, after this period, till ſigns of old age appear, I do not know any marks by which a tolerable gueſs can be made of the number of its years, unleſs we could examine the teeth accurately; and, even then, there would be much uncertainty.

Very erroneous notions have been entertained, with reſpect to the ſize of elephants, in different parts of India; for which reaſon, I have collected ſuch facts as were likely to aſcertain their general height. The following obſervations, of the gradual increaſe of growth, were made upon a young elephant of Mr. Stephen Harris, which was accurately meaſured from time to time, and upon a female elephant of my own, till I left Tiperah.

Mr. Harris's elephant, at its birth, October 16, 1789, was 35 inches high.

		Feet.	Inches.	
In one year he grew	11 inches, and was	3	10	high.
In the 2d year	8 - -	4	6	
In the 3d year	6 - -	5	0	
In the 4th year	5 - -	5	5	
In the 5th year	5 - -	5	10	
In the 6th year	$3\frac{1}{2}$ - -	6	$1\frac{1}{2}$	
In the 7th year	$2\frac{1}{2}$ - -	6	4	

Except during his 4th and 5th years, the above meaſurement ſhows a gradual decreaſe in the

the proportion of growth for every year; and there was no opportunity of tracing the growth of this elephant further than its 7th year.

Another elephant, ſix feet nine inches high, at the time ſhe came into my poſſeſſion, was ſuppoſed to be fourteen years old; but, as the accuracy of the hunters cannot be depended on, it will be proper to take Mr. Harris's elephant, whoſe age is exactly known, as a ſtandard: and, judging from its annual increaſe, this will lead us to conſider the elephant, at the time I received her, to be only eleven years old; giving a period of four years, for the addition of five inches. I have made a greater allowance of time, on account of this elephant being a female, and Mr. Harris's a male, which there is much reaſon to believe grows faſter.

During the next five years, before ſhe was covered, ſhe grew only ſix inches; but, what is extremely curious, while pregnant, ſhe grew, in twenty-one months, five inches: and in the following ſeventeen months, though again pregnant, ſhe grew only half an inch; at which time, ſhe was ſent from Comillah, as I was then preparing to leave India.

At this time, ſhe was about nineteen years old, and had, perhaps, attained her full growth. Her young one was then (Nov. 1796) not twenty months old; yet he was four feet five inches and a half high, having grown eighteen inches ſince his birth, which is the greateſt progreſſive growth, in the elephant, that I have known.

Theſe obſervations, when applied to the general growth of elephants, are to be taken with ſome allowance; ſince, during the ſtate of the firſt pregnancy, there is ſo great an irregularity in the growth of female elephants, as alone occaſions conſiderable difficulty, even ſuppoſing the progreſſive growth nearly equal in the ſpecies. It is probable, however, that this is not by any means equal: for, as the elephants vary greatly in ſize, and as males are generally much taller than females, we muſt conclude they either grow faſter, or are longer in attaining their full growth *. But it may be ſafely aſſerted, that elephants, like moſt quadrupeds, propagate their ſpecies before they have acquired their full growth. Many females have been known, when taken while pregnant, to have grown ſeveral inches higher before delivery; and, as it has been ſtated, that the female elephant on which my obſervations were made, could not exceed ſixteen years when ſhe received the male, it is probable the wild female elephants are in heat before that period.

If, from the above data, it may be allowed to form a probable conjecture, elephants attain their full ſize between eighteen and twenty-four years of age. The height of the elephant, I believe, has been generally much exaggerated. In India, the height of females is, in general, from ſeven to eight feet; and that of males, from eight to ten feet, meaſured at the ſhoulder.

I have never heard but of one elephant, on good authority, that much exceeded ten feet: this was a male, belonging to Aſoph ul Dowlah, the late vizier of Oude. His dimenſions, as obligingly communicated to me by Mr. Cherry, then reſident at Lucknow, were as follow.

* A male elephant, belonging to the Cudwah Rajah, till he was above twenty years of age, continued to increaſe in height, and was ſuppoſed not to have attained his full ſize, when I left Tiperah: he was then above twenty-two years old.

He was meaſured on the 18th of June, 1796.

	Feet.	Inches.
From foot to foot over the ſhoulder - - - -	22	10½
From the top of the ſhoulder, perpendicular height - -	10	6
From the top of the head, when ſet up, as he ought to march in ſtate	12	2
From the front of the face to the inſertion of the tail - -	15	11

Captain Sandys, of the Bengal eſtabliſhment, obligingly ſhewed me a liſt of about 150 elephants, of which he had the management during the late war with Tippoo Sultaun, in Myſore, and not one of them was ten feet, and only a few males nine feet and a half high. I was very particular in aſcertaining the height of the elephants employed at Madras, and with the army under Marquis Cornwallis, where there were both Ceylon and Bengal elephants; and I have been aſſured, that thoſe of Ceylon were neither higher, nor ſuperior, in any reſpect, to thoſe of Bengal; and ſome officers aſſert, that they were conſiderably inferior, in point of utility.

The Madras elephants have been ſaid to be from ſeventeen to twenty feet high; but, to ſhew how much the natives of India are inclined to the marvellous, and how liable Europeans themſelves are to miſtakes, I will relate a circumſtance that happened to myſelf.

Having heard, from ſeveral gentlemen who had been at Dacca, that the Nabob there had an elephant about fourteen feet high, I was deſirous to meaſure him; eſpecially as I had ſeen him often myſelf, during the year 1785, and then ſuppoſed him to be above twelve feet. After being at Tiperah, and having ſeen many elephants caught, in the years 1786, 1787, and 1788, and finding all of them much inferior in height to what I ſuppoſed the Nabob's elephant, I went to Dacca, in 1789, determined to ſee this huge animal meaſured. At firſt, I ſent for the driver*, to aſk ſome queſtions concerning this elephant; he, without heſitation, aſſured me he was from ten to twelve cubits, that is, from fifteen to eighteen feet high; but added, he could not, without the Nabob's permiſſion, bring me the elephant to be examined. Permiſſion was accordingly aſked, and granted: I had him meaſured exactly, and was rather ſurpriſed to find he did not exceed ten feet in height.

The honourable company's ſtandard, for ſerviceable elephants, is ſeven feet and upwards, meaſured at the ſhoulder, in the ſame manner as horſes are. At the middle of the back, they are conſiderably higher; the curve or arch of which, particularly in young elephants, makes a difference of ſeveral inches.

After an elephant has attained his full growth, it is a ſure ſign of old age when this curve becomes leſs; and ſtill more ſo, when the back is flat, or a little depreſſed. A partial depreſſion of the ſpine is, however, no uncommon occurrence, even in very young elephants; and I am convinced it happens from external injury. I have been ſurpriſed to ſee the violence uſed (in herds of wild elephants juſt taken) by the large elephants, both male and female, putting the projecting part of the upper jaw, from which the tuſks grow out, on the ſpine of the young ones, and preſſing them to the ground, while they roared from pain.

It has been ſtated, that the ſagacity of the elephant is ſo great, and his memory ſo retentive,

* Or *mahote*, as he is generally called.

that

that when once he has received an injury, or been in bondage, and afterwards eſcapes, it is not poſſible, by any art, again to entrap him. Great as my partiality is for this noble animal, whoſe modes of life and general ſagacity I have had ſo many opportunities of obſerving, yet a regard to truth compels me to mention ſome facts, which contradict that opinion. The following hiſtory of an elephant taken by Mr. Leeke*, of Longford Hall, Shropſhire, contains many intereſting particulars on this ſubject. The elephant was a female, and was taken at firſt, with a herd of many others, in the year 1765, by Rajah Kiſhun Maunick †, who, about ſix months after, gave her to Abdoor Rezah, a man of ſome rank and conſequence in the diſtrict. In 1767, the Rajah ſent a force againſt this Abdoor Rezah, for ſome refractory conduct, who, in his retreat to the hills, turned her looſe into the woods, after having uſed her above two years, as a riding elephant. In January, 1770, ſhe was retaken by the Rajah; but, in April, 1771, ſhe broke looſe from her pickets, in a ſtormy night, and eſcaped to the hills. On the 25th of December, 1782, ſhe was driven by Mr. Leeke's elephant hunters into a *keddah* ‡; and, the day following, when Mr. Leeke went to ſee the herd that had been ſecured, this elephant was pointed out to him by the hunters, and particularly by a driver who had charge of her for ſome time, and well recollected her. They frequently called to her, by name; to which ſhe ſeemed to pay ſome attention, by immediately looking towards them, when her name, *Juggut-Peäurce*, was repeated; nor did ſhe appear like the wild elephants, which were conſtantly running about the *keddah* in a rage, but ſeemed perfectly reconciled to her ſituation.

* He was then the reſident of Tiperah, and took ſome pains to aſcertain the facts here mentioned.

† The Rajah is the principal Zemindar in the province of Tiperah, paying the uſual revenue for his lands in the low country; but, in the hills he is an independent ſovereign, has the power of life and death over his ſubjects, a mint, and other inſignia of ſovereignty.

‡ The incloſure in which elephants are ſecured. *Vide* Aſiatic Reſearches, Vol. III. Art. "Method of catching Elephants."

(To be concluded hereafter.)

PHILOSOPHICAL NEWS AND ACCOUNTS OF BOOKS.

Experiments on the Vibrations of Plates of Glaſs. By Profeſſor Chladni, of Berlin.

IN the year 1787, M. Chladni publiſhed, at Leipzic, a work in German, entitled, *Diſcoveries reſpecting the Theory of Sound*; in which he announces, that if glaſs, ſprinkled with fine powder, be made to ſound, this powder will be ſo diſtributed by the effect of the vibrations, as to form very remarkable figures, which are conſtantly produced under ſimilar circumſtances.

These experiments have been lately repeated at Paris. In order to make them with success, it is necessary to take a square piece of glass, three or four inches wide, not too thick, and without either bubbles or knots. This plate is to be held firm between two very pointed pieces of cork, and then powdered, with very fine sawdust or sand; and when the bow of a violin, well rozined, is drawn against the edge of the glass, blunted or rounded by grinding, a sound will be produced, and, at the same time, the powder will be seen to dispose itself in lines which afford different figures, according to the manner or place at which the glass is held, the bow is drawn, and the sound produced.

If, for example, the square be pinched by its centre, and the bow drawn along the middle of one of its sides, the powder will dispose itself in two lines, nearly diagonal to the square. If the bow be drawn at the distance of one-fourth of the side from the angle, the two lines will become the sides of an octagon, and the sound will be the octave of the preceding tone.

By varying the position of the point at which the glass is held, the figures also become changed.

If the plate of glass be circular, and the bow be a little inclined, the six radii of an hexagon will be formed.

In this manner, Mr. Chladni obtained 166 different figures, which he calls resonant figures. Without precisely explaining the cause which produces these figures, their analogy with the stationary and vibrating parts of a musical string evidently shews that the vibrating surface divides itself into a number of portions which move separately, but, no doubt, in an isochronous or commensurate manner, when the tones are clear and musical. The lines in which the powder is collected, are a kind of gutters formed by the points which remain at rest, while the other parts become alternately convex and concave.

These experiments, which succeed equally with plates of metal, and even of wood, being carefully made and classed systematically, promise to throw much light on the manner in which surfaces vibrate, and may, perhaps, tend to the improvement of the theory of wind instruments, and other musical apparatus, which is still very imperfect, notwithstanding the labours of Euler, in attempting to reduce them to computation.

Decade Philos. No. 17. Art. VII.

Philosophical Transactions of the Royal Society of London, for the Year 1799. Part the First. Quarto, 156 Pages, with three Plates; London, sold by Elmsly.

This part contains, 1. The Croonian lecture. Experiments and observations upon the structure of nerves. By Everard Home, esq; F.R.S. 2. The Bakerian lecture. Observations upon an universal horizontal refraction of the air; with remarks on the variations to which the lower parts of the atmosphere are sometimes subject. By the rev. S. Vince, A.M. F.R.S. and plumian professor of astronomy and experimental philosophy in the university of Cambridge. 3. Abstract of a register of the barometer, thermometer, and rain, at Lyndon, in Rutland, 1797. With

With some remarks on the recovery of injured trees. By Thomas Barker, Esq. 4. Some additions to a paper read in 1790, on the subject of a child with a double head. By Everard Home, Esq; F.R.S. 5. Observations on the manners, habits, and natural history of the elephant. By John Corse, Esq. 6. On the decomposition of the acid of borax, or sedative salt. By Lawrence de Crell, M.D. F.R.S. Lond. and Edinb. and M.R.I.A. translated from the German. 7. A method of finding the latitude of a place, by means of two altitudes of the sun, and the time elapsed betwixt the observations. By the rev. W. Lax, A.M. Lowndes's professor of Astronomy in the university of Cambridge. 8. A fourth catalogue of the comparative brightness of the stars. By William Herschel, LL.D. F.R.S. 9. On a submarine forest on the east coast of England. By Joseph Correa de Seraa, LL.D. F.R.S. and A.S. Appendix. Meteorological journal, kept at the apartments of the Royal Society, by order of the president and council.

Traité des Montres à Longitudes, &c. or a treatise on time-pieces, containing the description, construction, and all the details relative to the workmanship of these machines; their dimensions, the method of trying them, &c.; together with, 1. An instructive memoir on the fabrication of clocks and time-pieces; 2. Description of two astronomical clocks; 3. Trial of a simple method of preserving the relation of weights and measures; and of establishing an universal and perpetual measure. One volume in quarto, with seven engraved plates; by Ferd. Berthoud, of the national institute of France, and mechanic to the marine. Sold at Paris by the author aux Galleries du Louvre, 1792.

This work *, though it has been printed seven years, has not been published till lately; and we hasten to announce it to the public. It is an important supplement to the Essai sur l'Horlogerie, 2 vols, in quarto, published in 1763 and 1786, and to the Traité des Horloges marines, 1773, of the same author.

The first article consists of the description of a marine time-piece, portable and vertical, denoted by the number 46, which has been used at sea. It is slung in jimbals; and when used on shore it is carried in the pocket. On this occasion we find a description of a new free escapement.

The second chapter presents a description of a portable vertical watch, without a suspension, distinguished by the number 47. It has the common form of a pocket watch.

In the third and fourth chapters descriptions are given of two small horizontal watches intended to serve as regulators, remaining at rest in the vessel, carried by their suspensions: that which is denoted by number 45 makes four vibrations per second. In the other time-piece, no. 48, the balance makes two vibrations per second.

Chapter V. contains the most essential details for the execution of watches and small time-pieces for the longitude.

* This account is given by Lalande in the Magasin Encyclopédique, VII. 113.

Chapter

Chapter VI. treats of the compenſation for temperature produced by the balance itſelf.

In the ſeventh chapter, cit. Berthoud gives the conſtruction and means of executing a portable watch, the compenſation of which is produced by the balance and the pendulum ſpring. And, laſtly, he explains the conſtruction of a vertical watch, without a fuſee, and carried by ſuſpenſion. A method is given to remedy the want of iſochroniſm in the pendulum ſpring by means of the balance itſelf.

The ſecond volume of this work is entitled, Suite du Traité des Montres à Longitudes, &c. or Supplement to the Treatiſe on Time-pieces, containing the conſtruction of portable vertical watches (montres), and of horizontal time-pieces (horloges), for the longeſt voyages: the deſcription and trials of ſmall horizontal * watches, very ſimple and portable; with two engraved plates. Paris, in the 5th republican year.

This volume, which has been printed a year, has not yet been announced or publiſhed. It is divided into two parts. Citizen Berthoud, in the firſt place, treats of the queſtion of the beſt poſition for portable watches, and ſmall time-pieces. In the next place, he treats of the moſt convenient number of vibrations of the balance, to diminiſh the frictions, and render the watch moſt convenient for the obſerver. He deſcribes the conſtruction of the balance, which bears its own compenſation; the moſt ſimple and ſure conſtruction of the free eſcapement, and the ſpiral ſprings of watches to render the vibrations of the balance iſchronal; the deſcription of the elaſtic balance, ſerving for the meaſure and proof of the ſpiral ſprings; and, laſtly, he explains the method of trying time-pieces.

In the ſecond part, we find the conſtruction of the vertical watch, no. 56.—the portable vertical watches, no. 60 and 62;—of the ſmall horizontal time-piece, no. 63. conſtructed to give the longitude in the longeſt voyages;—of the ſmall horizontal time-piece, no. 66. without (rouleau). And in the concluſion the author treats of vertical watches, and ſmall horizontal time-pieces. The author gives the preference to the latter.

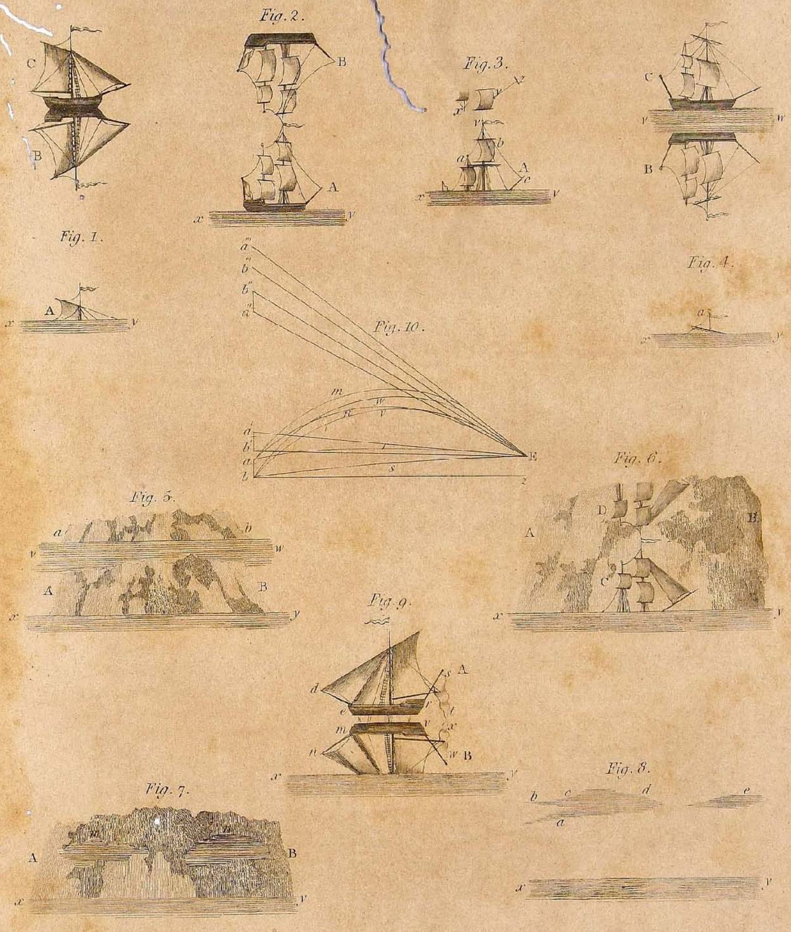
This work is terminated by a ſupplement, containing the reſult of the trials made with the horizontal watch, no. 65. in which the pivots of the balance turn ſimply in holes made in copper (cuivre); and, laſtly, we find the conſtruction of a very ſimple portable watch, and of a ſmall horizontal time-piece improved from no. 65.

In the eſſay on weights and meaſures, the author gives the dimenſions of a cylindrical pendulum, vibrating on knife edges, repreſenting the French foot, and the reſults of trials made in 1791, to determine the oſcillations, which proved to be 7710 per hour. Though the pendulum has been abandoned, in order to ſubſtitute the ten-millionth part of the quadrant of the meridian, yet the labours of citizen Berthoud to preſerve the dimenſions of any determinate pendulum, cannot be viewed without intereſt.

Upon the whole, this new work is calculated to add to the reputation of its illuſtrious author, and exhibits, without myſtery, thoſe practices which have procured him repeated ſucceſs in his marine time-pieces, of which the production is of ſo much importance to navigation.

* I ſuppoſe the words horizontal and vertical in the whole of this account, to relate merely to the poſition in which the time-piece is to be habitually kept, and not to the nature of their eſcapements. The word horizontal has not, I believe, been applied to any eſcapement by the French. N.

The Effects of unusual horizontal Refraction.



Barlow sculp.

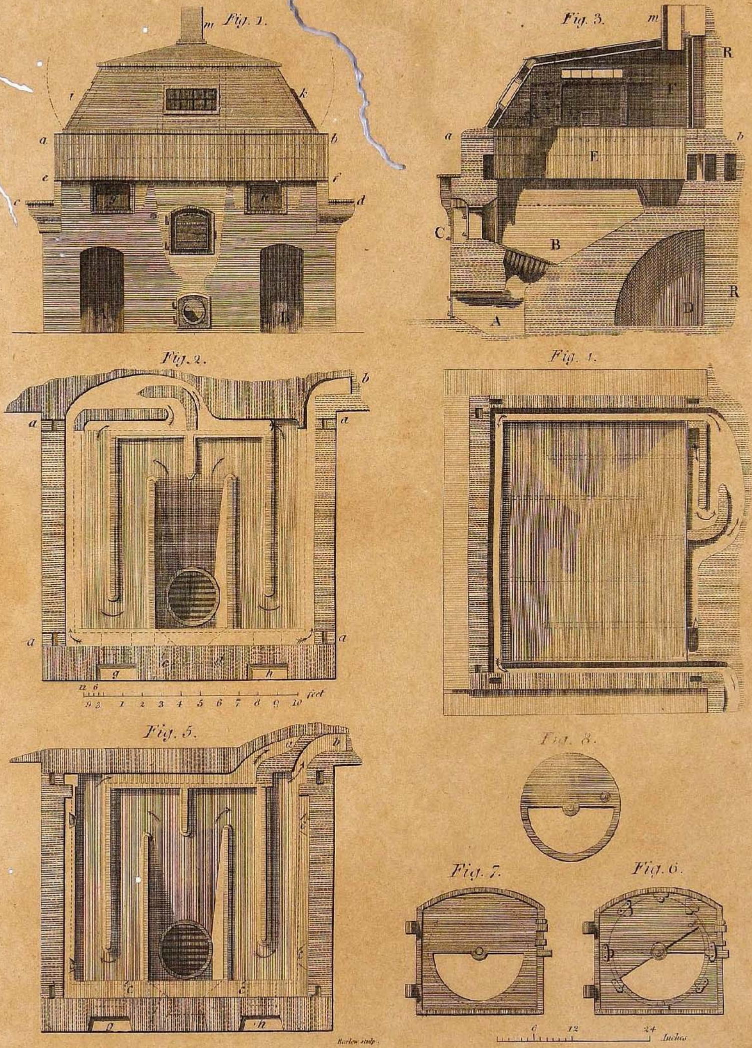


Fig. 1.
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Fig. 3.
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Fig. 8.
Fig. 7.
Fig. 6.
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24
Inches
Barlow sculp.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST 1799.

ARTICLE I.

Supplement to the Paper on the Philoſophical Uſes of a Common Watch. By the Rev. W. PEARSON, of Lincoln.

TO MR. NICHOLSON.

SIR,

AS the paper "On the Philoſophical Uſes of a common Pocket Watch," which I ſent you, was deemed ſufficiently important to be inſerted in your Journal, I am induced to tranſmit to you ſome additional numbers, ſuitable for a watch that may indicate hours, minutes, ſeconds, and quarters of a ſecond, without any extra-wheel work; in order that, out of the different varieties, the watch-maker may ſelect ſuch numbers as are already marked upon the plate of his engine; for I underſtand that 64, one of the numbers propoſed for a new watch, though as eaſily divided as any other number, is not uſually put upon the common plates of watch-makers.—That the different practicable varieties may be expreſſed in as conciſe a manner as is conſiſtent with perſpicuity, I will divide the whole train of a watch into three portions: that part which comprehends the fuſee, the great wheel, and its pinion, may be denominated the firſt portion; the ſecond portion may be the centre wheel, with the pinion which it actuates, and the third wheel with the pinion actuated by it, which is placed on

on the axle of the contrate wheel; and the third portion will then be the contrate wheel, with the pinion actuated by it, the crown wheel, and the palats: the first portion determines the time that a watch shall go at one winding up, as has been observed before; the second portion determines the mutual velocities of the minute and second hands; and the third portion regulates the number of beats per second: hence any one of these portions may be constructed variously, without affecting the calculations of the two others.

If a watch be required to have 6 spirals on the fusee, and to go just 30 hours, we shall have for the first portion $6 \times \frac{45}{9}$, $6 \times \frac{50}{10}$, $6 \times \frac{55}{11}$, and $6 \times \frac{60}{12}$: for the second portion we may take $\frac{48}{6} \times \frac{45}{6}$, or $\frac{45}{6} \times \frac{48}{6}$, $\frac{60}{6} \times \frac{42}{7}$, or $\frac{42}{7} \times \frac{60}{6}$, $\frac{60}{6} \times \frac{48}{8}$, or $\frac{48}{8} \times \frac{60}{6}$, $\frac{60}{6} \times \frac{54}{9}$, or $\frac{54}{9} \times \frac{60}{6}$, $\frac{60}{6} \times \frac{60}{10}$, or $\frac{60}{10} \times \frac{60}{6}$, $\frac{60}{7} \times \frac{49}{7}$, or $\frac{49}{7} \times \frac{60}{7}$, $\frac{60}{7} \times \frac{56}{8}$, or $\frac{56}{8} \times \frac{60}{7}$, $\frac{63}{7} \times \frac{60}{9}$, or $\frac{60}{9} \times \frac{63}{7}$; and, lastly, as already given, $\frac{60}{8} \times \frac{64}{8}$, or $\frac{64}{8} \times \frac{60}{8}$: likewise, for the third portion, we may put $\frac{64 \times 15 \times 2}{8}$, $\frac{56 \times 15 \times 2}{7}$, and $\frac{48 \times 15 \times 2}{6}$, any one of which formulæ, divided by 60, the seconds in a minute, will give 4 for the number of beats per second.

Here then we have got four varieties of the first portion, nine of the second, without inverting the order, and three of the third, which, by the rule for finding* compositions, will produce $4 \times 9 \times 3 = 108$ different forms of contraction, to answer the same purpose, without making any alteration in the number of spirals on the fusee, or in the dial-work; and where no number out of all the wheels exceeds 64, or is less than 42.

The allowance to be made in correcting the seconds gained by the beats of any watch must be $\frac{1}{1440}$ of the whole, for every minute in the daily error in the rate of going, *plus* or *minus*, accordingly as the rate is too slow or two fast: therefore, in the second note (N), page 50, where, for an error of five minutes, it is said that the allowance will be "less than $\frac{1}{300}$" of the whole, it ought to have been "upwards of $\frac{1}{300}$:" for $\frac{5}{1440} = \frac{1}{288}$ is a greater portion of unity than $\frac{1}{300}$, though inadvertently it might be taken as constituting a smaller.

It may probably appear too formal to notice here two typographical errors in the description of the numbers proposed for a new watch in my last paper, where "2 pivots" ought to have been "2 palats," (or "pallets," or "palettes," for different authors spell this word differently); and 6 spirals on the "barrel," 6 spirals on the "fusee;" but it is absolutely necessary to advert to another passage therein, which, partly from my own inaccuracy, which you have already noticed, and partly from the printer's omission of the signs + and —, as it now stands, is completely unintelligible. At the asterisk in page 53, let the sentence be understood to be thus: as 23 h. 56 m. 4,098 s. (the length of a sidereal rotation of the earth),

* Vide Hutton's Course of Mathematics, vol. i. p. 134.

plus

plus or *minus* the daily error in the rate of going : are to 360° : : ſo is the number of obſerved ſeconds of time : to the quantity of the horizontal angle required.

Theſe are all the additional obſervations which ſeem neceſſary to make, reſpecting the numbers of a new watch calculated for philoſophical uſes, and the method of applying it; but there is another inſtrument frequently to be met with, which is capable of various conſtructions, that will meaſure ſmaller portions of time, than it is uſually made to meaſure; and which is ſufficiently portable for being carried ſmall diſtances: I mean the ſpring clock, the vibrations of which are regulated by a ſhort pendulum. Out of the many inſtruments of this kind which I have noticed, I do not remember any one which meaſures, or, at leaſt, which indicates ſeconds: I ſhall, therefore, ſubjoin ſuch numbers as are proper for a ſpring clock, that ſhall indicate ſeconds, and alſo make a given number of vibrations in a ſecond.— If 39,2 inches be taken as the true length of a pendulum, to ſwing ſeconds in our climate, which varies not one-tenth of an inch from the reſult of Mr. Whitehurſt's and George Graham's experiments on the lengths of pendulums, the error in the length of ſmall pendulums, calculated therefrom, will fall within the threads of the adjuſting ſcrew at the inferior end of the rod; on which account the lengths, ſo deduced, may be put down as the true lengths without impropriety: thus,

4 vibrations in a ſecond will require a pendulum	-	2,45 inches long,
3 do. - - - -	-	4,35
$2\frac{1}{2}$ do. - - - -	-	6,27
$2\frac{1}{4}$ do. - - - -	-	7,74
2 do. - - - -	-	9,8

The two firſt of theſe pendulums appear to be too ſhort to perform with ſteadineſs, and conſequently will be conſidered as unworthy of further notice. The firſt portion of a train, ſuitable for any of the three laſt pendulums, for a ſpring clock to go upwards of a week, may be $\frac{84}{8} \times 17$ turns on the fuſee, or $\frac{84}{7} \times 15$ turns, or $\frac{96}{8} \times 12$ turns, or indeed any other ſimilar numbers producing a like reſult: for the ſecond portion, any one of the formulæ given for a watch in the former part of this article, will be proper; but for the laſt portion, each different pendulum will require different numbers: a pendulum to make $2\frac{1}{2}$ vibrations on a ſecond, may have the contrate wheel 24, its pinion 8, and the crown wheel 25; or, otherwiſe, the contrate wheel 40, with a pinion of 8, and the crown wheel 15: where the vibrations are $2\frac{1}{4}$ in a ſecond, the contrate wheel may be 36, with a pinion of 8, and the crown wheel, as before, 15: and for exactly two vibrations in a ſecond, the contrate wheel will be required to be 32, with a pinion of 8, and the crown wheel 15, as in the two laſt inſtances. In all theſe calculations, a hand, placed on the axle of the contrate wheel, will indicate ſeconds without apparent recoil; and the trains, compoſed of any of the various portions laid down, will be equally accurate, and admit of many varieties.

If a ſpring clock is to be uſed for meaſuring ſmall portions of time, by the vibrations of its pendulum, which falls not under the deſcription of any of thoſe conſtructions, the vibrations

and fractional parts of a vibration are calculable by the general rule for watches already given.

I am, ſir, with much eſteem, your's, &c.

Lincoln, May 9, 1799. W. PEARSON.

P. S. Since the preceding part of this article was written, I have met with a train propoſed for a new watch to indicate ſeconds, and quarters of a ſecond, by the beat, under the word "clock," in vol. v. part i. p. 76, of the Encyclopedia Britannica, which, by the mode of expreſſion adopted before, will ſtand thus:

	48 great wheel					
	12	60 ſecond or centre do.				
		10	60 third do.			
7½ turns on the fuſee.			6	48 contrate do.		
To go 30 hours.				6	15 crown do.	
					2 palettes.	

The reader will perceive that theſe numbers conſtitute one of the 108 varieties given above, except that the great wheel is given ſmaller here, by reaſon of the fuſee having more ſpirals. Under the ſame article, I alſo find that, as I ſuppoſed, ſpring clocks are not conſtructed to ſhew ſeconds, though it has been ſhewn that they are capable of many different conſtructions, which are equally calculated to anſwer this purpoſe, as well as to make, at the ſame time, a given number of vibrations in a ſecond.

Likewiſe, I have juſt had an opportunity of examining a ſmall larum, or alarum clock, going by a ſuſpended weight, and regulated by a ſhort pendulum only 4¼ inches long, which, conſequently, makes upwards of three vibrations in a ſecond, and which, I underſtand, meaſures time pretty accurately. I mean as accurately as theſe clocks are intended to do; for great accuracy cannot be expected from a clock with a very ſhort pendulum, particularly if it be a ſpring clock that goes a week; on account of the difficulty of forming the fuſee to correſpond exactly to the action of a powerful ſpring for many ſucceſſive days, as well as on account of the imperfections of the pendulum: but whatever be the error in any day, the error in the time of a beat of that day will be a proportional part thereof; and provided the rate of going be tolerably uniform throughout each ſucceſſive day, after winding up, the beats will alſo be nearly uniform: I ſhall, therefore, give the numbers proper for the third portion of a train ſuitable for a pendulum, to vibrate three times in a ſecond, which I had purpoſely omitted. If any of the 1ſt or 2d portions, already ſpecified, be adopted, the contrate wheel will be 48, with a pinion of 8, and the crown wheel 15. I have preferred a pinion of 8, from a perſuaſion that this number will render the works of a watch or clock more perfect, than either 7 or 6 would do; and is the number we find in the beſt finiſhed inſtruments. If any perſon ſhould wiſh to try the action of a pendulum to ſwing the quarters of a ſecond exactly, any of the trains propoſed for a new watch will be found applicable to this purpoſe. The reaſon why an *odd* number is always fixed upon as proper for a crown wheel, will need no explanation.

May 11*th.*

II. *Obſer-*

II.

Obſervations on the Manners, Habits, and Natural Hiſtory of the Elephant. By JOHN CORSE, *Eſq.* *(Continued from p.* 185.)

FROM the 25th of December to the 13th of January (a ſpace of eighteen days), ſhe never went near enough the outlet (or *roomee*) to be ſecured; from a recollection, perhaps, of what ſhe had twice before ſuffered *. Orders, however, had been given, not to permit her to enter the outlet, had ſhe been ſo inclined, as Mr. Leeke wiſhed to be preſent when ſhe was taken out of the *keddah*. On the 13th of January, 1783, Mr. Leeke went out, when there were only herſelf, another female, and eight young ones, remaining in the incloſure. After the other female had been ſecured, by means of the *koomkees* † ſent in for that purpoſe, the hunters were ordered to call *Juggut-Peāūree*. She immediately came to the ſide of the ditch, within the incloſure: on which, ſome of the drivers were deſired to carry in a plantain tree, the leaves of which ſhe not only took from their hands, with her trunk, but opened her mouth, for them to put a leaf into it, which they did, ſtroking and careſſing her, and calling to her by name. Mr. Leeke, ſeeing the animal ſo tame, would not permit the hunters to attempt tying her; but ordered one of the trained elephants to be brought to her, and the driver to take her by the ear, and order her to lie down. At firſt, ſhe did not like the *koomkee* to go near her, and retired to a diſtance, ſeemingly angry; but, when the drivers, who were on foot, called to her, ſhe came immediately, and allowed them to ſtroke and careſs her, as before; and, in a few minutes after, permitted the trained females to be familiar. A driver, from one of theſe, then faſtened a rope round her body, and inſtantly jumped on her back; which, at the moment, ſhe did not like, but was ſoon reconciled to it. A ſmall cord was next faſtened round her neck, for the driver to put his feet in, who, ſeating himſelf on the neck, in the uſual manner, drove her about the *keddah*, the ſame as any of the tame elephants.

After this, he ordered her to lie down, which ſhe inſtantly did; nor did ſhe riſe till ſhe was deſired. He fed her from his ſeat, gave her his ſtick to hold, which ſhe took with her trunk, and put into her mouth, kept, and then returned it, as ſhe was directed, and as ſhe formerly had been accuſtomed to do. In ſhort, ſhe was ſo obedient, that had there been more wild elephants in the *keddah* to tie, ſhe would have been uſeful in ſecuring them.

Mr. Leeke himſelf then went up, took her by the ear, and bade her lie down; a command ſhe inſtantly obeyed.

I have known ſeveral other inſtances of elephants being taken a ſecond time; and was myſelf a witneſs both of the eſcape and retaking of one, as related in the following account.

* When elephants were ſecured in the outlet from the *keddah*, they bruiſed themſelves terribly. *Vide* Aſiatic Reſearches, Vol. III.

† *Koomkees* are female elephants, trained for the purpoſe of ſecuring wild elephants, and more particularly thoſe large males which ſtray from the woods, named *goondahs*. *Vide* Aſiatic Reſearches, Vol. III.

In

In June, 1787, *Jâttra-Mungul*, a male elephant, taken the year before, was travelling, in company with ſome other elephants, towards Chittigong, laden with a tent and ſome baggage, for our * accommodation on the journey. Having come upon a tiger's track, which elephants diſcover readily by the ſmell, he took fright, and ran off to the woods, in ſpite of the efforts of his driver. On entering the wood, the driver ſaved himſelf, by ſpringing from the elephant, and clinging to the branch of a tree under which he was paſſing; when the elephant had got rid of his driver, he ſoon contrived to ſhake off his load. As ſoon as he ran away, a trained female was diſpatched after him, but could not get up in time to prevent his eſcape; ſhe, however, brought back his driver, and the load he had thrown off, and we proceeded, without any hope of ever ſeeing him again.

Eighteen months after this, when a herd of elephants had been taken, and had remained ſeveral days in the incloſure, till they were enticed into the outlet, there tied, and let out in the uſual manner, one of the drivers, viewing a male elephant very attentively, declared he reſembled the one which had run away. This excited the curioſity of every one, to go and look at him; but, when any perſon came near, the animal ſtruck at him with his trunk, and, in every reſpect, appeared as wild and outrageous as any of the other elephants.

At length, an old hunter, coming up and examining him narrowly, declared he was the very elephant that had made his eſcape about eighteen months before.

Confident of this, he boldly rode up to him, on a tame elephant, and ordered him to lie down, pulling him by the ear at the ſame time. The animal ſeemed quite taken by ſurpriſe, and inſtantly obeyed the word of command, with as much quickneſs as the ropes, with which he was tied, permitted; uttering, at the ſame time, a peculiar ſhrill ſqueak through his trunk, as he had formerly been known to do; by which he was immediately recognized, by every perſon who had ever been acquainted with this peculiarity.

Thus we ſee that this elephant, for the ſpace of eight or ten days, during which he was in the *keddah*, and even while he was tying in the outlet, appeared equally wild and fierce as the boldeſt elephant then taken; ſo that he was not even ſuſpected of having been formerly taken, till he was conducted from the outlet. The moment, however, he was addreſſed in a commanding tone, the recollection of his former obedience ſeemed to ruſh upon him at once; and, without any difficulty, he permitted a driver to be ſeated on his neck, who, in a few days, made him as tractable as ever.

Theſe, and ſeveral other inſtances which have occurred, clearly evince, that elephants have not the ſagacity to avoid a ſnare into which they have, even more than once, fallen.

The general idea, that tame elephants would not breed, has doubtleſs prevented trials being made, to aſcertain whether, under particular circumſtances, this ſuppoſed reluctance could be got the better of.

I was however convinced, from obſervation, as well as from ſome particular facts, that elephants had their ſeaſons in which they were in heat; I ſhall, therefore, firſt mention the

* Mr. Buller and myſelf.

circum-

circumſtances which induced me to attempt breeding from tame elephants, and then relate the ſucceſs of the experiments inſtituted for this purpoſe.

The circumſtances to which I allude, happened in January, 1790, at a *keddah* near to Comillah, the capital of Tiperah.

Meſſrs. Henry Buller and George Dowdeſwell, of Chittigong, being then on a viſit at Comillah, accompanied me and ſeveral others, to ſee a herd of elephants which had been lately taken. Our viſitors then propoſed a trial being made, of tying the wild elephants immediately, in the *keddah*, in the manner practiſed at Chittigong, inſtead of waiting till they were enticed, one after another, into the narrow outlet, there to be ſecured, and led out in the uſual manner*.

This mode they recommended ſo earneſtly, from a conviction of its ſuperior utility †, that Mr. John Buller, to whom the *keddah* belonged, aſſented to the trial being made, and gave orders for the trained females, and proper aſſiſtants, to go directly within the incloſure. Having but few trained females preſent, it was judged adviſable to ſend in a fine male elephant, taken many years before, and thoroughly broke in, to aſſiſt them, as well as to keep the herd in awe. He had no ſooner entered the incloſure, and been brought near the herd, than, diſcovering one of the females to be in heat, impelled by deſire, and eager to cover her, he daſhed through the herd, regardleſs of the orders and ſevere diſcipline of the driver, and had nearly accompliſhed his purpoſe. The driver, being alarmed for his own ſafety, exerted in vain all his ſtrength, to turn him, and bring him from among the wild elephants; but the drivers of the trained females, coming ſpeedily to his aſſiſtance, ſoon ſurrounded this furious animal, and ſeparated him from the herd. In reſentment, however, of his diſappointment, he attacked a ſmall *koomkee*, with ſuch violence as completely overturned her and her rider; and, had he not been of a particular ſpecies, called *mucknah*, which have only ſmall tuſks, he moſt probably would have transfixed, and killed her on the ſpot: fortunately, neither ſhe nor her driver received any conſiderable hurt. This accident prevented the trial being then made, to tie the wild elephants in the manner propoſed.

Reflecting on the diſobedience ſhown by an elephant remarkably docile, and which had

* *Vide* Aſiatic Reſearches, Vol. III. article, "Method of catching wild Elephants;" where this proceſs is particularly deſcribed.

† Though fully convinced of this, I could not bring the hunters to adopt the Chittigong method, till the year 1794. After this, during the laſt three years I remained at Tiperah, I did not loſe one elephant in twenty; whereas, by the former method, of tying them in the *roomee*, near one-third of thoſe taken died in leſs than a year, in conſequence of the hurts they received from their violent efforts to get free, before they could be properly ſecured. The natives of Tiperah, and indeed of moſt parts of India, are extremely attached to old cuſtoms; and it was with the utmoſt difficulty I prevailed on the hunters to deviate from the practice of their anceſtors, though the method recommended was followed at Silhet, as well as at Chittigong. The method was, ſimply to ſurround a herd, in the firſt convenient place, with a ditch and paliſade; and, when this was finiſhed, to ſend in the *koomkees*, and proper perſons to tie the wild elephants on the ſpot, and then conduct them, one by one, through an opening in the paliſade, from the *keddah*, as ſoon as they were tied.

been

been domeſticated for many years, when his paſſions were excited, and recollecting alſo, that a wild elephant had covered a female, in February, 1778, before many ſpectators, juſt after the herd had been ſecured in the incloſure, I was aſſured in my own mind, that it was not from any ſenſe of modeſty, either wild or tame elephants did not gratify their paſſions in public; but no opportunity offered of proſecuting this inquiry, till 1792. Having then taken upon myſelf the management of the elephant hunters, a very fine male was caught in November: he was both young and handſome, and alſo of a moſt docile diſpoſition; I therefore promiſed his driver a conſiderable gratuity, if he would get him into high order, ſo that I might have an opportunity of bringing his procreative powers to trial, with a tame female.

In the month of March, 1793, the driver of a favourite female elephant informed me, that ſhe had then ſigns of being in heat; and that, if the male and ſhe were kept together, and highly fed, an intimacy would probably ſoon take place. They were therefore, ſhortly after this, brought near to Comillah, where a ſpacious ſhed was erected for their accommodation.

In the day, they went out together, to feed; they alſo brought home a load of ſuch ſucculent food as their drivers and attendants could collect. After their return, they ſtood together, ſlept* near each other, and every opportunity was granted them to form a mutual attachment. In the evening, they had each from ten to twelve pounds of rice ſoaked in water, to which a little ſalt was added; and, from the middle of May till the latter end of June, ſome warm ſtimulants, ſuch as onions, garlic, turmeric, and ginger, were added to their uſual allowance of rice. Long before this, however, a partiality had taken place, as was evident from their mutual endearments, and careſſing each other with their trunks; and this without ceremony, before a number of other elephants, as well as their attendants.

Near the end of June, I was ſatisfied the male would not, even to regain his freedom, quit the object of his regard; I therefore ordered the keepers to picket the female, by one of her fore-legs only, in the houſe where they ſtood, but to leave the male at full liberty. Fearful, however, of hurting their ſuppoſed delicacy, and thinking the nearneſs and ſight of the attendants might poſſibly give umbrage to their modeſty, I deſired them to remain quiet in a little hut, erected on the outſide of the building appropriated to the elephants, where they could ſee equally well as if nearer.

On the evening of the 28th of June, 1793, the male was let looſe from his pickets; and, ſoon after, he covered the female without any difficulty, although before this ſhe never could have received the male, being taken when very young, about five years and a half prior to this period. The male was then led quietly to his ſtall; but, early on the morning of the 29th, he became ſo troubleſome, that the drivers, in order, as they ſaid, to quiet him, but

* It is always a good ſign, when an elephant lies down to ſleep, within a few months after he is taken; as it ſhews him to be of a good temper, not ſuſpicious, but reconciled to his fate. Elephants, particularly *goondabs*, have been known to ſtand twelve months at their pickets, without lying down to ſleep; though they ſometimes take a ſhort nap ſtanding.

partly,

partly, I suspect, to indulge their own curiosity, permitted him to cover her a second time; which he readily did, before the usual attendants, as well as a number of other spectators. After this, the driver brought me a particular account of the whole process. Though much pleased with the success of the experiment, yet I was rather chagrined he had not given me notice, that I might have been myself an eye-witness; and therefore told him, he should not receive the promised reward, till I had satisfied myself of the fact.

About two in the afternoon of the same day, I was desired to repair to the place where the elephants stood, as the male had been trying to get nearer the female. On this, I proceeded to the spot, with my friend captain Robert Burke Gregory: when we arrived, I ordered the male to be freed from his shackles; and, after some toying, and a few mutual caresses, we had the satisfaction to see him cover the female.

When the male mounted, he placed one of his fore-legs on each side of her spine, with his feet turned to, and pressing against, her shoulders, and his trunk over her forehead; supporting himself firmly in this situation, during coition, which he continued nearly the same time, and in the same manner, as a horse with a mare.

The female remained perfectly still, during the coitus. When the male had finished, he stood quietly by her side, while she caressed him with her trunk; and, as they then appeared well pleased, and gentle as usual, I went up and patted them both, as I had formerly been accustomed to do, without the smallest apprehension. In the evening, they were brought home to be fed; and, though only a few hours had elapsed since his last embrace, the male seemed inclined to make another attempt; to which I would have consented, to gratify a crowd of people then present, had I not now learned, that he had covered the female in the open plain, about ten in the morning, when going out for food, in spite of the exertions of the drivers and attendants; at least so they alleged, in excuse for having permitted it, contrary to my orders. As he had already covered four times in about sixteen hours, I was afraid a further indulgence might be prejudicial, and therefore would not permit it; especially as Mr. Imhoff, to whom he then belonged, was absent. That gentleman, however, returned two days after: but, when the two elephants were brought together, in order that Mr. Imhoff's curiosity might be indulged with so novel a sight, the female, being no longer in heat, was so uncivil as to give the male a kick in the face, when he was using what she then thought improper liberties; nor did she afterwards permit him to cover her, though, when standing together, they mutually indulged in a few caresses.

(To be continued.)

III.

Description and Use of a portable Instrument for comparing the Force of Gunpowder. By Citizen REGNIER *.

IN processes for the improvement of gunpowder, it is necessary to make comparative experiments. Different contrivances, exhibiting various degrees of ingenuity, have been made use of for this purpose, which are too well known to require description in this place. I shall, therefore, only remark, that trials of gunpowder, on a large scale, are always the best; but as these experiments require apparatus and conveniences, which are not in the power of every one, a small powder-proof has long since been received and commonly used, which is constructed in the form of a pistol, the blast from which drives a small toothed, or turned, wheel, which rubs against a spring, and is moved through a greater or less space, according to the force of the powder. But this machine, which is very defective in its results, and can be of no value when an absolute, or at least approximate, indication is required of the relative forces of a given measure of powder intended for fire-arms, which are charged with quantities determined in that manner.

In fact, these proof instruments have an arbitrary graduation. Their friction varies according to the force of the spring, and the cleanness or oxydation of the mechanism.

To obviate these different inconveniences, I made a number of experiments, which led me to a more valuable, and, in fact, a much more accurate principle; and, at length, with some modifications, I adapted a small brass cannon to the spring of the common weighing instrument. By this application, I immediately acquired the means of weighing the effort of the blast, and, consequently, a comparable method of estimating its action. This machine has likewise the valuable property of operating without friction. Its graduation is justly deter-

* From Mémoires explicatifs du Dynamometre et autre Machines inventées par le C. Regnier. 36 Pages, in Quarto, printed at Paris in the Year VII. The instruments descibed in this pamphlet are; 1. The dynamometer, which, by means of apparatus connected with a strong spring, measures the reaction exerted against the powers of men, horses, and other agents, at work. 2. A safeguard for the priming, in musketry. It consists of a cylindrical piece in brass, out of which the cavity, or pan for holding the priming, is excavated, and another hollow cylindrical piece, which covers the first, and is cut through in such manner as to present an opening corresponding with that of the pan. When this hollow cylinder is so disposed, that the two openings correspond, the musket may be discharged; but when, by turning the external part half round, the pan becomes completely covered, the priming is very secure, and the piece cannot be discharged either by accident or design. 3. The powder-proof, which is the subject of the present article; and, 4. An electrical machine. This machine operates by the friction of a circular plate. The principal singularity of this apparatus, is the conductor, which is not cylindrical, but has the form of a flat table, with thick rounded edges to prevent the escape of the electric fluid. A plate of coated glass is attached to the lower surface, which answers the same purposes of convenience and advantage, as the jars which Nairne put into cylindrical conductors about sixteen years ago. Electricians will not require that the conveniences of the flat conductor, or table, should be here pointed out. N.

mined,

mined, becaufe it expreffes the weight which ferved for its graduation, and the inftrument may likewife be ufefully applied for weighing fuch bodies as fall within the limits of its fcale.

The little attention I paid to render this inftrument public, and the prejudice which, in moft cafes, gives a preference to things brought from foreign countries, have caufed it to be defignated as the Englifh or German powder-proof; but the fact is, that I invented it myfelf in Burgundy, where, in the time of the academy of Dijon, it was confidered as the beft inftrument, on a fmall fcale, for determining the ftrength of gunpowder.

Defcription. Plate IX. fig. I. A. A perfpective view of the inftrument. B. C. A fpring bended into an angle. D. A fmall cannon of brafs, containing exactly a gramme of fine gunpowder ($15\frac{1}{2}$ grs). E. Arc of divifion graduated into kilogrammes (nearly 2lbs and a quarter avoirdupois each), terminating in a fcrew, which ferves as the breech-pin to the cannon. F. A cap, which may be confidered as the projectile. It clofes the mouth of the fmall cannon, upon which it preffes with a force equal to four kilogrammes. This ftopper is firmly fixed to one of the fpring radii, by means of a nut at the oppofite end of its branch or tail. G. A piece of hard brafs wire fixed to a projection, rivetted into one of the radii, by means of a fmall fcrew. H. An index of woollen cloth, or leather, which flides by a gentle friction upon the wire, when the branches of the fpring are preffed together by the inflammation of the powder. This index remains at the place to which it has been pufhed, and fhews precifely the diftance to which the fprings have been made to approach.

Method of ufing this inftrument. 1. The two extremities of the fpring are to be preffed together, in order to feparate the cap from the mouth of the fmall cannon. 2. Powder is poured either by means of card or paper into the cannon, fo as exactly to fill it; the ftopper is then fuffered to apply itfelf gently to the muzzle, and clofe it exactly, without leaving any grain of powder between them. 3. The circular piece of cloth, or leather, which ferves as the index, muft be moved into contact with that branch of the fpring, to which the tail of the ftopper is fixed. 4. Priming being then put into the fmall pan of the cannon, it muft be difcharged, holding the inftrument fufpended by the ribband or ftring which is paffed through the angular bend.

The effects of the explofion are, that the powder occupying a greater fpace by its inflammation, drives back the ftopper, which carries with it that branch of the fpring in which its tail is firmly fixed. This branch cannot move relatively to the other, which is alfo moved by the recoil of the gun, without driving before it the fmall index piece, and the fpace through which this is carried will fhew the force of the powder.

The arcs moved through by the explofion of different famples of powder, will fhew, by the numbers of their graduations, the comparative forces of each. There is a ftar marked on the graduated arc which fhews the force of powder of a medium quality, in order that the relative value of any fample, with regard to that medium, may be known. The friction of the index is the only friction to which this inftrument is fubject, and this is fo flight, that it may be confidered as nothing. The index itfelf may be eafily renewed when worn out.

It is obvious, that by adding a hook to the perforation at the extremity of the divided arc, and a ring in the eye of the ſtopper, this inſtrument may be uſed for weighing: the diviſions are half kilogrammes; (they might conveniently be pounds avoirdupois).

IV.

An Account of ſome Endeavours to aſcertain a Standard of Weight and Meaſure. By Sir GEORGE SHUCKBURGH EVELYN, *Bart. F.R.S. and A.S.* *Continued from p.* 157.

(§. 35.) THE chief ſtandards of longitudinal meaſure, as far as I can learn, that carry any authority with them, are thoſe preſerved in the exchequer; in the houſe of commons; at the Royal Society, and in the Tower. The firſt alone, indeed, bear legal authority, and have been in uſe for more than 200 years; the laſt is conſidered as a copy of them, and is not uſed for ſizing generally. The two remaining ones are of modern date; and, although they do not carry with them *at preſent* any ſtatuteable authority, yet, from the high reputation and acknowledged care of the artiſts who made them (the celebrated Mr. George Graham, and Mr. John Bird), are undoubtedly entitled to very great reſpect; and are probably derived from a mean reſult of the compariſons of the old and diſcordant ones in the exchequer. I ſhall begin with that of Mr. Graham, which contains alſo the length of the Tower ſtandard laid down upon it; will proceed then to Mr. Bird's, and finally conclude with thoſe at the exchequer.

(§. 36.) May 5, 1797. I went to the apartments of the Royal Society, at Somerſet Houſe, and, with the ready aſſiſtance of Mr. Gilpin, at the kind inſtance of ſir Joſeph Banks, I made the following obſervations on Mr. Graham's * braſs ſtandard yard, made in 1742. This ſcale is about 42 inches long, and half an inch wide, containing three parallel lines engraven thereon, on the exterior and ulterior of which are three diviſions, expreſſing feet, with the letter E at the laſt diviſion; and, by a memorandum preſerved with it in the archives of the ſociety, is ſaid to ſignify Engliſh meaſure, as taken from the ſtandard in the Tower of London. That with the letter F denoting the length of the half of the French toiſe, put on here, by the authority and under the inſpection of the Royal Academy of Sciences, then ſubſiſting at Paris, to whom it was ſent in 1742, for the purpoſe of comparing the French and Engliſh meaſures. The middle line, marked Exch. of the three abovementioned, denotes, as is ſuppoſed, the ſtandard yard from the exchequer.

(§. 37.) This bar of Mr. Graham's had been previouſly laid together with my ſcale divided by Mr. Troughton, for twenty-four hours, to acquire the ſame temperature; they were alſo of the ſame metal, and, by placing it under my microſcopes, adjuſted to the interval between 10 and 46 inches, I found the interval on the Tower ſtandard exceed

* This rod was not made by Mr. Graham, but, at his inſtance, procured by him from Mr. Jonathan Siſſon, a celebrated artiſt of that time. See Phil. Tranſ. Vol. XLII.

mine,

inches.
mine, by — 0,00127 ,00135 ,00128 ; Mean = ,00130 = the total length therefore 36,00130 inches, the therm. at 60°,8.

The interval on the line marked Exch. was shorter than mine

inches.
by - — ,0066 ,0066 ,0068 ; ,0067 = the total length = 35,9933 inches, the therm. at 60°,6.

And the Paris half-toise, which had been supposed by the Academy to be = 38,355 English inches, was found, compared

inches. inches.
with mine, to be = 38,3561 ,3563 ,3559 Mean = 38,3561 *.

* Dr. Maskelyne says, this standard yard of Mr. Graham's was $\frac{1}{1000}$ inch longer on three feet than Mr. Bird's divided scale, which he generally made use of in all his operations of dividing; and, from one made conformably to this of Mr. Bird's, Mr. Troughton divided my scale of 60 inches. This remark seems to agree with my 1st and 3d comparison, but not with the intermediate one. See Phil. Trans. for 1768, p. 324.

As I am now upon the subject of foreign measure, it may not be quite out of place to say a word on the length of the ancient Roman foot, which I am enabled to do with some precision.

Some years ago, when I was in Italy, I had several opportunities of ascertaining the length of this measure, by actual examination of the Roman foot rules, of which I have met with nine, viz. two in the Capitol at Rome; one in the Vatican; five in the museum at Portici, near Naples; and, lastly, one in the British museum, sent from Naples by sir William Hamilton. They were all of brass, except one half-foot, of ivory, with a joint in the middle, resembling our common box or ivory rules: and, by reference to my journal kept at that time, I find the mean result from all the nine rules, viz. by taking both the whole and the parts of each (for they were divided into 12 inches, and also into 16ths, or digits), gave, for the length of the old Roman foot, in English inches, correspondent to Mr. Bird's measure, = 11,6063.

In confirmation also of this conclusion, and agreeably to the idea of M. de la Condamine, in the "Journal of his Tour to Italy," I took the dimensions of several ancient buildings, viz. the interior diameter of the temple of Vesta; the width of the arch of Severus; the door of the Pantheon; and the width of the base of the quadrilateral pyramid of Cestius, which, it is curious to observe, I found exactly 100 old Roman feet, and 125 feet high. This I do not remember to have seen noticed by any former traveller.

The mean result of these experiments gave me - - - - - 11,617 English inches.
Ditto, as before, from the rules - - - - 11,606 ditto.
The mean of the two modes of determination is - - - 11,612 ditto.

I may add, that in the Capitol is a stone, of no very ancient date however, let into the wall, on which is engraven the length of several measures, from whence I took the following:

The ancient Roman foot, = 11,635 English inches.
The modern Roman palm, = 8,82 ditto.
The ancient Greek foot, = 12,09 ditto.

The

	inches.
The 1ſt of the preceding obſervations giving - - - -	36,0013
The 2d - - - - - - -	35,9933
The mean length of Mr. Graham's ſtandard becomes - - -	35,9973

(§. 38.) From the information in the report of a committee of the houſe of commons, that ſat in the year 1758, I learnt that Mr. Bird's parliamentary ſtandard had been in the cuſtody of ſome of its officers, but of whom nobody knew: however, under the authority of the ſpeaker, who was ſo good as to furniſh me with a room in his houſe, to make the compariſons in, I at laſt diſcovered this valuable original in the very ſafe keeping of Arthur Benſon, eſq. clerk of the journals and papers, and which, I believe, had never ſeen the light for five-and-thirty years before. It is a braſs rod or bar, about 39 inches long, and 1 inch ſquare, incloſed in a mahogany frame, inſcribed "ſtandard Geo. 2d. 1758;" at each extremity of it is a gold pin, of about $\frac{1}{15}$ inch in diameter, with a central point, and theſe points are diſtant = 36 inches. It bears, however, no diviſions; but there was found with it, in another box, a ſcale divided into 36 inches, with braſs cocks at the extremities, for the purpoſe of ſizing or gauging other ſcales or rules by. Beſides theſe, I found another ſtandard, in ſize, and in all reſpects, ſimilar to the laſt, inſcribed 1760, having been made for another committee, that ſat in that year; this alſo was accompanied with a ſimilar divided ſcale of 36 inches.

Theſe bars being too thick to be conveniently placed under the microſcopes of my inſtrument, the interval of 36 ſtandard inches was laid down on my ſcale with a beam-compaſs, two fine points made, and, compared with Troughton's diviſions, was = 36,00023 inches; the thermometer being at 64°. I then examined the other ſtandard, marked "ſtandard, 1760," and found it to agree exactly with that of 1758; at leaſt it did not differ from it more than ,0002 inch *.

(§. 39.) I was now to examine the old ſtandards kept in the exchequer: theſe Mr. Charles Ellis, deputy chamberlain of the tally court at the receipt of the exchequer, was ſo good as to ſupply me with; viz. the ſtandard yard of the 30th of Eliz. 1588, and alſo the ſtandard ell of the ſame date. Theſe are what have been conſtantly uſed, and are indeed the only ones now in uſe, for ſizing meaſures of length †. They are made of braſs, about 0,6 inch ſquare, and are very rudely divided indeed, into halves, quarters, eighths, and ſixteenths; the lines being two or three hundredths of an inch broad, and not all of them drawn ſquare, or at right angles to the ſides of the bar, ſo that no accuracy could poſſibly be expected from ſuch meaſures. However, the middle point of theſe tranſverſe lines, between the ſides of the bar, was taken as the intended original diviſion; and theſe diviſions, ſuch as

* Theſe quantities then being ſo ſmall, I ſhall conſider them as wholly inſenſible; and ſhall ſay, that Mr. Bird's parliamentary ſtandards of 3 feet exactly correſpond with Mr. Troughton's ſcale.

† There was alſo a ſtandard yard of Henry VII. but of very rude workmanſhip indeed; now quite laid by, and at what time laſt uſed, no information remains: but of this more hereafter.

they

they were, were transferred, by a dividing knife, to the reverſe ſide of my braſs ſcale made by Mr. Troughton, the thermometer being at 63°; and, at my leiſure afterwards, I found as follows.

The ends of theſe venerable ſtandards having been bruiſed a little, or rounded, in the courſe of ſo many years' uſage, I conceived a tangent to be drawn to the moſt prominent part, which was about the centre or axis of the bar, and this point being referred to Troughton's ſcale, between 6 and 42 inches, the entire yard of 1588, meaſuring from one extremity to the other, was found to be ſhorter than this, by —,007 inch: but theſe compariſons will be better exhibited in a table.

Exchequer ſtandard of 1588.	Difference from Troughton.	Length in inches.	Difference on 36 inches.	Mean difference on 36 inches.
	Inches.			
Entire yard - -	— ,007	35,993	— ,007	Inches.
$\frac{1}{2}$ yard, from 24 to 42 inches	+ ,063	18,063	+ ,126	= + 0,015
$\frac{3}{4}$ yard, from 15 to 42 inches	— ,008	26,992	— ,011	
$\frac{7}{8}$ yard, from $10\frac{1}{2}$ to 42 inches	+ ,022	31,522	+ ,025	
$\frac{15}{16}$ yard, from $8\frac{1}{4}$ to 42 inches	— ,055	33,695	— ,059	
Entire ell, from 2 to 47 inches	— ,036	44,964	— ,029	
$\frac{1}{2}$ ell, from 2 to $24\frac{1}{2}$ inches	+ ,032	22,532	+ ,052	= + 0,016
$\frac{3}{4}$, from 2 to $35\frac{3}{4}$ inches	+ ,017	33,767	+ ,018	viz. the exchequer
$\frac{7}{8}$, from 2 to 41,375 inches	— ,001	39,374	— ,001	meaſure is by ſo much
$\frac{15}{16}$, from 2 to 44,1875 inches	+ ,051	42,239	+ ,043	the longer, or about 1 in 2322.

(§. 40.) It appears then, from the above table, that the ancient ſtandards of the realm differ very little from thoſe that have been made by Mr. Bird, or Mr. Troughton, and conſequently, even in a finance view (if one might look ſo far forward), nothing need be apprehended, of loſs in the cuſtoms, or exciſe duties, by the adoption of the latter.

(§. 41.) I ſhall now endeavour to ſhew the proportion of the weights that I have uſed, compared with the ſtandards that were made by Mr. Harris, aſſay maſter of the mint, under the orders * of the houſe of commons, in the year 1758. They are kept in the ſame cuſtody with Mr. Bird's ſcales of length, and appear to have been made with great care, as a mean reſult from a great number of compariſons of the old weights in the exchequer, which have been detailed at length in that report. Mr. Harris having been of opinion that the troy pound was the beſt integer to adopt, as the ſtandard of weight, I venture to conclude that this was the moſt accurate, and moſt to be depended upon, of all the various weights and duplicates that he made for the uſe of this committee; for he made them of 1, 2, 4, 8, 16, lb. and of $\frac{1}{2}$, 1, 2, 3, 6 ounces. It will therefore be ſufficient for my purpoſe, to compare the 1 and 2 pounds troy, and their duplicates, with the weights of Mr. Troughton.

I did this, June 2d, 1797; the barometer being at 29,72 inches, and thermometer 67°.

* See the report referred to in the note of page 106.

The

	Troughton's weights. lb. grains.	grains.
The ſtandard weight of 1 troy pound, or 5760 grains, marked 1758, kept at the houſe of commons, in a ſmall box by itſelf, by Mr. Benſon, weighed - - - -	= 1 3,75 / ,74	= 5763,745
A duplicate of the preceding, kept with ſome other weights, in a box marked B - - - - -	= 1 3,70 / ,67	= 5763,685
The mean weight of the troy pound, from theſe two - - -		= 5763,715
The two-pounds weight, from the houſe of commons, kept in a deal box, marked A - - -	= 10000 / 1000 / 400 / 100 / 20 / 7 / 0,84	= 11527,84
A duplicate of the laſt-mentioned 2lb. weight, preſerved in a deal box, marked B - - - - The thermometer now ſtood at 68°.	10000 / 1000 / 400 / 100 / 20 / 7 / 0,55	= 11527,55
Therefore the mean weight of 2lb. troy, from the two laſt trials, is -		= 11527,70
And conſequently 1lb. becomes - - - - - -		5763,85
But, from the examination of the two ſingle pound weights, as above, 1 pound is		5763,71
Therefore the mean of all is - - - -		= 5763,78
That is, Mr. Troughton's weights are too light by $\frac{378}{576000}$ -		= 0,6562

grain on 1000 grains, or 1 in 1523,92 grains.

(§. 42). In concluſion, it appears then that the difference of the length of two pendulums, ſuch as Mr. Whitehurſt uſed, vibrating 42 and 84 times in a minute of mean time, in the latitude of London, at 113 feet above the level of the ſea, in the temperature of 60°, and the barometer at 30 inches, is = 59,89358 inches of the parliamentary ſtandard; from whence all the meaſures of ſuperficies and capacity are deducible.

That, agreeably to the ſame ſcale of inches, a cubic inch of pure diſtilled water, when the barometer is 29,74 inches, and thermometer at 66°, weighs 252,422 parliamentary grains; from whence all the other weights may be derived.

As a ſummary of what has been done, I hope it may now be ſaid, that we have attained theſe three objects;

1ſt. An invariable, and at all times communicable, meaſure of Mr. Bird's ſcale of length, now preſerved in the houſe of commons; which is the ſame, or agrees within an inſenſible quantity, with the ancient ſtandards of the realm.

2dly. A ſtandard weight of the ſame character, with reference to Mr. Harris's troy pound.

3dly. Beſides the quality of their being invariable (without detection), and at all times com-

communicable, thefe ftandards will have the additional property of introducing the leaft poffible deviation from ancient practice, or inconvenience in modern ufe.

(§. 43.) Before I clofe this paper, after having faid fo much on the fubject of weights and meafures, it may not be improper to add a few words upon a topic that, although not immediately connected, has fome affinity to it; I mean the fubject of the prices of provifions, and of the neceffaries of life, &c. at different periods of our hiftory, and, in confequence, the depreciation of money. Several authors have touched incidentally upon this queftion, and fome few have written profeffedly upon it; but they do not appear to me to have drawn a diftinct conclufion from their own documents. It would carry me infinitely too wide, to give a detail of all the facts I have collected; I fhall therefore content myfelf with a general table of their refults, deduced from taking a mean rate of the price of each article, at the particular periods, and afterwards combining thefe means, to obtain a general mean for the depreciation at that period; and laftly, by interpolation, reducing the whole into more regular periods, from the conquefts to the prefent time: and, however I may appear to defcend below the dignity of philofophy, in fuch œconomical refearches, I truft I fhall find favour with the hiftorian, at leaft, and the antiquary *.

(*The author's appendix hereafter.*)

V.

Defcription of a new Arrangement of the Bars in the Gridiron Pendulum. With a Drawing.

(*W. N.*)

FIG 2, 3, 4, &c. in plate IX. reprefent a pendulum which I conftructed fome months ago. It differs only in the arrangement from the modern gridiron pendulum of five bars. The fame letters denote the fame parts in all the figures. A A is a cylindrical box of brafs, filled with lead, conftituting the principal mafs of the pendulum. In fig. 4 a fection of this piece is feen in the fection of its axis. A piece of brafs, P P P P, is let through the middle of this cylinder and firmly fecured by a flanch piece fcrewed to the brafs face. Two holes are made quite through the cylinder, and the piece of brafs P P, parallel to the diameter of its face, for the purpofe of fuffering the two fteel rods B B to pafs through. Two fteel pieces, L and M N O, are filled to fquare cavity in the piece P P. Thefe pieces, of which fig. 7 gives a vertical fection, and fig. 8 the fection parallel to the horizon, are applied together, by paffing the fcrew M through the end hole in L, and then fcrewing on the milled head or nut K, in which fituation the two fteel pieces fit the fquare hole; the vertical hole in L being applied over the long hole N, and nearly coinciding with one of the diametral perforations in A A, while the other, O, nearly coincides with the other perforation. If

* This table has already been given in our Journal, vol. II. 284.

in this ſituation, while the nut K is but looſely ſcrewed on, the bars B B be paſſed through the perforations in the cylinder, the braſs and ſteel pieces, and K, be then ſcrewed up, it will cauſe the vertical hole O, in the one ſteel piece, and L in the other, to approach nearer together, and carry the bars B B along with them. But this can take place only to a very ſmall extent, becauſe the bars will be preſſed againſt the inner ſurfaces of the holes in the braſs piece P P, which very nearly fit thoſe bars. The uſe of the milled head K is, therefore, to diſengage the weight A A from the bars B B, in which ſituation the former may either be taken off, or ſlided up and down to any deſired poſition, where it may immediately be fixed by turning the nut.

Fig. 2 ſhews the whole pendulum, which vibrates half ſeconds, in its full dimenſions, excepting as to its length, which is diminiſhed by a fracture, repreſented in the middle of the bars, to get it into the plate. The ſyſtem of bars conſiſts of a croſs-piece, G G, attached to another piece, H H, to which the ſprings of ſuſpenſion are fixed as uſual. From G G proceed downwards two ſteel bars, which paſs through holes in the braſs ſtage E E (repreſented alſo in fig. 5), with which they have no connection, and are firmly ſecured in the ſtage, D D, which is ſimilar to E E. From the ſtage E E proceed a light pair of ſteel bars, downwards through the ſtage D D, without connection with this laſt, and thence through the cylinder A A, where they appear at B B. And, laſtly, from the centre of the lower ſtage D D, proceeds upwards a bar of the ſame dimenſions, but compoſed of zinc and ſilver, forming a compound, which expands more than twice as much by heat as ſteel does. This bar paſſes through the ſtage E E, till it almoſt reaches G G. It is repreſented by the dotted outline. The ſtage E E may be ſlided up and down, ſo as to vary its diſtance from D D; but when required to be fixed, the ſcrew pin F is to be turned, which cauſes a piece to preſs againſt the central bar and binds it faſt.

It is ſcarcely neceſſary to explain this action. The pendulum A A hangs upon the ſteel bars B B, which are fixed in the ſtage E E. This ſtage is ſupported by the central rod or pillar of zinc and ſilver which bears on the ſtage D D, and this laſt is ſuſpended from G G by the bars C C. If, therefore, the expanſion of the compound metal between the ſtages E and D be equal to the whole expanſion of the ſteel rods, and the reſt of the apparatus downwards, the centre of A A will remain at the ſame diſtance from the axis of motion whatever may be the temperature. But if the effect of the ſteel be greateſt, it will be neceſſary to raiſe the ſtage E and fix it higher, or in the contrary caſe it muſt be fixed lower; taking care to preſerve the due ſituation of A A with reſpect to I, by means of its apparatus K.

This may ſeem to be but a rough method of adjuſting for temperature. But when it is conſidered that the whole daily difference in a ſimple pendulum with a ſteel rod amounts to ſix ſeconds at a mean between ſummer and winter *, it will be readily underſtood that the

* Philoſ. Journal, i. 58.

purpoſe

purpose of compensation for temperature will be obtained, as nearly as observation can indicate its changes, by making the lengths of the bars inversely as their expansions, as known from general experiment, or from tables. Thus, for example, if brass and steel were used, and their expansions taken as 113 to 68, and the whole length of the brass were 10 inches, we may conclude that the changes of temperature would not alter the pendulum, provided the above proportions were accurately kept, namely, that the length of the steel were 16.62 inches. But if the proportions either of expansion or length should be so far mistaken or neglected, as that the brass should be made one-tenth of an inch too short, = 9.9 inches, this piece would compensate only 16.56 inches of steel, and consequently (the steel being lengthened 0.1 inch) there would remain 0.16 inch of steel uncompensated; or 0.016 part of the whole pendulum, which would produce a difference of less then one-tenth of a second between the mean daily rates in summer and winter.

The adjustment for time is made by a screw adapted to the springs of suspension, or by any other of the usual methods; of which I do not mean at present to consider the advantages and defects.

VI.

On the Epocha of the Discovery of the Telescope, and the Opinion of Boyle, that Plants derive their Nourishment from Water only. By Citizen BOISSONADE *.

WE sometimes find in books which are little known and read, particulars of information of a very interesting nature. I have found in the *Probabilia* of the learned Wesseling, editor of Diodorus Siculus, a chapter (the eleventh) on the antiquity of astronomical telescopes, of which I send you a translation. The researches it contains appear sufficiently curious to deserve to be better known.

The astronomical telescope †, by means of which the moderns have made discoveries in the heavens, which escaped the penetration of the ancients, is, with justice, placed among the most memorable inventions. It has often been enquired whether they were known to the ancient astronomers, and several writers have decided for the affirmative. They ground their opinion on an ancient drawing in the scholastic history of Peter Comestor, in which Conrad, a monk of the thirteenth century, had represented astronomy ‡. "On the right hand of the figure," according to father Mabillon, who had seen the manuscript, and mentions it in his travels in Germany, there was "a figure of Ptolemy observing the stars, with a long instrument resembling an astronomical telescope with four sliding tubes §." This conjecture

* In a letter to citizen Millin, inserted in the Magasin Encyclopédique, v. 466.

† I thus translate the words *tubi optici*. Note of the writer.—His object is to avoid the ambiguity of the French word *lunettes*, which signifies *spectacles* as well as *telescopes*. I have not the original work. N.

‡ The Latin of Mr. Wesseling is not clear in this passage, for the understanding of which, it is necessary to refer to the work of P. Mabillon, from which he quotes only a few lines.

§ Ope instrumenti longioris quod instar tubi optici, quatuor ductus habentis, concinnatum est.

was fortified by a paſſage of Ditmar, biſhop of Merſbourgh, who, at the end of his fixth book, ſpeaking of Gerbert, afterwards pope Silveſter II. expreſſed himſelf thus: "He perfectly knew the courſe of the ſtars, and exceeded his cotemporaries in the variety of his knowledge. When driven from his own country, he repaired to the emperor Otho, at whoſe court he remained a long time. He conſtructed a clock at Magdebourgh, and to aſcertain its accuracy, he obſerved with a tube the ſtar which directs mariners *." It is thought that Ditmar, by the word tube, meant the aſtronomical teleſcope, which, in fact, has a tube. Strabo alſo mentions a tube of this kind, if we admit that the text is not in this place corrupted. "The diſc of the ſun acquires a more conſiderable apparent magnitude at the riſing and ſetting of that luminary out at ſea, on account of the quantity of vapours which riſe from the water, and becauſe the ſight, refracted through theſe vapours, *in the ſame manner as through tubes* †, receives the images of a larger ſize."

Theſe paſſages appear to me to be the ſtrongeſt which have been brought in proof that teleſcopes were known to the ancients. But it is very ſurprizing that this knowledge, if it exiſted, could have been loſt to ſuch a degree, that before the ſeventeenth century the moſt ſkilful and learned aſtronomers ſhould not even ſuſpect its exiſtence. Men have, at all times, applied to ſtudy the courſe and poſition of the ſtars. No one is ignorant with what ardour the Greeks, the Arabs, and the Latins, cultivated aſtronomy, and how much this ſtudy, encouraged by the liberality of princes, has been conſtantly purſued. Can it be ſuppoſed that the ancient obſervers deſpiſed an inſtrument ſo uſeful to their labours? I admit that much of the knowledge of antiquity has been loſt by wars, by ravages, and a multiplicity of other cauſes; but if this ſubject be attentively examined, it will be ſeen that the information which has been loſt, relates either to objects of luxury, or is of ſuch a nature, that greater ſubſequent diſcoveries muſt have cauſed them to be neglected; and ſtill they are not ſo loſt but that ſome trace of their ancient exiſtence remains. But this is not the caſe with teleſcopes; for we have no deſcription in the writings of the ancients which can agree with them. Strabo ſpeaks of tubes αυλῶν; but nothing leads us to apprehend that they were uſed to obſerve the magnitude and courſe of the ſtars; for I cannot ſay that I place much dependance on the conjecture of Theodore Almeloven, who thinks that Strabo wrote ὑαλων (glaſſes). In fact, cryſtal, or glaſs, is particularly endued with the property mentioned in this paſſage of the geographer. But let us ſuppoſe that Strabo wrote αὐλῶν (tubes), who ſhall aſſure us that theſe tubes were provided with glaſſes? The ſame queſtion may be offered reſpecting the painting of the monk Conrad. In fact, though it repreſents an inſtrument conſiderably reſembling a teleſcope, who can prove that this inſtrument contained glaſſes like our teleſcopes, which without them would be of little utility? I am almoſt certain that tube of Gerbert, of which Ditmar ſpeaks, had them not; and in order that I may not be accuſed of deciding eaſily, I offer the following proof. Father Mabillon, in his Analecta, has

* Conſiderata per fiſtulam quadam ſtella nautarum duce.

† ὡσ δι αὐλῶν. Lib. 3. p. 138.

publiſhed

published a letter of Gerbert to the monk Constantin, on the construction of the sphere, in which he says, "so that there will be seven perforations, in each of which you will place tubes (fistulas) of six inches. The two extremes, or most remote, are to be placed opposite each other in such a manner that both may be seen through as if composing one tube.— These tubes differ from musical pipes (fistulis organicis) in the equality of their diameter throughout, for fear lest any obstacle should impede the sight while occupied in observing the celestial bodies." If these tubes differed from those of musical instruments only in the uniformity of their bore, it is clear that they had not glasses; consequently they were not telescopes, but dioptræ, and the glory of the invention of telescopes must continue to belong to the seventeenth century.

I do not consider the celebrated opinion of Vanhelmont and Boyle as equally new, who, from well-made experiments, thought that trees and plants derive their principal nourishment from water. These great observers have informed us, that if a determinate quantity of earth be weighed, and if a twig be planted in the vessel containing this earth, it will be found several years afterwards upon transplanting the tree, that the same quantity of earth will remain as at first. Several centuries before Boyle and Vanhelmont, the author of the Clementine Recognitions had the same idea. His words, book viii. chap. xxvii. are: "In order to prove by experiment that seeds draw nothing from the earth, but are entirely supported by water, let us suppose that the weight of one hundred talents of earth to be put into a very large receptacle, and that the seeds of various species of plants be sowed therein, and abundantly watered, for several years; let us, moreover, suppose that all the seeds of wheat, barley, and other vegetables, thus annually produced, to be collected until the quantity of grains of each shall weigh one hundred talents; if at this period the roots be drawn out, there will still be found in the vessel from whence so great a product has been obtained, the original weight of one hundred talents of earth. Whence can we say that this quantity of seeds and trees was formed? Is it not evident that it has been produced from the water?" This passage contains exactly the system of Boyle and Vanhelmont. I do not, however, think they have borrowed it. Accurate observers of nature are led by their observations to very similar conclusions.

So true it is, as Terence affirms, that,

Nullum jam dictum est quod non dictum sit prius.

I may add to this chapter of Mr. Wesseling, which has been inserted in the Journal des Savans, for February, 1679, the different proofs of the existence of telescopes in the thirteenth century. It would follow also, from a passage of John, abbot of Beaugency, that spectacles were used for reading in the twelfth century, if the word bustula, used by this abbot, may be translated spectacles. The following is the passage: "Statim ut litterarum vestrarum ba-"julam vidi, bustulam arripiens, non solum avide legi et perlegi *." The Journal Encyclopédique,

* This passage has very often been quoted in favour of the invention of spectacles in the thirteenth century. It is thought, nevertheless, with much probability, that the word bustula comes from the word buxus

pédique, for May, 1766, in which there is a long article on the ſubject, gives his ſenſe to the word, and adds, that the principles, according to which ſpectacles are fabricated, are found in Euclid and the ancient geometers.

VII.

*Obſervations on the Proportion of real Acid in the three ancient known mineral Acids, and on the Ingredients in various neutral Salts and other Compounds.** By RICHARD KIRWAN, Esq. LL.D. F.R.S. and M.R.I.A.

THE fundamental experiments on which the proportion of real acid in the three mineral acids anciently known, and alſo the proportion of ingredients in many neutral ſalts, were determined, are ſet forth in a paper of Mr. Kirwan, in the IVth vol. of the Tranſactions of the Iriſh Academy. That paper contains tables of the quantity of ſtandard acid exiſting in 100 parts of each of the acid liquors, of given ſpecific gravities, and alſo in each of the neutral ſalts therein mentioned, according to a method which Mr. Kirwan has ſince diſcovered to be very inconvenient, becauſe ſome of theſe neutral ſalts contain an acid ſtill ſtronger than the aſſumed ſtandard. It is there alſo noticed, that the ſtrongeſt vitriolic acid now known exiſts in *vitriolated tartarin*, the ſtrongeſt nitrous acid in *nitrated ſoda*, and the ſtrongeſt muriatic acid in *muriated tartarin*. Acids of ſuch ſtrength he has, therefore, denominated *real*, as either containing no water, or containing only as much as is neceſſary to their eſſential compoſition, as far as is at preſent known. The method of transforming the expreſſion *ſtandard* into that of *real*, is given in the ſame paper at p. 67, and by it he has formed the table here preſented. This latter expreſſion he therefore employs in every caſe inſtead of that of *ſtandard*, together with the ſubſtitution of a more commodious expreſſion of the ſtrength of acids. Another object of his preſent communication is, to exhibit an illuſtration or amendment of ſeveral of the determinations contained in his laſt, which, being for the moſt part ſingle, required confirmation, by ſhewing their agreement with the experiments of ſeveral of the moſt eminent chemiſts, made ſince that publication, that is, ſince the year 1791, with a few made nearly at the ſame time. In his former paper he compared his reſults with thoſe of Bergman and Wenzel, they being almoſt the only perſons who had made this ſubject the principal object of their enquiry, and had purſued it to a conſiderable extent. In each particular inſtance he traced the reaſon of the difference of their reſults from his own, when it was ſuch as to deſerve notice: he does not, therefore, repeat what was there obſerved; but cannot avoid again men-

buxus (box), and denotes the little box in which the letter was contained. According to this interpretation, the above may be thus tranſlated. "As ſoon as I ſaw the bearer of your letters, I ſeized the ſmall box which contained them, and frequently read them over with avidity;" and not that I ſnatched up my ſpectacles. Note of citizen Millin.

* Abridged from an extenſive paper, which will hereafter appear in the Tranſactions of the Royal Iriſh Academy. I was favoured, by the author, with this paper, entitled, *Additional Obſervations*, &c.

tioning

tioning one general source of error attending the mode of investigation adopted by both, and yet noticed by neither, namely, the loss that many neutral salts undergo during evaporation; a loss, whose discovery is of considerable importance, not only to the present enquiry, but also to the conduct of several manufactures, particularly to that of saltpetre, and hence noticed by Mr. Lavoisier, 15 An. Chem. 254.

Other remarks and observations respecting the experiments of Bergman, Wenzel, and other chemists, form a very essential and valuable part of the paper; but the indispensable attention to brevity, which the nature and object of a periodical work demands, will oblige me to confine this abstract to the immediate structure and use of the tables.

When alkalies or earths, combined with fixed air, are dissolved in acids, though far the greater part of the fixed air is expelled during the solution, yet some portion of it is often retained, and may, in some degree, alter the sp. grav. of the solution; this circumstance was not recollected by Mr. Kirwan till lately; it was first noticed by Mr. Cavendish, Phil. Trans. 1766, p. 172, and afterwards by Bergman, in his notes on Scheffer, § 51, but more explicitly by Scheele, Chy. An. 1786, p. 13, and by Butini on Magnesia, p. 149. As to the use resulting from researches of this nature, he remarks that it would be superfluous to attempt to prove it at this day; as the recourse which the most eminent analysts have been obliged to have to it in particular instances, sufficiently evinces it. "Inquiries of this kind (says Mr. "Fourcroy) are more difficult and delicate than those which have hitherto been made on "salts; whatever requires a precise knowledge of quantities and proportions, presents difficul-"ties so great as often to appear insurmountable, yet without this knowledge no progress "can now be made in chemistry," 10 An. Chy. 325; and according to Bergman, "Usus "cognitæ proportionis principiorum ingredientium egregius est et multifarius." 1 Bergm. 137. chap. 1. § 1.

The Experiments made for ascertaining the Changes of Density in the Acids by Change of Temperature, were as follow:

Vitriolic acid 1,8360 at temperature 60°, becomes 1,8292 at 70°;....1,8317 at 65°;.... 1,8382 at 55°;....1,8403 at 50°;....and 1,8403 at 49°.—Hence we see that vitriolic acid, whose density at 60°, is 1,8360, loses by *ascending* and gains by *descending* 0,00068 for every degree of temperature between 60° and 70°, and 0,00043 nearly by each degree between 60° and 49°.

Again, vitriolic acid 1,7005 at 60°, becomes 1,6969 at 70°;....1,6983 at 65°;....1,7037 at 55°; and 1,7062 at 50°.—Hence vitriolic acid, which at 60° is 1,7005, gains or loses 0,00036 nearly for every degree between 60° and 70°, and 0,00051 by every degree between 60° and 50°.

Lastly, vitriolic acid 1,3888 at 60°, becomes 1,3845 at 70°;....1,3866 at 65°;....1,3898 at 55°; and 1,3926 at 49°.—Hence vitriolic acid, which at 60° is 1,3888, gains or loses

0,00043

0,00043 nearly by every degree between 60° and 70°, and 0,00034 nearly by every degree between 49° and 60°; between 49° and 50° no difference was perceived.

The Alterations of Density from Difference of Temperature in Nitrous Acid, were as follow:

Nitrous acid, which was 1,4279 at 60°, became 1,4178 at 70°;....1,4225 at 65°;....1,4304 at 55°;....1,4336 at 50°; and 1,4357 at 45°.—Hence nitrous acid, which at 60° is 1,4279, gains or loses 0,00101 nearly by every degree between 60° and 70°; and 0,00052 by every degree between 45° and 60°.

It was formerly found by Mr. Kirwan, that the strongest *spirit of nitre* is most expanded by heat, or contracted by cold. Also, that nitrous acid, whose sp. grav. at 34° was 1,4750, was expanded by heat, as follows: viz. 1,4750 at 34° became 1,4653 at 49°; whence it gains or loses 0,0097 by 15°, between 34° and 49° inclusively.

Again, that colourless nitrous acid, whose sp. grav. was 1,4650 at 30°, became 1,4587 at 46°, and 1,4302 at 86°.—Whence, by the first 16°, from 30° to 46°, it gained 0,0063, and by 40°, that is, from 46 to 86°, it gained 0,0285.

Again, nitrous acid, whose density was 1,2363 at 60°, became 1,2320 at 70°;....1,2342 at 65°;... 1,2384 at 55°;....1,2406 at 50°; and 1,2417 at 45°.—Whence nitrous acid, which at 60° is 1,2363, gains or loses by every degree between 60° and 70° 0,00043, and 0,00036 by every degree betwen 60° and 45°; and we may assume 0,0005 as the variation incident to every degree between 60° and 70° in nitrous acid, whose density at 60° is between 1,3 and 1,4, and 0,0004 for the variation between 44° and 60°.

Of Marine Acid.—It was formerly found that this acid of the density 1,196 at 33° became of the density 1,1820 at 66°; the alterations of acids of lower sp. grav. were not examined; but it was found that in general its dilatability is greater than that of nitrous acid of the same density.

TABLE

TABLE *of the Quantity of Real Acid, in* 100 *Parts of Vitriolic, Nitrous, and Marine Acid Liquors of different Densities, at the Temperature of* 60°.

In *Vitriolic Acid* of different Densities, at the Temperature of 60°.

100 Parts Sp. Gravity.	Real Acid.	100 Parts Sp. Gravity.	Real Acid
2,0000	89,29	1,4666	44,64
1,9859	88,39	1,4427	43,75
1,9719	87,50	1,4189	42,86+
1,9579	86,61	1,4099	41,96
1,9439	85,71	1,4010	41,07
1,9299	84,82	1,3875	40,18
1,9168	83,93	1,3768*	39,28
1,9041	83,04+	1,3663	38,39
1,8914	82,14	1,3586	37,50
1,8787	81,25	1,3473	36,60
1,8660	80,36	1,3360	35,71
1,8542	79,46	1,3254	34,82
1,8424	78,57	1,3149	33,93
1,8306	77,68	1,3102	33,03
1,8188	76,79+	1,3056	32,14
1,8070	75,89	1,2951	31,25
1,7959	75,—	1,2847	30,35
1,7849	74,11	1,2757	29,46
1,7738	73,22	1,2668	28,57
1,7629	72,32	1,2589	27,68+
1,7519	71,43	1,2510	26,78
1,7416	70,54+	1,2415	25,89
1,7312	69,64	1,2320	25,—
1,7208	68,75	1,2210	24,10
1,7104	67,86	1,2101	23,21
1,7000	66,96	1,2009	22,32
1,6899	66,07	1,1918	21,43+
1,6800	65,18	1,1836	20,53
1,6701	64,28	1,1746	19,64
1,6602	63,39	1,1678	18,75
1,6503	62,50	1,1614	17,85
1,6407	61,61	1,1531	16,96
1,6312	60,71	1,1398	16,07
1,6217	59,82	1,1309	15,18+
1,6122	58,93	1,1208	14,28
1,6027	58,03	1,1129	13,39
1,5932	57,14	1,1011	12,50
1,5840	56,25	1,0955	11,60
1,5748	55,36+	1,0896	10,71
1,5656	54,46	1,0833	9,80
1,5564	53,57	1,0780	8,93+
1,5473	52,68	1,0725	8,03
1,5385	51,78	1,0666	7,14
1,5292	50,89	1,0610	6,25
1,5202	50,00	1,0555	5,35
1,5112	49,11+	1,0492	4,46
1,5022	48,21	1,0450	3,57
1,4933	47,32	1,0396	2,67
1,4844	46,43	1,0343	1,78
1,4755	45,53		

* The Sp. Gravity was 1,3741 in the former Table.

In *Nitrous Acid* of different Densities, at the Temperature of 60°.

100 Parts Sp. Gravity.	Real Acid	100 Parts Sp. Gravity.	Real Acid.
1,5543	73,54	1,3364	41,91
1,5295	69,86	1,3315	41,18
1,5183	69,12	1,3264	40,44
1,5070	68,39	1,3212	39,71
1,4957	67,65	1,3160	38,97
1,4844	66,92	1,3108	38,34
1,4731	66,18	1,3056	37,50
1,4719	65,45	1,3004	36,77
1,4707	64,71	1,2911	36,03
1,4695	63,98+	1,2812	35,30+
1,4683	63,24	1,2795	34,56
1,4671	62,51	1,2779	33,82
1,4640	61,77	1,2687	33,09
1,4611	61,03	1,2586	32,35
1,4582	60,30	1,2500	31,62
1,4553	59,56	1,2464	30,88
1,4524	58,83	1,2419	30,15
1,4471	58,09	1,2374	29,41
1,4422	57,36	1,2291	28,68
1,4373	56,62	1,2209	27,94
1,4324	55,89	1,2180	27,21+
1,4275	55,15	1,2152	26,47
1,4222	54,12+	1,2033	25,74+
1,4171	53,68	1,2015	25,00
1,4120	52,94	1,1963	24,26
1,4069	52,21	1,1911	23,53
1,4018	51,47	1,1845	22,79
1,3975	50,74	1,1779	22,06
1,3925	50,00	1,1704	21,32
1,3875	49,27	1,1639	20,59
1,3825	48,53	1,1581	19,85
1,3775	47,80	1,1524	19,12
1,3721	47,06	1,1421	18,48
1,3671	46,33	1,1319	17,65+
1,3621	45,59	1,1284	16,91
1,3571	44,86+	1,1241	16,17
1,3521	44,12	1,1165	15,44
1,3468	43,38	1,1111	14,70
1,3417	42,65	1,1040	13.27

The Numbers above the Lines drawn across the Tables of vitriolic and nitrous Acids were found by Experiments; those under the Lines only by Analogy.

The Affinity of vitriolic Acid to Water decreases in the Ratio of the Square of the Quantity of Water united to it. 23 Ann. Chy. 196 and 197.

And so Mr. K. thinks it does to all other Substances; it is the *mean* Affinity that is commonly given.

In *Marine Acid* of diff. Densities at the Temp. of 60°.

100 Parts Sp. Gravity.	Real Acid.
1,196	25,28
1,191	24,76
1,187	24,25
1,183	23,73
1,179	23,22
1,175	22,70
1,171	22,18
1,167	21,67
1,163	21,15
1,159	20,64
1,155	20,12
1,151	19,60
1,147	19,09
1,1414	18,57
1,1396	18,06
1,1358	17,54
1,1320	17,02
1,1282	16,51
1,1244	15,99
1,1206	15,48
1,1168	14,96
1,1120	14,44
1,1078	13,93
1,1036	13,41
1,0984	12,90
1,0942	12,38
1,0910	11,86
1,0868	11,35
1,0826	10,83
1,0784	10,32
1,0742	9,80
1,0630	8,25
1,0345	5,16
1,0169	2,58

Note. The standard Quantities of *Vitriolic* Acid were reduced to *Real* by multiplying them into 0,89293; of the *Nitrous*, by multiplying them into 0,7354; and the *Marine* by multiplying them into 0,516, for the Reasons mentioned in the Author's last Paper.

TABLE II.—*Quantity of Real Acid taken up by mere Alkalies and Earths.*

100 Parts.	Vitriolic.	Nitrous.	Marine.	Fixed air.
Tartarin - - -	82,48	84,96	56,3	105 almost
Soda - - - -	127,68	135,71	73,41	66 8
Vol-alkali - - -	383,8	247,82	171	Variable
Barytes - - -	50	56	31,8	282
Strontian - - -	72,41	85,56	46	43,2
Lime - - -	143	179,5	84,488	81,81
Magnesia - - -	172,64	210	111,35	200 Fourcroy
Argill - - -	150,9			335 nearly, Bergmann

TABLE III.—*Of the Quantity of Alkalies and Earths taken up by* 100 *Parts of Real Vitriolic, Nitrous, Muriatic, and Carbonic Acids, saturated.*

100 Parts.	Tartarin.	Soda.	Vol-alkali.	Barytes.	Strontian.	Lime.	Magnesia.
Vitriolic - -	121,48	78,32	26,05	200	138,	70,	57,92
Nitrous - -	117,7	73,43	40,35	178,12	116,86	55,7	47,64
Muriatic - -	177,6	136,2	58,48	314,46	216,21	118,3	89,8
Carbonic - -	95,1	149,6	— —	354,5	231,+	122,	50,

TABLE IV.—*Quantity of Neutral Salts afforded by* 100 *Parts of the abovenamed Acids when saturated with the abovenamed Bases.*

100 Parts	Tartarin.	Soda.	Vol-Alkali.	Barytes.	Strontian.	Lime.	Magnesia.
Vitriolic	221,48	425 chrystall[d] 178,5 deficc[d]	182,94	300	238	170 in a white heat 198 at 170°	340 crystallized 158 deficcated
Nitrous	227,22	188	175,44			174 well dried, that is, in air	
Marine	277,6	257,2	233,9	487,4 chystall[d] 454,5 deficc[d]	540 crystall[d] 313,5 deficc[d]	238 in a red heat	286,2 well dried
Carbonic	232,5	69,35 crystall[d] 150 deficc[d]		454,5	331,7	222,25	200

TABLE V.—*Quantity of Neutral Salt afforded by* 100 *Parts of different* Bases *when* combined *with the Vitriolic, Nitrous, Marine, or Carbonic Acid.*

100 Parts.	Vitriolic.	Nitrous.	Marine.	Carbonic Acids.
Tartarin	182,48	193 +	156,3	244
Soda	541,1 crystallized 22,74 deficcated	246,42	188,74	463,3 crystallized 167 deficcated
Vol-alkali	702,94	435	400	
Barytes	150		155,16 crystallized 142,8 deficcated	382
Strontian	172,41		250 crystallized 145 deficcated	143,16
Lime	312 dried at 50° 284 dried at 130° 262,8 ignited 243 incandescent	312 dried at 80° 280 fully deficcated	200 in a red heat	182
Magnesia	588,23 crystallized 272,62 deficcated		321,8 gently but sensibly dried	400

Table VI.—*Of the Proportion of Ingredients in the following Saline Compounds.*

100 Parts *Carbonic.*	Basis.	Acid.	Water.	State.
Aerated Tartarin	41, - -	43,	16, - -	Cryftallized
Common Salt of Tartarin or Pearl Ash	60, - -	30,	6 - -	Dry
Aerated Soda	21,58 -	14,42	64, - -	Fully Cryftallized
Do.	59,86 -	40,05	- - -	Deficcated
Aerated Barytes	78, - -	22,	- - -	Natural or ignited
Aerated Strontian	69,5 - -	30,	- - -	Natural or ignited
Aerated Lime	55, - -	45,	- - -	Natural, if pure, or artificial ignited
Aerated Magnesia	25, - -	50,	25, - -	Cryftallized
Common Magnesia	45, - -	34,	21, -	Dried at 80°
Aerated Vol-alkali	In the Ratio of 6 to 13 fixed Air	10	13 fixed Air	
Vitriolic.				
Vitriolated Tartarin	54,8	45,2	- -	Dry
Glauber	18,48 -	23,52	58, - -	Fully cryftallized
Do.	44, - -	56,	- - -	Deficcated at 700°
Vitriolated Vol-alkali	14,24 -	54,66	31,1 - -	
Barofelenite	66,66 -	33,33	- - -	Natural and pure, artificial ignited
Vitriolated Strontian	58, - -	42,	- - -	Natural and pure, artificial ignited
Selenite	32, - -	46,	22, - -	Dried at 66°
Do.	35,23 -	50,39	14,38 - -	Dried at 170°
Do.	38,81 -	55,84	5,35 -	Ignited
Do.	41, - -	59,	- - -	Incandefcent
Epsom	17, - -	29,35	53,65 -	Fully cryftallized
Do.	36,68 -	63,32	- - -	Deficcated
Alum	12, ignited	17,66	51, of Cryftal + 19,24 in the Earth	Cryftallized
Do.	63,75 -	36,25	- - -	Deficcated at 700°
Vitriols.				
Of Iron	28, Calx = 12, tal	26,	38, + 8 of Composition	Cryftallized
Do.	45, -	41,93	13,07 -	Calcined to Redness
Lead	75, Calx = 71, Metal	23,37	1,63 -	
Copper	40, Calx = 30, Metal	31,	29, - -	
Zinc	40, Calx = 30, Metal	20,5	39, - -	

100 Parts *Nitrous.*	Basis.	Acid.	Water.	State.
Nitre	51,8	44, -	4,2 of Compos.	Dried at 7[illegible]
Nitrated Soda	40,58	53,21 -	6,21 of Composi.	Dried at 4[illegible]
Do.	42,34	57,55 -	- - -	Ignited
Nitrated Vol-alkali	23	57, -	20, - -	
Nitrated Barytes	57,	32, -	11, - -	Cryftallized
Nitrated Strontian	36,21	13,07 -	32,72 - -	Cryftallized
Nitrated Lime	32,	57,44 -	10,56 - -	Well dried, that is in Air
Nitrated Magnesia	22,	46, -	22, - -	Cryftallized
Muriated.				
Muriated Tartarin	64,	36, -	- - -	Dried at 80°
Common Salt	53,	47, aqueous, 38,88 real	- - -	Dried at 80°
Sal Ammoniac	- -	- -	- - -	Cryftallized
Do.	25,	42,75 -	32,25 -	Sublimed
Muriated Barytes	64,	20, -	16, - -	Cryftallized
Do.	76,2	23,8 -	- - -	Deficcated
Muriated Strontian	40,	18, -	42, - -	Cryftallized
Do.	69,	31, -	- - -	Deficcated
Muriated Lime	50,	42, -	8, - -	Red hot
Muriated Magnesia	31,07	34,59 -	34,34 - -	Sensibly dry
Muriated Silver	75,	16,54 -	8,46 Oxygen	Dried at 130°
Muriated Lead	81,77 Calx	18,23 -	In the Calx	Cryftallized
Do.	83, Calx	17, -	- - -	Deficcated

(*The use and application of these Tables, with other extracts from this valuable paper, to be given in our next.*)

VIII.

On a ſubmarine Foreſt on the eaſt Coaſt of England. By* JOSEPH CORREA DE SERRA, *LL.D. F.R.S. and A.S.*

IN geology, more perhaps than in any other branch of natural hiſtory, there exiſts a neceſſity of ſtrictly ſeparating the facts obſerved from the ideas, which, in order to explain them, may occur to the mind of the obſerver. In the preſent ſtate of this ſcience, every well aſcertained fact increaſes our ſtill narrow ſtock of real knowledge; when, on the contrary, the reaſonings we are enabled to make are, at beſt, but ingenious gueſſes, which too often bias and miſlead the judgment. I ſhall, therefore, endeavour in this paper, to give, firſt, a mere deſcription of the object unmixed with any ſyſtematical ideas, and ſhall afterwards offer ſuch conjectures on its cauſe as ſeem to me to be fairly grounded on obſervation.

It was a common report in Lincolnſhire, that a large extent of iſlets of moor, ſituated along its coaſt, and viſible only in the loweſt ebbs of the year, was chiefly compoſed of decayed trees. Theſe iſlets are marked in Mitchell's chart of that coaſt, by the name of *clay huts*; and the village of Huttoft, oppoſite to which they principally lie, ſeems to have derived its name from them. In the month of September, 1796, I went to Sutton, on the coaſt of Lincolnſhire, in the company of the right honourable preſident of this ſociety, in order to examine their extent and nature. The 19th of the month, being the firſt day after the equinoctial full moon, when the loweſt ebbs were to be expected, we went in a boat at half-paſt 12 at noon, and ſoon after ſet foot upon one of the largeſt iſlets then appearing. Its expoſed ſurface was about thirty yards long, and twenty-five wide, when the tide was at the loweſt. A great number of ſimilar iſlets were viſible round us, chiefly to the eaſtward and ſouthward; and the fiſhermen, whoſe authority on this point is very competent, ſay that ſimilar moors are to be found along the whole coaſt, from Skegneſs to Grimſby, particularly off Addelthorpe and Mablethorpe. The channels dividing the iſlets were, at the time we ſaw them, wide and of various depths; the iſlets themſelves ranging generally from eaſt to weſt in their largeſt dimenſion.

We viſited them again in the ebbs of the 20th and 21ſt; and though it generally did not ebb ſo far as we expected, we could, notwithſtanding, aſcertain that they conſiſted almoſt entirely of roots, trunks, branches, and leaves of trees, and ſhrubs, intermixed with ſome leaves of aquatic plants. The remains of ſome of theſe trees were ſtill ſtanding on their roots; while the trunks of the greater part lay ſcattered on the ground, in every poſſible direction. The bark of the trees and roots appeared generally as freſh as when they were growing, in that of the branches particularly, of which a great quantity was found; even the thin ſilvery membranes of the outer ſkin were diſcernible. The timber of all kinds, on the contrary, was decompoſed and ſoft in the greateſt part of the trees; in ſome, however,

* Philoſ. Tranſactions, 1799.

it was firm, especially in the roots. The people of the country have often found among them very sound pieces of timber, fit to be employed for several economical purposes.

The sorts of wood which are still distinguishable, are birch, fir, and oak. Other woods evidently exist in these islets, of some of which we found the leaves in the soil; but our present knowledge of the comparative anatomy of timbers is not so far advanced as to afford us the means of pronouncing with confidence respecting their species. In general, the trunks, branches, and roots, of the decayed trees, were considerably flattened; which is a phenomenon observed in the *furtarbrand*, or fossil wood of Iceland, and which Scheuchzer remarked also in the fossil wood found in the neighbourhood of the lake of Thun, in Switzerland.

The soil to which the trees are affixed, and in which they grew, is a soft greasy clay; but for many inches above its surface, the soil is entirely composed of rotten leaves, scarcely distinguishable to the eye; many of which may be separated, by putting the soil in water, and dexterously and patiently using a spatula, or blunt knife. By this method I obtained some perfect leaves of *illex aquifolium*, which are now in the herbarium of the right honourable sir Joseph Banks, and some other leaves, which, though less perfect, seem to belong to some species of willow. In this stratum of rotten leaves, we could also distinguish several roots of *arundo phragmites*.

These islets, according to the most accurate information, extend at least twelve miles in length, and about a mile in breadth, opposite to Sutton shore. The water without them, towards the sea, generally deepens suddenly, so as to form a steep bank. The channels between the several islets, when the islets are dry in the lowest ebbs of the year, are from four to twelve feet deep; their bottoms are clay, or sand, and their direction is generally from east to west.

A well, dug at Sutton by Joshua Searby, shows that a moor of the same nature is found under ground in that part of the country, at the depth of sixteen feet; consequently, very nearly on the same level with that which constitutes the islets. The disposition of the strata was found to be as follows:

Clay, - - - - - - - -	16 feet
Moor, similar to that of the islets, from - - - -	3 to 4 do.
Soft moor, like the scowerings of a ditch bottom, mixed with shells and silt,	20 do.
Marly clay, - - - - - - -	1 do.
Chalky rock, from - - - - - - -	1 to 2 do.
Clay, - - - - - - - -	31 yards.

Gravel and water. The water has a chalybeate taste.

In order to ascertain the course of this subterraneous stratum of decayed vegetables, sir J. Banks directed a boring to be made in the fields belonging to the Royal Society in the parish of Mablethorpe. Moor, of a similar nature to that of Searby's well, and of the islets, was found very nearly on the same level, about four feet thick, and under it a soft clay.

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The whole apppearance of the rotten vegetables we obſerved, perfectly reſembles, according to the remark of ſir J. Banks, the moor which in Blankeney fen, and in other parts of the eaſt fen, in Lincolnſhire, is thrown up in the making of banks; barks, like thoſe of the birch tree, being there alſo abundantly found. This moor extends over all the Lincolnſhire fens, and has been traced as far as Peterborough, more than ſixty miles to the ſouth of Sutton. On the north ſide, the moory iſlets, according to the fiſhermen, extend as far as Grimſby, ſituated on the ſouth ſide of the mouth of the Humber; and it is a remarkable circumſtance, that, in the large tracts of low lands which lie on the ſouth banks of that river, a little above its mouth, there is a ſubterraneous ſtratum of decayed trees and ſhrubs, exactly like thoſe we obſerved at Sutton; particularly at Axholme iſle, a tract of ten miles in length by five in breadth; and at Hatfield Chaſe, which comprehends one hundred and eighty thouſand acres. Dugdale * had long ago made this obſervation in the firſt of theſe places; and de la Pryme †, in the ſecond. The roots are there, likewiſe, ſtanding in the places where they grew; the trunks lie proſtrate. The woods are of the ſame ſpecies as at Sutton. Roots of aquatic plants, and reeds, are likewiſe mixed with them; and they are covered by a ſtratum of ſome yards of ſoil, the thickneſs of which, though not aſcertained with exactneſs by the abovementioned obſervers, we may eaſily conceive to correſpond with that which covers the ſtratum of decayed wood at Sutton, by the circumſtance of the roots being (according to Mr. Richardſon's obſervations ‡) only viſible when the water is low, where a channel was cut, and has left them uncovered.

Little doubt can be entertained of the moory iſlets of Sutton being a part of this extenſive and ſubterraneous ſtratum, which, by ſome inroad of the ſea, has been there ſtripped of its covering of ſoil. The identity of the levels; that of the ſpecies of trees; the roots of theſe affixed in both to the ſoil where they grew; and, above all, the flattened ſhape of the trunks, branches, and roots found in the iſlets (which can only be accounted for by the heavy preſſure of a ſuperinduced ſtratum), are ſufficient reaſons for this opinion.

Such a wide-ſpread aſſemblage of vegetable ruins, lying almoſt in the ſame level, and that level generally under the common mark of low water, muſt naturally ſtrike the obſerver, and give birth to the following queſtions:

1. What is the epoch of this deſtruction?

2. By what agency was it effected?

In anſwer to theſe queſtions, I will venture to ſubmit the following reflections.

The foſſil remains of vegetables, hitherto dug up in ſo many parts of the globe, are, on a cloſe inſpection, found to belong to two different ſtates of our planet. The parts of vegetables and their impreſſions, found in mountains of a cotaceous ſchiſtous, or even ſometimes of a calcareous nature, are chiefly of plants, now exiſting between the tropics, which could neither have grown in the latitudes in which they are dug up, nor have been carried and depo-

* Hiſtory of Embanking and Draining. chap. xxvii. † Philoſ. Tranſ. vol. XXII. p. 980.

‡ Philoſ. Tranſ. vol. XIX. p. 528.

ſited

fited there by any of the acting forces under the prefent conftitution of nature. The formation, indeed, of the very mountains in which they are buried, and the nature and difpofition of the materials which compofe them, are fuch as we cannot account for by any of the actions and re-actions which in the actual ftate of things take place on the furface of the earth. We muft neceffarily recur to that period in the hiftory of our planet, when the furface of the ocean was at leaft fo much above its prefent level, as to cover even the fummits of thefe fecondary mountains, which contain the remains of tropical plants. The changes which thefe vegetables have fuffered in their fubftance, is almoft total. They commonly retain only the external configuration of what they originally were. Such is the ftate in which they have been found in England by Llwyd; in France by Juffieu; in the Netherlands by Burtin; not to mention inftances in more diftant countries. Some of the impreffions, or remains of plants, found in foils of this nature, which were, by more ancient and lefs enlightened oryctologifts, fuppofed to belong to plants actually growing in temperate and cold climates, feem, on accurate inveftigation, to have been parts of exotic vegetables. In fact, whether we fuppofe them to have grown near the fpot where they are found, or to have been carried hither from different parts by the force of an impelling flood, it is equally difficult to conceive how organized beings, which, in order to live, require fuch a vaft difference in temperature and in feafons, could live on the fame fpot, or how their remains could (from climates fo widely diftant) be brought together to the fame place by one common diflocating caufe. To this ancient order of foffil vegetables, belong whatever retains a vegetable fhape, found in or near coal mines; and (to judge from the places where they have been found) the greater part of the agatized woods. But from the fpecies and prefent ftate of the trees, which are the fubject of this memoir, and from the fituation and nature of the foil in which they are found, it feems very clear that they do not belong to the primeval order of vegetable ruins.

The fecond order of foffil vegetables comprehends thofe which are found in the ftrata of clay, or fand; materials which are the refult of flow depofitions of the fea or of rivers; agents ftill at work under the prefent conftitution of our planet. Thefe vegetable remains are found in fuch flat countries, as may be confidered to be of a new formation. Their vegetable organization ftill fubfifts, at leaft in part; and their vegetable fubftance has fuffered a change only in colour, fmell, or confiftence; alterations which are produced by the developement of their oily and bituminous parts, or by their natural progrefs towards rottennefs. Such are the foffil vegetables found in Cornwall by Borlafe; in Effex by Derham; in Yorkfhire by de la Pryme and Richardfon; and in foreign countries by other naturallifts. Thefe vegetables are found at different depths, fome of them much below the prefent level of the fea, but in clayey or fandy ftrata (evidently belonging to modern formation), and have no doubt been carried from their original place, and depofited there by the force of great rivers or currents, as it has been obferved with refpect to the Miffiffippi *. In many inftances,

* La Condreniere fur les Depôts du Miffiffippi. Journ. de Phyf. vol. XXI. p. 230.

however,

however, these trees and shrubs are found standing on their roots, generally in low or marshy places, above, or very little below, the actual level of the sea.

To this last description of fossil vegetables, the decayed trees here described certainly belong. They have not been transported by currents or rivers; but, though standing in their native soil, we cannot suppose the level, in which they are found, to be the same as that in which they grew. It would be impossible for any of these trees or shrubs to vegetate so near the sea, and below the common level of its water; the waves would cover such tracts of land, and hinder any vegetation. We cannot conceive that the surface of the ocean has ever been lower than it now is; on the contrary, we are led, by numberless phenomena, to believe that the level of the waters in our globe is much below what it was in former periods: we must, therefore, conclude, that the forest here described grew in a level high enough to permit its vegetation; and that the force (whatever it was) which destroyed it, lowered the level of the ground where it stood.

There is a force of subsidence (particularly in soft ground), which, being a natural consequence of gravity, slowly though perpetually operating, has its action sometimes quickened and rendered sudden, by extraneous causes; for instance, by earthquakes. The slow effects of this force of subsidence have been accurately remarked in many places; examples also of its sudden action are recorded in almost every history of great earthquakes. The shores of Alexandria, according to Dolomieu's observations, are a foot lower than they were in the time of the Ptolemies. Donati, in his natural history of the Adriatic, has remarked, seemingly with great accuracy, the effects of this subsidence at Venice; at Pola, in Istria; at Lissa, Bua, Zara, and Diclo, on the coast of Dalmatia. In England, Borlase has given, in the Philosophical Transactions *, a curious observation of a subsidence, of at least sixteen feet, in the ground between Sampson and Trescaw islands, in Scilly. The soft and low ground between the towns of Thorne and Gowle, in Yorkshire, a space of many miles, has so much subsided in latter times, that some old men of Thorne affirmed, "That whereas they could before see little of the steeple (of Gowle), they now see the church-yard wall †." The instances of similar subsidence, which might be mentioned, are innumerable.

This force of subsidence, suddenly acting by means of some earthquake, seems to me the most probable cause to which the actual submarine situation of the forest we are speaking of, may be ascribed. It affords a simple, easy explanation of the matter, its probability is supported by numberless instances of similar events, and it is not liable to the strong objections which exist against the hypothesis of the alternate depression and elevation of the level of the ocean; an opinion which, to be credible, requires the support of a great number of proofs; less equivocal than those which have hitherto been urged in its favour, even by the genius of a Lavoisier ‡.

* Vol XLVIII. p. 62. † Gough's edition of Camden's Britannica. t. III. p. 35.

‡ Mem. de l'Acad. de Paris, 1789, p. 351.

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The ſtratum of ſoil, ſixteen feet thick, placed above the decayed trees, ſeems to remove the epoch of their ſinking and deſtruction far beyond the reach of any hiſtorical knowledge. In Cæſar's time, the level of the North Sea appears to have been the ſame as in our days. He mentions the ſeparation of the Wahal branch of the Rhine, and its junction to the Meuſe; noticing the then exiſting diſtance from that junction to the ſea, which agrees according to D'Anville's inquiries * with the actual diſtance. Some of the Roman roads, conſtructed, according to the order of Auguſtus, under Agrippa's adminiſtration, leading to the maritime towns of Belgium, ſtill exiſt and reach the preſent ſhore †. The deſcriptions which Roman authors have left us of the coaſts, ports, and mouths of rivers, on both ſides of the North Sea, agree in general with their preſent ſtate; except in the places ravaged by the inroads of this ſea, more apt from its form to deſtroy the ſurrounding countries, than to increaſe them.

An exact reſemblance exiſts between maritime Flanders, and the oppoſite low coaſt of England, both in point of elevation above the ſea, and of internal ſtructure and arrangement of their ſoils. On both ſides ſtrata of clay, ſilt, and ſand (often mixed with decayed vegetables), are found near the ſurface; and, in both, theſe ſuperior materials cover a very deep ſtratum of bluiſh or dark coloured clay, unmixed with extraneous bodies. On both ſides they are the lowermoſt part of the ſoil, exiſting between the two ridges of high lands ‡ on their reſpective ſides of the ſame narrow ſea. Theſe two countries are certainly coeval, and whatever proves that maritime Flanders has been for many ages out of the ſea, muſt, in my opinion, prove alſo that the foreſt we are ſpeaking of was long before that time deſtroyed and buried under a ſtratum of ſoil. Now it ſeems proved from hiſtorical records, carefully collected by ſeveral learned members of the Bruſſels academy, that no miterial change has happened to the lowermoſt part of maritime Flanders, during the period of the laſt two thouſand years §.

I am, therefore, inclined to ſuppoſe the original cataſtrophe, which buried this foreſt, to be of a very ancient date; but I ſuſpect the inroad of the ſea, which uncovered the decayed trees of the iſlets of Sutton, to be comparatively recent. The ſtate of the leaves and of the timber, and alſo the tradition of the neighbouring people, concur to ſtrengthen this ſuſpicion. Leaves, and other delicate parts of plants, though they may be long preſerved in a ſubterraneous ſituation, cannot remain uninjured, when expoſed to the action of the waves and of the air. The people of the country believe that their pariſh-church once ſtood on the ſpot where the iſlets now are, and was ſubmerged by the inroads of the ſea; that at very low water their

* D'Anville Notice des Gaules, p. 461.

† Nicol. Bergier. Hiſt. des grands Chemins des Romains. Ed. de Bruxelles, vol. II. p. 109.

‡ Theſe ridges of high land, both on the Britiſh and the Belgic ſide, muſt be very ſimilar to each other, ſince they both contain parts of tropical plants in a foſſil ſtate. Cocoa nuts, and fruits of the arecas are found in the Belgic ridge. The petrified fruits of Sheppey, and other impreſſions of tropical plants on this ſide of the water, are well known.

§ Vide ſeveral papers in the Bruſſels *Memoirs*; alſo Journ. Phyſ. t. XXXIV. p. 401.

anceſtors

anceſtors could even diſcover its ruins; that their preſent church was built to ſupply the place of that which the waves waſhed away; and that even their preſent clock belonged to their old church. So many concomitant, though weak teſtimonies, incline me to believe their report, and to ſuppoſe that ſome of the ſtormy inundations of the North Sea, which, in theſe laſt centuries, have waſhed away ſuch large tracts of land on its ſhores, took away a ſoil reſting on clay, and at laſt uncovered the trees which are the ſubject of this paper.

IX.

Optical Remarks, chiefly relating to the Reflexibility of the Rays of Light *. *By* P. PREVOST, *Profeſſor of Philoſophy at Geneva, of the Academy of Berlin, of the Society des Curieux de la Nature, and of the Royal Society of Edinburgh.*

PART I. Concerning reflexibility. *Section* 1. The word reflexibility is uſed in two different ſenſes.

1. Newton † indicates, by this word, that property of a ray of homogeneous light, by virtue of which this ray is reflected, if it fall under a certain angle of incidence; and tranſmitted, if it fall under a certain ſmaller angle: or, more ſimply, a diſpoſition to be reflected, and not tranſmitted at the limit which ſeparates two refracting mediums ‡.

This philoſopher thinks that the reflexibility of the rays of light, taken in this ſenſe, is not the ſame in all the ſeveral kinds. He eſtabliſhes, by experiments, which he concludes to be deciſive, that the moſt refrangible rays are alſo the moſt reflexible. So that, according to him, all other circumſtances being the ſame, if a white ray fall under a certain angle on the directing ſurface, the violet ray will be reflected, while the ſix others will be ſtill tranſmitted and refracted. But by augmenting the angle of incidence, the ſucceſſive reflection of all the rays will be obtained from the violet, which is the moſt reflexible, to the red, which is the leaſt ſo.

Mr. Brougham § does not find the experiments of Newton, by which he eſtabliſhes this propoſition, concluſive: and, upon the foundation of another experiment, he eſtabliſhes the contrary propoſition; namely, that all the rays have the ſame diſpoſition to be reflected, provided the angle of incidence be the ſame.

2. Mr. Brougham underſtands by reflexibility, a diſpoſition in the ray to be reflected nearer to the perpendicular, to a certain degree; or, in other words, a property of the homogeneous ray, by which its angle of reflexion bears a certain ratio to its angle of incidence, which is not the ratio of equality, except in certain caſes, which he points out.

According to this philoſopher, the above ratio varies in each homogeneous ray. The ratio

* Tranſlated from the French original, in the Philoſ. Tranſ. 1798.

† Opt. l. i. part i. prop. 3.

‡ Opt. l. i. part i. defin. 3.

§ Philoſ. Tranſ. 1796. p. 272. or Philoſ. Journal. i. 595.

of

of equality takes place in thofe rays, which occupy the confines of the blue and green: that of inequality takes place with regard to the others, and the moft refrangible one the leaft reflexible. So that with regard to the red ray, the angle of refraction is lefs, and in the violet greater, than the angle of incidence.

It is known that Newton, on the contrary, affirms that the angle of reflexion is, in all cafes, equal to the angle of incidence.

Let us examine thefe oppofite opinions.

Section 2. The firft queftion. Do the homogeneous rays of light differ in reflexibility in the Newtonian fenfe of the term? Or, more directly; does it happen, in fact, that the violet ray will be reflected under the fame angle of incidence at which the red ray is tranfmitted, all other circumftances being precifely the fame?

Of the two experiments by which Newton eftablifhes the unequal reflexibility of the rays, it will be fufficient to mention that which Mr. Brougham attacks directly.

Newton caufed a white ray to fall perpendicularly upon the anterior furface of a prifm; after which, turning the prifm on its axis, he obferved the reflexion which took place, from its pofterior furface. He faw that the violet ray was firft reflected; and afterwards, the other rays in the order of their refrangibilities, the red ray being reflected the laft. Whence he concludes, that the violet ray is reflected under a lefs angle of incidence than the red. Experiment 9.

This is the conclufion which Mr. Brougham attacks; and to avoid altering his thought, I fhall here tranfcribe his expreffions. "That the demonftration involves a logical error, appears pretty evident. When the rays, by refraction through the bafe of the prifm ufed in the experiment, are feparated into their parts, thefe become divergent, the violet and red emerging at very different angles; and thefe were alfo incident on the bafe at different angles, from the refraction of the fide at which they entered: when, therefore, the prifm is moved round on its axis, as defcribed in the propofition, the bafe is neareft the violet, from the pofition of the rays by refraction, and meets it firft: fo that the violet being reflected as foon as it meets the bafe, it is reflected before any of the other rays, not from a different difpofition to be fo, but merely from its different refrangibility."

So that Mr. Brougham thinks that the reflection of the violet ray does not precede that of the red ray; but becaufe the refraction which takes place at the anterior furface, forces the violet ray to arrive at the pofterior furface fooner than the red ray.

But it feems that the effect is here the inverfe of its caufe. Let us firft exclude a falfe fenfe of the words. It is impoffible that the author can mean to fay that the eye is capable of appreciating the interval of time, between the arrival of the violet and the red ray at the pofterior face of the prifm. However, that ray which defcribes the fhorteft courfe, falls neareft the perpendicular let fall from the point of departure; and from this circumftance alone, we can conclude that its angle of incidence is fmaller. Whence it follows, that the red ray ought to be reflected the firft, and not the violet.

Let us, in fact, confider the pofition of the prifm at the firft moment, as it is reprefented in

in the optics of Newton. The white ray F M, plate xvi. fig. 1, is perpendicular to A C; and, in this cafe, it is not refracted, but defcribes a right line. At this point, Newton reprefents the violet ray M N as alone reflected; while all the others, fuch as M H, M I, are tranfmitted and refracted (at leaft the violet alone is totally reflected).

It is, neverthelefs, certain, that in order to obtain this phenomenon, it was requifite to give the proper degree of inclination, by turning the prifm, in order that the experiment might fucceed: and Mr. Brougham has very properly obferved, that the perpendicularity of the ray on the anterior face A C muft then ceafe; confequently, there muft have been refraction, and the feveral homogeneous rays could not have followed a right-lined courfe, fuch as F M, nor have fallen on the pofterior face B C, under equal angles.

Let A′B′C′, fig. 2, be the new pofition of the prifm, which it has taken by virtue of its notation on its axis; the ray F P will then fall obliquely upon A C, at the point P, fo that the perpendicular P O may be on the fide A, and, confequently, comprized in the angle A P F. The refult will be, 1. What Newton propofed, namely, to increafe the angle of incidence on the pofterior furface; which angle (formed at the point M, in fig. 1, which reprefents the firft pofition of the prifm) was before too fmall to produce reflection; and, 2. The precife expreffions of Newton affirm, that the prifm was turned on its axis, according to the direction indicated by the order of the letters A B C, in his figure, which, as far as relates to my object, is the fame as my figure 1.

The ray F P, fig. 2, will, therefore, be refracted, in approaching the perpendicular O P, but the moft refrangible ray (the violet) will approach the neareft, and the leaft refrangible will be the fartheft off. So that the courfes of the rays refpectively, will be well reprefented by the lines P V, P R. The violet ray will, therefore, make, with the pofterior face B C, an angle P V C′, greater than the angle P R C′, formed by the red. Now the angles of incidence at the points V, R, are the complements of the angles P V C, P R C, refpectively.

It is, therefore, certain, that by virtue of the refraction, which is produced at the anterior furface, the violet ray meets the pofterior furface under a lefs angle of incidence than the red; and, confequently, the former is under lefs favourable circumftances for reflection than the latter; yet the former is reflected before the latter. We are, therefore, right in concluding, that it is in its own nature more reflexible, in the Newtonian fenfe of the word *.

This very proper confideration, which was introduced by Mr. Brougham, affords, therefore, a ftill ftronger conclufion in favour of the Newtonian affertion. We may fay, that the violet is not only reflected at the fame incidence at which the red is tranfmitted; but, likewife, that this phenomenon takes place, though the angle of incidence of the violet be more unfavourable to reflection than that of the red.

* This reafoning is equally applicable to the tenth experiment of Newton, in which he ufes two prifms joined together, to form one parallelopipedon. In both the experiments (9th and 10th), another acceffory prifm was ufed, to render the effect more fenfible, by difperfing the reflecting rays. It was unneceffary to attend to this.

In

In a word, therefore, the rays differ in reflexibility in the Newtonian ſenſe, and the moſt refrangible is alſo the moſt reflexible.

Thus far, in order to give ſimplicity to my argument, I have left the refracting angle C, of the priſm, undetermined. Newton determines it in the ninth experiment of the firſt book of his optics, part 1. He uſed a refracting angle of 45°; and, nevertheleſs, he ſays expreſsly, that the rays entered perpendicularly; whence it follows, that the angle of incidence at the point M was alſo 45°. We ſhould, therefore, be juſtified in thinking, that rays falling under this angle of incidence on the ſurface B C, paſſing from glaſs to air, are not totally reflected: Briſſon affirms this to be the caſe, when he explains this ſame experiment in his Elementary Treatiſe of Natural Philoſophy, printed at Paris, in the year 1789. vol. II. § 1411. Nevertheleſs, it is well known that the total reflection takes place at a ſmaller angle, that is to ſay, about 40°. This has been determined, even with the utmoſt preciſion, for which I will quote only a ſingle authority. "A ray of light will not paſs out of glaſs into air, if the angle of incidence exceeds 40° 11′." Adam's Lectures on Natural and Experimental Philoſophy, London, 1794. ii. 163. The determination of Newton does not ſenſibly differ from this: "Totalis reflexio tum incipit, cum angulus incidentiæ ſit 40° 10′." Opt. II. p. 3. prop. 1. Can we affirm that under the angle of 45°, there ſtill paſſes a ſufficient number of rays to render the experiment preceptible? Or muſt we ſuppoſe that the ray F M, was not exactly perpendicular to the face A C? I think the latter is the truth; that is to ſay, that, in the firſt poſition of the priſm, the ray began by being oblique to A C, in the oppoſite ſituation to that indicated in fig. 2. So that the angle A P F was leſs than F P C. Whence it would reſult, that the moſt refrangible rays would fall on the poſterior-ſurface, B C, under a greater angle of incidence, and, therefore, more favourable to reflection. Under this form the argument of Mr. Brougham acquires new force.

The optics of Newton, in this place, requires a commentary. The beſt will be that which his Lectiores Opticæ preſent us—*Iſ. Newtoni Opuſcul. Lauſannæ et Genevæ*, 1744. ii. 217. 223. Here the author replies to our doubts as follows: "Ne qua oriatur ſurpicio, quod refractiones in ſuperficiebus, A C et A B, ad ingreſſum radiorum in priſma et egreſſum facta, poſſint aliquid conducere ad effectus hoſce producendos, obſervare licet quod effectus iidem producuntur, cujuſcunque magnitudinis ſtatuatur angulus A C B *; hoc eſt, quæcunque ſit refractio ſuperficiei A C. . . . . . . Imo poſſis efficere quod cum colores partim reflectuntur . . . . et partim trajiciuntur . . . . . . radii perpendiculariter incidant in A C, emergantque ex A B, et ſic neutra ſuperficie refringantur, modo ſtatuas angulum A C B, eſſe grad. 40 circiter, et iidem tamen effectus producentur †." p. 219. The moſt accurate writers

* It is A B C, in the text; manifeſtly by an error of the preſs. This is, indeed, of no conſequence to my object. I do not quote what relates to the equality required between the angles B and C, becauſe it does not affect the preſent enquiry.

† *Anglice.* Leſt any ſuſpicion ſhould ariſe, that the refractions at the ſurfaces A C, and A B, at the entrance

writers have not overlooked this circumſtance. Robert Smith, after having explained the 9th experiment of Newton's Optics, ſays, or makes Newton ſay, " I did not obſerve any refraction at the ſides A C, A B, of the firſt priſm, becauſe the light was *nearly* perpendicular to the firſt ſides, and emerged *nearly* perpendicular to the ſecond; and, in conſequence, underwent none, or ſo little, that the angles of incidence at the baſe B C were not ſenſibly altered; *particularly if the angles of the priſm at the baſe B C were each about* 40°. For the rays F M begin to be totally reflected when the angle C M F is about 50°; and, conſequently, they will then form an angle of 90° with A C." *Smith's Optics tranſlated into French, by L. P. P. (Pezenas).* I. §. 173. p. 190*.

From all this it follows, that when the angle of a priſm is well choſen, a white ray, falling perpendicularly on its anterior ſurface A C, may be decompoſed; becauſe it is partly, and not totally, reflected, the violet ray being reflected while the red is ſtill tranſmitted.

I muſt here remark, that in order to repeat this experiment, and render it concluſive, it is not neceſſary to circumſcribe the angle C, within the limits which render the ray F M perpendicular (or nearly ſo) to the anterior face A C. All the reaſoning we have offered on this head (§ 2.) will be juſt, provided that, in the firſt poſition of the priſm, the angle A P F be greater than its ſupplement F P C′. But in order that this circumſtance ſhould take place, when the reflection is made at the point M, it is ſufficient that the refracting angle C′ be leſs than 40°.

Intereſting reſults may be obtained, by varying this experiment; but the authorities I have quoted, leave no doubt with regard to the particular reſult, which we have to examine. *The moſt refrangible rays* are reflected at a leſs angle of incidence. They *are more reflexible in the Newtonian ſenſe.*

Section 3. But Mr. Brougham ſupports the contrary opinion, by an experiment which he thus announces: " I held a priſm vertically, and let the ſpectrum of another priſm be reflected by the baſe of the former, ſo that the rays had all the ſame angle of incidence; then turning round the vertical priſm on its axis, when one ſort of rays was tranſmitted or reflected, all were tranſmitted or reflected †."

As the complete diſcuſſion of this experiment would require ſome detail, I ſhall content myſelf with obſerving, that the plane of the vertical face cannot be adjuſted, ſo as to receive

entrance and emergence of the rays, with regard to the priſm, might, in ſome reſpect, aſſiſt in producing theſe effects, it may be obſerved, that the ſame effects are produced, whatever may be the magnitudes of the angle A C B; that is, whatever may be the refraction of the ſurface A C . . . . . It may even be contrived, that, when the colours are partly reflected . . . . and partly tranſmitted . . . . . the rays may fall perpendicularly on A C, and A B, at their entrance and emergence, and they be refracted by neither ſurface, provided the angle A C B be made of about 40°, and yet the ſame effects are produced. N.

* The paſſage here marked as quotation, is retranſlated from the French; and, conſequently, is not given in the very words of Smith, whoſe work I have not. N.

† The ſame experiment, tried by Newton, afforded preciſely the contrary reſult. " Radii purpuriformes primo omnium reflectuntur et ultimo rubriformes." Lect. Opt. Opuſcul. ii. 220.

all

all the rays of the ſpectrum at once under the ſame angle; and even ſuppoſing this to have been poſſible, and done for an inſtant, yet the rotation of the priſm would have changed this diſpoſition, by altering this angle with regard to the ſeveral rays. We may, conſequently, imagine a variety of different reſults; and, among others, we may conceive the angles of incidence of the ſeveral rays to be ſuch, that the obſervation of Mr. Brougham may be reconciled with the opinion of Newton, on their unequal reflexibility. But ſince Mr. Brougham does not enter into this detail, and gives only a ſingle reſult, it is to be preſumed, that he has not repeated, or at leaſt varied, this experiment. This philoſopher even appears, by his rapid enunciation, to have conſidered it of no very great importance.

I think, therefore, that it cannot at preſent weaken the concluſions of Newton; and that we are ſtill juſtified in affirming, in the ſenſe of that philoſopher, that the moſt refrangible rays are alſo the moſt reflexible.

Section 4. SECOND QUESTION. Do the rays of light differ in reflexibility, in the Broughamian ſenſe? In other words, does the red ray at the ſame angle of incidence form a leſs angle of reflection, and the violet a greater, than the angle of incidence?

Section 5. The fundamental experiment from which Mr. Brougham deduces this unequal reflexibility in the ſenſe of his definition, is this:

A bright poliſhed cylinder, of very ſmall diameter (a metallic fibre), having its convex ſurface preſented to a white ray, reflected a coloured ſpectrum; and after proper admeaſurement and computation, it appeared that the rays in the confine of the blue and green, were alone reflected, under an angle equal to that of the incidence. The red rays were reflected under a leſs angle, and the violet under a greater.

The queſtion, therefore, is reduced to determine, whether this experiment be concluſive in favour of Mr. Brougham.

Section 6. To aſcertain this, it is of importance to advert to a principle laid down by Newton, and admitted by Brougham, p. 250. (or Philoſ. Journal, i. 561), namely, that the force, whatever it may be, that produces reflection, acts in the direction of a line, perpendicular to the reflecting ſurface.

Section 7. From this principle it follows, that the reflection operated by a plane ſurface, muſt be made according to the law hitherto admitted by all opticians (Newton's Princip. l. i. p. 96). And this is true, whatever may be the intenſity of the repulſive force, and the inclination of the incident ray; provided the ray be really incident, and do not move parallel to the repulſive ſurface.

Section 8. This conſequence, and the whole of the Newtonian demonſtration, ſuppoſes that the ſurface acts on the ray during its whole courſe through the ſphere of its activity; and equally at equal diſtances. Mr. Brougham alleges nothing againſt this hypotheſis, but even ſeems to admit it expreſsly (p. 269, or Ph. Journal, i. 593); and indeed how can it be denied?

Section 9. Therefore, from a principle which is not conteſted, it appears that reflection cannot decompoſe white light, when it is made totally from a plane ſurface.

This is perfectly conformable to the obſervation of Mr. Brougham, that there is no mean of

of effecting this decompofition by employing plane furfaces, nor curve furfaces, whichhave not a very minute, and, as it were, evanefcent radius. It may, in fact, be conceived, that a fmall portion of a curved furface of a greater radius is a plane with regard to a particle of light. The author indeed, explains this phenomenon in another manner; but the fact, independent of all explanation, is not the lefs certain and acknowledged.

Section 10. Let H H H, fig. 3, now reprefent a very fmall bright polifhed cylinder (a metallic fibre), and B R V K the cylinder on the fame axis, which is the fphere of activity of this fmall body: each of them being reprefented by its circular fection. (Thefe two circles, though very unequal, are confounded together in actual obfervation.)

A B reprefents a white ray, incident at the point B, on the cylinder, on its fphere of activity.

Suppofe the homogeneous rays to be unequally repulfive, and that the red be more ftrongly repelled than the violet: Mr. Brougham admits this fuppofition (p. 267, or Philof. Journal, i. 592).

On this hypothefis, the violet ray muft penetrate farther into the fphere of repulfion.

The courfe defcribed by an homogeneous ray, within the fphere of repulfive activity, muft be formed of two equal and fimilar branches: and its axis muft pafs through the centre of the fphere or fection. This follows from the principle before laid down (§ 6).

And it is an immediate confequence of this remark, that this homogeneous ray will iffue from the fphere of activity, under an angle of reflection equal to the angle of incidence.

So that all the homogeneous rays forming, in B, the fame angle of incidence, will be reflected under equal angles.

But fince fome penetrate farther into the fphere of activity than others, they will come out divergent; for this is the only condition by which the equality of the angles of reflection can be preferved.

Fig. 3 is intended to fhew this effect. The red ray penetrating lefs into the fphere or cylinder of activity B R V K, defcribes the curve B O R, the axis of which paffes through the centre C; and it emerges through R G, making the angle of reflection E R G = A B D, the angle of incidence. The violet ray, penetrating deeper, defcribes the curve B Q V, of which the axis likewife paffes through C. This ray emerges through V L, and the three angles F V L, E R G, A B D, are equal.

But the obferver feeing the arc B R V like a point, and knowing that the angles which he meafures are the fum of the angle of incidence and of reflection for the rays of each kind, will be induced to think, that, under the fame angle of incidence, the angles of reflection vary: for he will find that the right line A B, forms unequal angles with the right lines R G, V L; and, in a word, he will have all the fame appearances which prefented themfelves to Mr. Brougham (§ 5).

It is of importance to remark here, that though this philofopher affirms that the rays on the confine of blue and green are reflected under an angle of reflection equal to the angle of incidence, he did not, nor could not, afcertain this equality by any direct experiment: it was

was simply the most natural supposition he could make. And if it had been pretended, that all the angles of reflection are smaller, or all greater than those of incidence; or that the limit of equality falls on any other division of the spectrum, the supposition would have been gratuitous, but against which the observer could oppose no direct fact. For there is no conceivable means by which the angle of incidence can be measured in these experiments, directly and apart from that of reflection.

Section 11. Since, therefore, by supposing (with Mr. Brougham), that the red ray is more repulsive than the violet, we perfectly reconcile the phenomenon observed with the law of reflection acknowledged to prevail in plane surfaces, there is no reason to depart from it in the present case.

And I conclude, from all that has been said, *that the homogeneous rays are not unequally reflexible* in the Broughamian sense; or, in other terms, that the law of reflection, admitted by Newton, is the true law of nature.

(*To be concluded in our next.*)

X.

On the Manufacture of Beech Oil, by AARON HILL, *and other Projects of Improvement. By a Correspondent.*

SIR,

WE have an old English proverb, which affirms, that it is much easier to ask questions than to answer them. I have lately met with a pamphlet, which has tempted me to propose a number to your correspondents; but have been somewhat intimidated by the above consideration, which might, perhaps, prevent your favouring me with their insertion in your excellent work. It has, however, occurred to me, that a kind of middle way may be devised, to render the business more fair and reasonable.—If the account I now send you should be thought to contain information enough to render it acceptable to your readers, it will be but fair to place it in the balance against the questions I may hope to have resolved.

The pamphlet I allude to, is *Aaron Hill's Account of the Rise and Progress of the Beech Oil Invention* (8vo. 112 pages), which bears date in the year 1715: and, as it appears, was given away. This ingenious man, equally known for his works of imagination, his projects, and the tried rectitude of his principles, passed the latter part of an active life, of sixty-five years, in a state of poverty. His project of making oil from the beech-nut was, therefore, either unsuccessful, or, what is more probable, it failed from circumstances dependant either on himself personally, or those who were engaged with him. The elucidation of this object, which is of no small importance to our woollen and other manufactures, cannot surely be foreign to the purposes of your Journal; and even if any o your correspondents should favour us with historical facts, tending to shew the inutility of the scheme, the public would, no doubt,

 receive

receive it with gratification on the mere inducement of curiofity. The pamphlet before me contains alfo notices of inventions of fome curiofity and apparent value, though related almoft as concifely as the century of inventions of the Marquis of Worcefter: upon the whole, they fhew that when a man of ability leaves the beaten path of the current affairs of life, to purfue the remote analogies of improvement, new refults fpring up around him; to the practical accomplifhment of which the life of man is totally inadequate, and in the purfuit of only a few of which, the utmoft endeavours of an unfortunate individual may be exerted without effect, until at length he finks under the extent and variety of his plans and occupations.

To your difcretion, fir, I fubmit an abridged account of this pamphlet. If from the promife of utility it fhould appear fit for the public eye, I fhall receive pleafure from your decifion; if the contrary, I fhall feel no pain from the free operation of a power which I think you worthy to exercife.

The treatife confifts of a general introduction on the nature and advantages of new inventions, and the prejudices they have to contend with: with an account of the particular invention which forms the chief fubject of the addrefs. The author complains of his fufferings from the mifreprefentations of ignorance and malice, and after relating the ancient ftory of Thales, the Milefian, who enriched himfelf by buying up oil previous to a fcarce year of olives, which he forefaw, he gives an account of the difcovery of the ftocking engine, as follows:

"It is not out of fome mens' remembrance, that a young gentleman of no fortune, a ftudent in Oxford, fell in love with an innkeeper's daughter of that town, whofe circumftances were very narrow. He had philofophy enough to defpife fuperfluous wealth, and judgment to forefee the neceffity of a competence, but love was headftrong, and too hard for reafon; fo that after a year or two's ineffectual delay, they bid defiance to their ftars, and had courage enough to marry. The fcholar gained a wife, and loft a fellowfhip: the only fmall fubfiftance he before depended on."

The narrative proceeds to acquaint us, that the father-in-law, who during his life-time defpifed the unproductive acquifitions of his daughter's hufband, died miferably poor, and left this couple in a ftate of extreme indigence; their whole fupport being derived from the induftry of the wife in knitting ftockings.

In this fituation, rendered more anxious by the certainty of an event which promifed to increafe their family, and to fufpend that labour which afforded their fubfiftence, the hufband could only wafte his hours between his books and his fighs, with ineffectual meditations on fchemes for relief. In thefe reflections, his eyes were often involuntarily fixed on the motion of his wife's fingers at her work. This attention was at laft more fteadily directed, and his imagination fet to work on the enquiry, whether the fame operation could not be performed by machinery. Both parties directed their ingenuity to this refearch, and at length fucceeded in making a ftocking loom, to their own great relief and comfort, and lafting benefit to the public.—*Qu.* Who were the parties here fpoken of?—The author then mentions other fpeculations, and the numerous inventions which diftinguifh cultivated nations; from whence he deduces the evident confequence, that improvements ought to be encouraged.

couraged. He refers to *Stow*, and other authors, for accounts of the calumnies and oppofition which were made to the project of the New-River, and occafioned the ruin of the projector of that noble work. The burning of bricks with pit-coal, which was firft done at the beginning of the feventeenth century; the improvement of glafs, introduced by the Duke of Buckingham; the project of James the Firft, to introduce the growth of filk; the eftablifhment of the woollen manufacture from Flanders; and the manufacture of rape oil, are among the inftances he offers in favour of projects: and to thefe he adds three fpeculations of difcoveries of his own, which are little more than hinted at. Speaking of the firft, he affirms, that there is a common vegetable, almoft the growth of every hedge, which yields a wax, finer than that ufed for candles, and at an expence fo inconfiderable, that he is very fure, that in the practice of the thing, a pound of fuch wax candles would not coft three halfpence. I will not annex a formal query what this vegetable may be: but fimply obferve, that an enquiry into the combuftibility and other properties of vegetable products, with regard to this great object of affording light, appears to deferve the attention of curious men.

Another difcovery, which he fays he has lately beftowed upon an honeft gentleman, is that by the charge of a fingle penny, exclufive of the price of coals, he can produce a gallon of aqua-vitæ, much fitter for all common ufes than the thrice-rectified fpirit of malt. Query: What is the date of the general practice of diftilling fpirit from molaffes? Much earlier, I fuppofe than this period. I am difpofed to think, that the project of Aaron Hill may have confifted in the ufe of fome faccharine product, which was cheap, but not plentiful; and, confequently, could not be procured in fufficient quantity, or fuddenly rofe in price as foon as a demand was felt.

Another object of information is, that acorns, dried and ground, are a much better and cheaper tanning material than oak-bark, and that it is ufed in Italy, and all over the Archipelago, by the name of *velania*. Is this true in point of fact?

The laft project is a fcheme of finance, which is related, or alluded to, in terms of the higheft promife; but as there is no hint of the nature of the plan, and as political arithmetic is a fcience in which miftakes are not only poffible, but very likely to be made, I fhall pafs it over without further remark.

The reft of the book contains an hiftory of the difcovery and practice of the act of making oil from beech-maft. The author firft ftates, that the raw material is very plentiful, and that every bufhel is found to yield two gallons of much better oil than that of olives, called Seville or Gallipoli oil. He informs us, that he made the difcovery at Naples, in 1699; but being at that time only fifteen years of age, he thought no more of it till 1712, when he was induced to enquire into the demand for oil, by the foap makers, clothiers and others, and alfo from the dealers and the entrances at the Cuftom-houfe. He was furprifed at the extent of the confumption, and immediately undertook to afcertain the practicability of procuring the beech-nut in fufficient plenty, as well as the fources from which he might derive the neceffary affiftance for eftablifhing the bufinefs. He met with no encouragement in his guarded applications to men of capital, till after he had taken out a patent, at which period he found

no difficulty in obtaining a ſubſcription of twenty thouſand pounds, which he raiſed by engaging to grant annuities of 50 per cent. on the ſum ſubſcribed. He afterwards raiſed one hundred thouſand pounds, upon an engagement to receive beech-maſt from the ſubſcribers, at 100 per cent profit to them, for two years. With this ſtock, he proceeded to eſtabliſh his works and procure orders; the latter of which, amounted to upwards of two and twenty thouſand tuns of oil, at 40l. a tun, or near a million ſterling. The reports concerning the beech muſt appear to ſhew, that the quantity in plentiful years is prodigiouſly great; but that this happens about every third year. It alſo appears in his foreign correſpondence, not only that the beech-nut is very plentiful on the continent, but that its application to the purpoſe of manufacturing oil, had been known and practiſed for near a century in the northern parts of France. One of his correſpondents points out, that this oil is preferred by many to the beſt olive oil; that it had long before been mentioned with commendation by Evelyn, in his Diſcourſe on Trees; and that in Bretagne, the millers convert the beech-maſt into oil, for the price of one ſous per buſhel, and the oil-cake which is uſed for feeding cattle.

After this maſs of apparently good evidence, I ſhould be glad to know, if it be my ignorance only which leads me to ſuppoſe that there is little, if any, beech oil in the market? If a buſhel of beech-nuts be worth leſs than ſixpence, and will afford, by mere mechanical preſſure, two gallons of oil, equal in quality, or even much worſe than olive oil, how happens it that the conſumer now pays five ſhillings a gallon for whale oil for lamps, and ſixteen ſhillings a gallon for olive oil, in flaſks?

To return to my hiſtory.—It appears on the whole, that Aaron Hill did manufacture this oil, and that the ſamples were highly approved by the Britiſh clothiers, who gave him large orders; that the year immediately ſubſequent to the formation of his ſubſcription company, proved very unfavourable for the beech-maſt, which was lighter and leſs oily than in good ſeaſons; that his ſubſcribers became alarmed after having paid their firſt inſtalment, which was ſubject to forfeiture in caſe of failure in the laſt payment; that the patentee, inſtead of inſiſting upon this bargain, had the honourable ſpirit to offer them a repayment of their money, with a profit of twenty-five per cent.— that this offer was accepted, and actually performed to the whole of the ſecond claſs of ſubſcribers, and one-third part of the original annuitants; the remainder, to the amount of about thirteen thouſand pounds, chooſing to retain their annuities.

Here I muſt remark, that the ſcheme ſeems to have realized ſome conſiderable profit, ſince the advantage allowed to the ſubſcribers over and above their money returned, muſt have been near eight thouſand pounds; beſides which, there were undoubtedly many expences and diſburſements made. Is it to be ſuppoſed, that theſe were all paid out of the thirteen thouſand pounds which were not withdrawn?

The object of the pamphlet was to circulate propoſals for eſtabliſhing a new company, to which the whole patent, ſubject to the annuities, was to be aſſigned for twenty-five thouſand guineas, and one-twentieth part of the profits, under certain general regulations, expreſſed in a deed, inrolled in Chancery for that purpoſe. The ſhares were five thouſand, at forty pounds a-piece: beſides the premium of five guineas on each ſhare. What became of it afterwards, I know not.

I remain, Sir,

Your conſtant reader, X. X.

London, July 6, 1799.

XI.

Letter from Mr. TROUGHTON reſpecting a Balance of his Conſtruction.

To Mr. NICHOLSON.

SIR, Fleet-ſtreet, July 9, 1799.

YOU will not, I apprehend, think yourſelf at all obliged to the perſon who communicated the information expreſſed in your note, page 101, vol. III. of your Journal, when I aſſure you, that the beam which I made for Sir Geo. Shuckburgh Evelyn, had circular rings faſtened inſide the cones; without which neceſſary precaution, it would moſt certainly have been good for nothing. The other difference is moſt trifling; however, I will tell you, that the ſenſibility of his balance is not perceptibly diminiſhed, when the beam deviates three degrees from the horizontal poſition; and that the cryſtal planes were ſet and levelled within half that number of minutes, by other means than thoſe pointed out in your note; and your informant may, perhaps, be able to ſhew the mighty difference between the axis being inclined to the planes, and thoſe planes being inclined to the axis. It is moſt true, that the conical form of this beam is borrowed from that of the Royal Society: but the former being deſigned to weigh twelve times as much as the latter, I ſaw ample cauſe, in other reſpects, to deviate from that model.

After all, thoſe who are acquainted with the ſimple principles of the balance, and eaſe of conſtructing it, will hardly think the maker of either inſtrument in queſtion, entitled to any higher credit than what is due to good workmanſhip: and more eſpecially ſo, was it generally known, that there is an inſtrument in the aſſay-office, Tower of London, and which was uſed there by Sir Iſaac Newton, when he was aſſay-maſter, to which the Royal Society's balance bears full as great an affinity both in form and properties, as Sir George's does to the latter.

I am, ſir, relying on your candour, and thankful for the information and pleaſure which I receive from the conſtant peruſal of your Journal,

Your moſt humble ſervant,

EDWARD TROUGHTON.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

AN important work of Mr. Kirwan is now in the preſs, and nearly ready for publication, under the title of "Geological Eſſays; comprehending, the origin and conſtitution of mountains, ſeas, lakes, coal-mines, beds of ſalt, and of metallic ores, and evincing the accuracy of the details of the formation of the earth and of the deluge, delivered by Moſes, from the laws of nature, and the appearances it exhibits in its preſent ſtate. By Richard Kirwan, Eſq. F.R.S."

Propoſals

Propofals have been circulated by Mr. A. Q. Buée, a French clergyman at Bath, for publifhing, by fubfcription, a work, entitled, *Recherches Mathematiques fur la Texture intime des Corps;* or, Mathematical Enquiries concerning the intimate Texture of Bodies; of which he is the author. It will be printed on fine paper, and illuftrated with fix copperplates. The manufcript is in the hands of the printer, and the work will be put to prefs as foon as one hundred and fifty fubfcribers fhall be obtained at half a guinea each: the price will be greater to non-fubfcribers. Meffrs. Dulau and Co. Cox, White, and Phillips, are authorifed to receive fubfcriptions.

The author of the above treatife has drawn up and printed (in French) an outline of its contents, in twenty-three octavo pages. From the perufal of this, I gather that it is a work of confiderable novelty and importance. Whether any philofopher has before undertaken to folve the phenomena of nature, by the univerfal combination of projectile forces with the attractive power, in the particles of matter, is to me unknown; and it is evidently impoffible for me to fpeak of the manner in which he has treated this curious fubject. I am aware alfo, of the difficulties and probability of miftake attendant on an endeavour to give an outline of an outline. In fact, there muft be a large part of the author's fketch which will be unintelligible, without reference to the treatife itfelf; notwithftanding which confiderations, I am perfuaded that my readers will be pleafed to know fomething more of this object.

The author begins his fketch, by ftating that we are acquainted with two facts concerning the intimate texture of bodies: namely, their cryftallization, which fhews that their elements are difpofed in right lines; and their dilatation by heat, which fhews that thofe elements are not in contact. From the two grand laws of attraction, following the inverfe ratio of the fquares of the diftances, and that of inertia, the mutual action of the elements upon each other may be expreffed by an algebraic equation: this may be called the equation of the material univerfe. The author could not enter upon it in his fketch, and therefore only obferves, that, according to this equation, each element defcribes a line, which if there were but three elements prefent, would be the fame as is well known in phyfical aftronomy in the *problem of the three bodies*, but univerfally is the refult of as many fmall arcs of conic fections as there are other elements.

As fome of the conic fections return into themfelves, and others do not, the elements will be fome planetary, and fome cometary; the latter being diftinguifhed from the former, by a greater initial velocity. But the cometary elements ariving in the vicinity of other elements are difturbed, and made to circulate round a certain number of elements; the planetary elements ofcillate.

Abfolute repofe or equilibrium has, therefore, no place in bodies, except eventually and for minute portions of time; but apparent repofe is produced by the rapidity of ofcillation in the planetary elements, and the conftancy of their greateft and leaft diftances: this apparent repofe implies fymmetrical arrangement, and the great agents of this fymmetry are the cometary elements.

The doctrine of fymmetry is applied to the explanation of chemical facts. Four kinds of aggregation include all the poffible fyftems of elements: 1. Igniform aggregations; containing only

only cometary elements: 2. Aeriform; containing more comets than planets: 3. Liquidiform; in which the planets exceed the comets: 4. Solidiform; containing planets only. These states may not, perhaps, exist purely and distinct from each other in nature. Another fifth state is, that in which no aggregation takes place. The latter cometary elements are the particles of light, of which the colours, the reflection, refraction, diffraction, absorption, double refraction, the Newtonian fits, &c. are explained by analysis; together with those results, in which light is said to enter into combination. The formulæ, which relate to the igneous aggregation, are applied to caloric, electricity, and magnetism. The aeriform aggregation exhibits the phenomena of fluidity, compressibility, hydrostatics, and sound; and under the article of the solidiform aggregations, some observations are made respecting impulse, elasticity, mechanical division, and re-union: and the causes which produce crystallization, vegetation, and animalization. When two distinct bodies approach each other as nearly as possible, without ceasing to be distinct, and by the influence of the elements of the one upon the elements of the other, their centres of gravity acquire a new motion; this operation is called impulse. Mechanical division is the separation of the parts of a body, by contrary impulsions given to those parts: if the direct contrary operation could be performed, mechanical union would take place; this is performed to a certain extent when polished surfaces adhere by application to each other.

With regard to the other doctrines of crystallization, vegetation, and animalization, in which, I presume, there must be some principles assumed as data, which may require farther experiment; it would answer no useful purpose to enumerate the results. For the developement of these, we must wait till the work shall appear.

Death of Galvani.

The celebrated philosopher, Galvani, died lately at Bologna, at the age of fifty-five. His name has been given to the discovery of the influence of the contact of two metals on the animal economy. The circumstance which occasioned this discovery, is not, perhaps, generally known. The wife of Galvani took soup of frogs on account of her health; her husband, who was much attached to her, had skinned several frogs, and on touching them by chance, he unintentionally made a communication, which produced the singular phenomenon, since known by the name of Galvanism. The account to be published by the commissaries of the institute, and the memoir of Baron Humboldt, in the Journal de Physique, for the month of Prarial, an 6, give an ample account of this curious discovery. Dr. Aldini, nephew and co-operator with Galvani, is employed on a continuation of experiments calculated to elucidate this phenomenon still farther.

Lalande, in the Magaz. Encycl. V. 551.

On

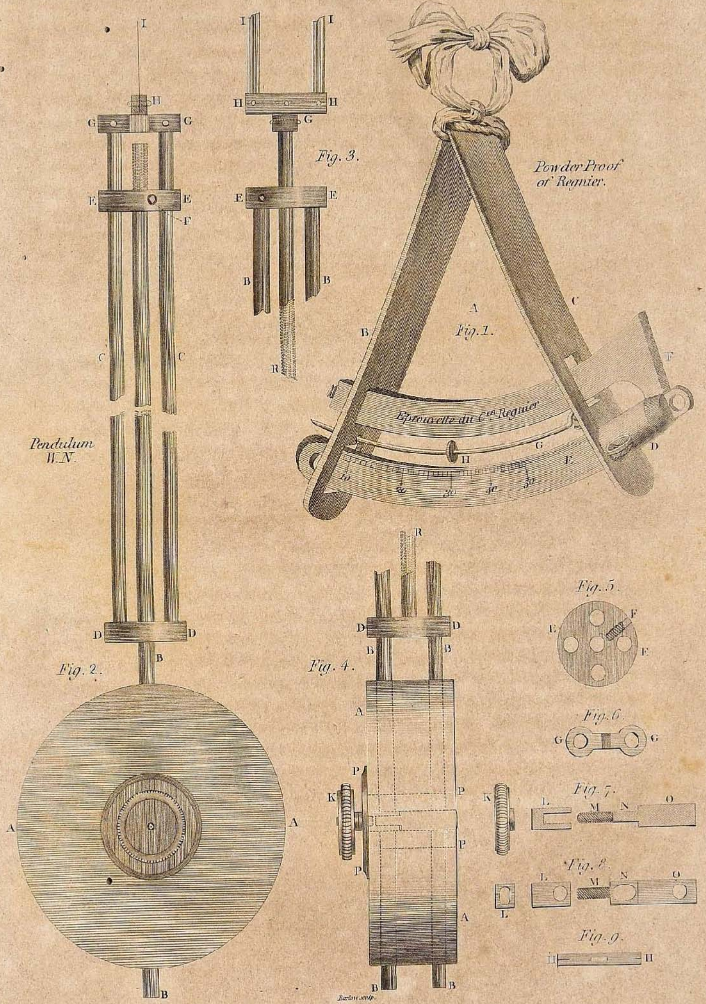
On the Acoustic Experiments of Chladni and Jacquin.

M. E. Perrole, some of whose experiments on sound are inserted in our Journal, I. 411, has written to Citizen Delametherie * a letter, in which he expresses his doubts respecting the truth or accuracy of the experiments of Chladni and Jacquin, announced at p. 43 of our present volume. His arguments are, in short, that Dr. Priestley, who made experiments with a bell under a jar, successively filled with the several gases, as well as himself, did not perceive any such difference of tone. He candidly takes notice, that these last experiments are different in their circumstances from those, in which a flute was used; but does not seem disposed to consider the difference as of much importance. On this head it may be sufficient to remark, that the difference, if theoretically considered, is indeed very great: and that the practical results may most conveniently be established, or refuted, by the test of experiment.

Effect of Hydrogen Gas on the Voice.

Odier, in the Bibliotheque Brittanique †, informs us that Maunoir, at Geneva, amusing himself by respiring pure hydrogene gas, found it attended with no sensible effect, either at the inspiration, or when expired; but when after having inspired a considerable quantity (une forte dose) he attempted to speak, he was strangely surprized that the sound of his voice had become shockingly hoarse and shrill. M. Paul, at whose house this trial was made, repeated it upon himself, and experienced the same effect.

* Journal de Physique, V. 455. † Quoted by Delametherie, J. de Phys. V. 459.


Powder Proof
or Regnier.
Fig. 1.
Epreuvette du Cen. Regnier
A
B
C
D
E
F
G
H
10
20
30
40
50
Fig. 2.
Pendulum
W. N.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
Fig. 9.
Barlow sculp.

Reflexibility of Light.

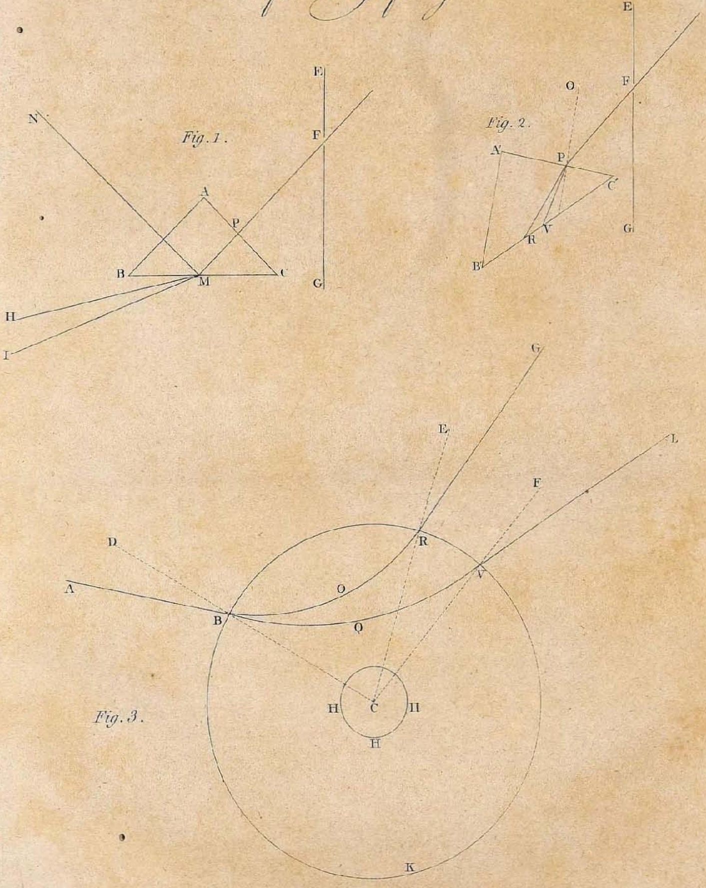


Fig. 1. Fig. 2. Fig. 3.

Barlow sculp.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SEPTEMBER 1799.

ARTICLE I.

A circumſtantial Deſcription of the Method of cultivating the White Beet (Runkelrübe), in order to obtain the greateſt Quantity of Saccharine Matter, and to prepare it for the Manufacture of Sugar. By F. C. ACHARD, Director of the Phyſical Claſs in the Royal Academy of Sciences.*

IN the courſe of ſeveral years paſt I have made experiments to aſcertain how far various native plants might be fit for making ſugar. In theſe enquiries I had the opportunity of obſerving, that the quantity of ſaccharine matter, and its proportion to the other conſtituent parts of the ſame ſpecies of plants, may be increaſed or diminiſhed by the manner of cultivation.

2. Among the various plants which I examined, for the purpoſe of making ſugar in this country, I paid particular attention to the ſeveral ſpecies of *beta vulgaris Linnæi*; one of which, peculiarly proper for manufacturing ſugar, is known to the economiſt in this country by the name of *runkelrübe*†, and to the gardener more eſpecially by the name of *mangoldrübe*. On

* Tranſlated from "Ausführliche Beſchreibung der Methode nach welcher bei der Kultur der Runckelrübe," &c. von F. C. Achard, Director, &c. Berlin, 1799. Octavo, 63 pages.

† As no botaniſt has given the characters of this Runkelrübe, *Beckmann* deſcribes it in the following manner: *Beta altiſſima, floribus ternis vel quaternis, foliolis calycis inermibus, carinatis, caule craſſiſſimo faſciato, radice maxima, rubro et albo intus variegata, foliis maximis rubentibus*.—Note of the tranſlator, from *Nöldecben*'s treatiſe on the culture of this plant.

comparing the ſeveral varieties of this kind of plants, I was convinced, that the particular variety, poſſeſſed of a *long conical root, red rind, and the interior part white*, was the moſt abundant in ſugar, and that the ſaccharine contents of this variety of the *beta vulgaris*, called runkelrübe, may be greatly augmented or diminiſhed, according to the various methods of cultivating it.

3. Having treated this root by various methods of culture, I obtained ſugar from it, with more or leſs profit; in ſome inſtances with loſs, or even no ſugar at all; but frequently a mere extract, in the form of a pulp, ſmelling like turnips; in which, from the exceſs of extractive matter, no ſugar could cryſtallize, unleſs ſome expenſive artificial expedients, not applicable in the large way, had been employed. Theſe obſervations have convinced me of the great influence of cultivation on the ſaccharine contents; and I ſpared no induſtry in order to diſcover the management by which this root might be cultivated of the greateſt richneſs in ſugar. I have not only raiſed them under various ſituations on my eſtate, *French Buckholtz*, but have procured ſuch roots from various other territories, as from *Magdeburgh*, *Halberſtadt*, *Brunſwick*, *Blankenburg*, *Ciecſar*, and *Nauen*, with accounts of the methods by which they were cultivated.

4. I then compared the reſults of the experiments which I had inſtituted for the purpoſe of obtaining ſugar, partly from roots of my own in different circumſtances of growth, and partly from thoſe of other ſoils, which were likewiſe differently cultivated.—By this means I have found that the ſaccharine matter of this root may be conſiderably increaſed, and the extractive matter conſiderably diminiſhed. The conditions are: (a) That it ſhould be cultivated in a rich ſoil, which will be beſt adapted to it, if it be rather compact.

(b) The ſeed is not to be ſown in one bed, and the plants afterwards removed to another, as is commonly done; but, on the contrary, they muſt be ſuffered to ripen on the ſpot in which they germinate from their ſeeds.

(c) The roots muſt not be too diſtant from each other. In the beſt ſoil, their mutual diſtance ſhould be one foot: in a poorer ſoil, ſtill nearer,—nine inches at moſt.

(d) After they have ſprouted, they muſt be cleared of the weeds, either by the hoe or by pulling: taking care, when the hoe is uſed, that the earth be not removed from the plant; but rather, though ſlightly, brought nearer to it. This is not neceſſary, when the weeds are pulled up. It is uſual to remove the earth from the plant, when it is cultivated to ſerve as food for cattle; for its upper part is, by this management, greatly enlarged, and it acquires a greater maſs in the whole: but ſuch management is highly detrimental to the runkelrübe intended for making ſugar.

(e) The leaves muſt not be taken off from the plant, as is the cuſtom, for the purpoſe of feeding cattle. This treatment diminiſhes the ſaccharine matter of the root, at the ſame time that it increaſes its mucilaginous, earthy, and farinaceous parts; and, conſequently, is very injurious to the quality of the root, if appropriated for making ſugar.

5. On theſe five poſitions, which are grounded on repeated experiments and obſervations, I can with juſtice and truth inſiſt; and that, with reſpect to the manufactory of native ſugar, if

carried

carried on with profit from the runkelrübe, every thing depends on its proper culture. For it is by this means only that the increaſe of its ſaccharine contents can be promoted; and it is only from the quantity of this laſt product that ſugar can be made with profit from that root in the large way.

That this root contains ſugar, has been long ſince proved by my celebrated predeceſſor in the Royal Academy of Sciences, the late Director *Margraaf*. But it was then unknown and unſuſpected that it could be obtained from it in the large way, and ſo cheap as 2 *groſhes* (about threepence Engliſh) for the pound of cryſtalline raw ſugar*, and in ſome trials ſtill cheaper, as I have demonſtrated to be practicable, by the experiments made in the preſence of the committee, ſelected for that purpoſe, by the king's command. This reſult is different from all trials hitherto made in this repect, by the moſt able chemiſts. The cauſe is ſimply, that the great influence which the culture of the runkelrübe has, with regard to the increaſe of its ſugar, has not been ſuſpected, and that the different modifications of that culture were unknown; though, in fact, the quantity of ſaccharine matter may, on the one hand, be highly augmented, while, on the other hand, the proportion of thoſe conſtituent parts, which prevent the ſeparation of the ſugar, are greatly diminiſhed.

6. From the method before deſcribed, of producing the runkelrübe abundant in ſugar by means of a proper cultivation, and from my other obſervations on the moſt profitable management of this root, the following inſtructions may be taken for its cultivation:

A ſoil upon which wheat has grown is to be choſen, and kept in good condition. A low ſituation, not expoſed to great or laſting drought, yet without being moiſt or ſwampy, is to be preferred. It is better if manured the year before than recently; which, however, muſt be done, if the former manuring has been omitted. This ground is to be ploughed thrice over, and as deep as the nature of the ſoil will admit. It is alſo very advantageous to perform, if poſſible, the firſt tillage in Autumn. Immediately after the third ploughing, which ſhould be done in the middle of April, or, at lateſt, about the middle of May, the ground is to be ſmoothed by the harrow, as much as poſſible; and by means of a rake, whoſe teeth are diſtant from 9 to 12 inches, lines are to be traced along the ſurface, and by drawing the rake in lines acroſs theſe the ground becomes divided into ſquares, meaſured by the diſtance of the rake's teeth†.

* In this computation the labourer's wages are taken at 8 groſhes per day, and the fuel at the price of the royal Berlin wood-market. Beſides, in my preſent experiments, a circumſtance occurs which greatly enhances the expences of particular operations: namely, that the proceſſes cannot be *concatenated*, or connected with each other. Notwithſtanding this, the pound of raw ſugar will coſt, at the higheſt, only 2 groſhes, and by ſome methods ſtill leſs; becauſe the manufacturer will not pay the day wages at higheſt rate, nor chuſe Berlin, the deareſt place, for eſtabliſhing his manufactory. Thus, alſo, he will not buy his fuel at the greateſt or retail price, but will take all the advantages reſulting from the connection of the ſeveral proceſſes, which do not take place in ſingle trials, as they muſt in carrying on the buſineſs at large.

† As I have not ſpoken with preciſion of this diſtance, but have only given it from 9 to 12 inches, I muſt here remark, that it muſt be regulated by the goodneſs and richneſs of the ſoil. On the beſt ground the diſtance of 1 foot is moſt ſuitable: on a leaner ſoil, leſs manured, it is better to contract it to 9 inches.

7. Into each interfecting point of the lines delineated by the rake, one fingle feed capfule, if you are convinced of its good quality, is to be ftuck in. But if not, then two fuch capfules are to be put in; and, in either cafe, to the depth of an inch. This operation may be done by children, or inferior labourers. When the plants have germinated out of the ground, and fix or eight leaves are formed, the weeds muft be deftroyed or pulled up; but, as I have already obferved, the removal of the earth from the plant is to be very carefully avoided. It anfwers better to pufh the earth nearer to the plant, though this may be neglected without any bad confequence. At this period of the culture there is another operation to be performed. If the plants be too much accumulated on a particular fpot, which is often the cafe when very good feed has been ufed, becaufe one capfule contains feveral feed grains, and produces more plants than one on the fame fpot; in this cafe the fuperabundant plants are to be pulled out.

8. There is no occafion for this operation, if the feed has not been quite frefh, or not quite ripe. But in cafe fome empty places fhould be found, where nothing has grown up, two frefh grains fhould be inferted. After the ground has been once cleared of the weeds, the plants grow up fo fpeedily, that their leaves foon completely cover the ground; and thus abfolutely prevent the growing of any more weeds. In confequence of this, and to the great advantage to the farmer, an acre of ground cultivated with runkelrübe, occafions no more trouble till the time of gathering: which circumftance greatly facilitates their cultivation; becaufe the time of the cultivator, who is then bufied in his corn harveft, is not required to be at all employed on this object. For the gathering of thefe roots begins only towards the end of September, and may be continued to the end of October, if no early froft fets in.

9. At this gathering nothing particularly remarkable occurs, except that the root muft be as little injured as poffible; partly to prevent the lofs of its juice, and partly to prevent the decay to which the wounded parts are more expofed than the found ones. The verdure muft then be cut off, in fuch a manner, that the *heart* may be alfo feparated, in order to prevent the germination of the root. Too much fhould not, however, be lopped off the head; becaufe the juice would, in that cafe, too plentifully exfude. Thefe leaves and hearts are exceedingly valuable to the farmer at this time, when other green food for his cattle is wanting.

10. The roots may be kept for ufe in ditches dug in the earth, where the depth, the drynefs, and the loofe nature of the foil, admits of it. Where this is not practicable, on account of the moifture and firmnefs of the foil, they may be preferved and fecured againft the froft in cellars. But the heart muft not be taken out nor injured in fuch roots as are kept during the winter, in order to obtain feed from them by tranfplantation in the fpring. The leaves are merely to be broken off. The roots muft be well covered during winter, and fheltered againft froft. In general, in the production of the feed from the runkelrübe, the fame method is to be ufed as with other biennial roots and fpecies of cole. As this procedure is known to every economift and gardener, I fhall lofe no time by defcribing it.

11. When large diftricts of ground are to be cultivated with this plant, the feed capfules cannot, for want of time, be fingly put into the ground. The feed muft, therefore, be fown with

with the greateſt poſſible uniformity. The Magdeburg* acre will require from 3 to 4 pounds of ſeed, according to the quality of the ſoil. If the ſowing has been well performed, the plants will be diſtant 9 inches, or a foot at moſt.—In caſe the roots grow nearer to each other, they do not contain leſs ſugar on this account, but they remain ſmall. If too far aſunder, they grow larger, but abound leſs in ſugar. It is, therefore, leſs detrimental to ſow too thick, than too ſparingly.

12. It has before been remarked, that the practice of pulling the leaves from the plant ought to be carefully avoided; but this obſervation relates only to the green vegetating leaves. The under leaves frequently turn yellow and die, and, in theſe circumſtances of decay, they may be taken off, and will afford the farmer ſome aſſiſtance in a ſcarcity of food, without injury to the culture of our root.

13. Reſpecting the choice of the ſeed, beſides its early and perfect ripening, regard muſt be had that it be not obtained from roots which, after their germination, have been tranſplanted on ſeed-beds; but from ſuch as remained on the ſpot where they grew from the capſules, till autumn, and which likewiſe have produced the true oblong, thin, conical roots†. This is neceſſary, becauſe ſuch a ſeed from untranſplanted plants produces roots more partaking of the ſpindle-form; whereas the ſeed from the tranſplanted roots forms thicker, and, at the ſame time, ſhorter, and on the lower parts roundiſhly terminated roots.—The *Art of Gardening* affords numerous inſtances of the effect of this management of ſeed plants, different from thoſe that are to be produced from their ſeed. The ſeeds obtained from untranſplanted lettuce yield, on being ſown, plants which but extremely ſeldom form any heads, and never obtain any firmneſs. The ſeed of a looſe, and not tranſplanted, cabbage, never produces white cabbage, but a looſe cole, not ſhooting into a head. The ſeed of ſellery, if not procured from a plant, which by tranſplantation has been formed into a knob or nodule; but from ſellery which, for want of tranſplantation, has produced rather fibrous roots, yields, on being ſown, only herb, and no nodules. I am convinced of the truth of theſe aſſertions from my own experiments, and appeal to what *Luder* and *Germerſhauſen* have written on this ſubject, as men whoſe ſcience and accuracy will not be diſputed.

14. Among the ſpindle-ſhaped runkelrübes there exiſts a variety as to their colour. Some have a pale red rind, and are internally *quite white*; others with a rind uſually of a more deep red, are internally *ſtriped reddiſh*; others again of a more or leſs deep red have *red circles*; and, laſtly, there are ſome, which, with an almoſt *white rind*, have the internal part yellow.—Thoſe which are white, with a light red rind, deſerve the preference beyond all others. For they yield much ſugar, and an agreeable ſweet ſyrup; which, if well prepared, has no taſte of the root.

* The author has not ſaid whether he means the large or the ſmall Magdeburg or Berlin acre.—The former contains 53771, and the ſecond 24197 French ſquare feet. Probably he underſtands the large, in the common uſe of the language.—Tranſlator.

† It is in *autumn*, as noticed before, that the runkelrübes muſt be gathered, and kept, during the winter, defended from the froſt; becauſe they are biennial plants.

15. The red-ſtriped or circled roots, whoſe rind alſo is always of a darker colour, afford ſugar indeed; but the ſyrup is bad, on account of its taſte of the root, which cannot be removed but by expenſive chemical proceſſes. The runkelrübes of a white rind and yellow internal part do certainly afford moſt ſugar, which ſhoots very readily into large cryſtals; but their ſyrup being of an extremely diſguſting taſte, is of no uſe, when raw ſugar only is made. Even the ſugar itſelf, prepared from theſe roots, is not eaſily, but with difficulty, cleared of that taſte in the condition of raw ſugar, though it certainly diſappears in refining. For this reaſon, this laſt variety of the runkelrübe, that contains ſo much ſugar, is not to be totally rejected, but is rather profitable in the manufactory of ſugar. More eſpecially, if not intended to be employed as raw ſugar, and if the acquiſition of the ſyrup be diſregarded.

16. It is ſufficiently proved, from the phyſiology of plants, that the *matter of light* has a great ſhare in the formation of ſome of their conſtituent parts, as to quality, and conſequently on their mutual proportions. I ſhall here mention one or two inſtances.

While aſparagus is defended againſt the light, it becomes ſweet, and of a pleaſant taſte; but if light has acted upon it, merely for a ſhort time, it loſes its ſweetneſs and turns bitter.

Endive likewiſe has a tough, harſh taſted leaf, provided it be expoſed to the free action of light. On the contrary, when the inner leaves are defended, by tying the outer ones together, they change their colour, which paſſes from green to yellow; the firmneſs of their texture is weakened; they become tender, ſoft, brittle, and full of juice; and their taſte, which was almoſt diſguſting, becomes mild and agreeable. All theſe changes produced in the external appearance and flavour of plants, by the preſence or abſence of the matter of light, can only originate from the *different modifications*, which this matter of light effects either in the nature of their conſtituent parts, or in their mutual proportions.

17. Theſe obſervations, depending on reſults generally known, led me ſeveral years ago to make experiments, in order to diſcover whether the matter of light contributes in general to the *increaſe* or *decreaſe* of any certain conſtituent part of plants, *principally* and *excluſively*? whether this action have the ſame efficacy upon all the parts of plants? or whether the matter of light does *increaſe* or *diminiſh* the ſame conſtituent part of a plant, in its various parts, as *roots*, *leaves*, *fruit*, &c.? To avoid prolixity, I ſhall not deſcribe the ſeveral experiments I have made for the purpoſe of anſwering theſe queſtions, either with plants growing in open air, or cultivated in hot-houſes, and more eſpecially thoſe made with the ſugar-cane in the hot-houſe. I ſhall only mention thoſe reſults which bear any relation to the preſent ſubject, adding ſuch obſervations, or well-known facts, as tend to confirm thoſe reſults.

The following are the reſults of my reſearches:

(a) That the abſence of light augments the ſaccharine liquor in almoſt all roots, or in the germs ariſing from them; that its preſence diminiſhes it; and that to ſhade the whole ſurface of a piece of ground on which ſuch roots are raiſed, adds very much to the increaſe of their ſaccharine matter.

(b) That the matter of light has not the ſame efficacy, but rather one of an oppoſite kind, on the other parts of plants; for inſtance, the fruits, the ſaccharine matter of which is augmented

mented by the presence of the matter of light, and, on the contrary, by its absence the formation of sugar in them not only *retarded*, but in every case *diminished*.

(The conclusion in our next.)

II.

Appendix to Sir GEORGE SHUCKBURG EVELYN'S *Paper* on the Means of ascertaining a Standard of Weight and Measure.*

(§. 44.) SINCE the writing of the preceding Memoir, I have had an opportunity of examining three other scales, divided into inches, or equal parts, of considerable authority in this country, having been executed by the late Mr. J. BIRD. I have also compared the old standard in the Exchequer, of the time of Henry VII. and which is considered to be the most ancient authority of this sort now subsisting: these observations, I flatter myself, the Royal Society will be desirous of possessing.

(§. 45.) The first of the abovementioned scales belonged to the late General ROY, and was purchased by him at Mr. Short's sale, the celebrated optician; it was used by him in his operations of measuring a base line on Hounslow-heath. (See Phil. Trans. vol. LXXV.) It was originally the property of Mr. G. GRAHAM, has the name of JONATHAN SISSON engraven upon it, but is known to have been divided by Mr. BIRD, who then worked with old Mr. SISSON. It is 42 inches long, divided into tenths, with a vernier of 100 at one end, and of 50 at the other, giving the subdivisions of 500ths, and 1000ths, of an inch.

(§. 46.) The second is in the possession of ALEXANDER AUBERT, Esq. and formerly belonged to Mr. HARRIS, of the Tower; contains 60 inches, divided into tenths, with a vernier, like that of the preceding. It is one inch broad, and 0,2 thick.

(§. 47.) The third was presented by Alexander Aubert, Esq. and the late Admiral Campbell, Mr. Bird's executors, to the Royal Society, in whose custody it now remains. It consists of a brass rod, 92,4 inches long, 0,57 inch broad, and 0,3 inch thick; bearing a scale of 90 inches, or equal parts, each subdivided into 10, with a vernier at the commencement, being a scale of 100 divisions to 101 tenths. *This* has been called Mr. Bird's own scale, *viz.* made for his own use; and was the instrument with which he is said to have laid off the divisions of his 8-feet mural quadrants. It is probable that Mr. Bird made many more of these scales, now in the hands of private persons (one of which, indeed, I saw at the President de Saron's, many years ago, at Paris), but those have not come to my knowledge.

(§. 48.) In comparing General Roy's (Bird's) scale with Mr. Troughton's, I found 42 inches of the former were = 42,00010 inches on Troughton's; (the thermometer 51°,7;) 36 inches were consequently = 36,00008.

* Which was concluded at p. 205.

And

		inches.
And 12 inches on the 1ft foot were equal to the 12 inches from 12 to 24 on Troughton's fcale - -	— ,0003 =	11,9997
The 2d foot - - - -	+ ,0006	12,0006
The 3d foot - - - - -	— ,0004	11,9996
The laft foot - - - - -	+ ,0006	12,0006

The mean foot, therefore, in General Roy's fcale, taken from four different feet compared with Troughton's between the 12th and 24th inch, is as 12 to - - - - - - 12,00012

That is, general Roy's fcale is longeft on 1 foot by fo much, and longer on 3 feet by - - - - - - ,00036

And the greateft probable error from the inequality in the divifions is about ,0005

And the mean probable error about - - - ,0003

(§. 49.) Mr. Aubert's fcale, compared with Mr. Troughton's, was as follows: 58 inches were equal to 57,9982 inches on Troughton's; (thermometer at 51°,0;) *viz.* Mr. Bird's meafure was fhorteft ,0018; or, fhorteft on 36 inches = ,0012,

	inches.	
And 12 inches, or 1ft foot, on Mr. Aubert's	= 11,9999	on Mr. Troughton's fcale from 6 inches to 18 inches; the thermometer being at 50°,0.
2d foot, -	= 12,0005	
3d foot, -	11,9996	
4th foot -	12,0019	
5th foot -	12,0006	
Therefore the mean foot is - -	12,0005	

The greateft error in this fcale appears to be about = ,0012

And the mean probable error - = ,0006

(§. 50.) The Royal Society's fcale, compared, was as follows: 58 inches on Mr. Bird's were equal to 57,99912 inches on Mr. Troughton's; (thermometer 50°,5;)

viz. Mr. Bird's meafure was fhorteft - - -	,00088
Or fhorter on 36 inches - - - - -	,00054
32 inches on the fame were equal to - - -	31,99967
viz. Mr. Bird's was fhorteft by - - - -	,00033
Or, on 36 inches, by - - - - -	,00037
The mean of thefe two comparifons is - - -	,00045

And, by fo much, is Mr. Bird's fcale fhorter, in three feet, than Troughton's.

	inches.	
And 12 inches, or 1ft foot, of the Royal Society fcale, is - - -	= 12,00013	on Troughton's fcale; the thermometer at 51°.
2d foot of ditto	= 11,99957	
3d foot of ditto	= 12,00027	
4th foot of ditto	= 11,99990	
5th foot of ditto	= 12,00063	
6th foot of ditto	= 11,99823	
7th foot of ditto	= 12,00000	

The

The mean of these seven feet is - = 11,99982
And the greatest error in these divisions - = ,0008
And the mean probable error - - = ,0004.

(§. 51.) Lest, however, it should be suspected, that Mr. Troughton's scale, with which I have made these comparisons, is not sufficiently correct for this apparent preference, I will now give the result of my examination of that scale, from one end to the other. I set the microscopes to an interval of nearly 6 inches, correctly speaking it was 6,00013 inches, taken from a mean of the whole scale; and, comparing this interval successively, I found as follows:

	inches.	inches.	inches.	Error, or difference from the mean.
viz. from	0 to 6	- -	= 6,00025*	+ ,00012
	6 to 12	- -	= 6,00013	,00000
	12 to 18	- -	= 6,00020	+ ,00007
	18 to 24	- -	= 6,00000	— ,00013
	24 to 30	- -	= 6,00007	— ,00006
	30 to 36	- -	= 6,00033	+ ,00020
	36 to 42	- -	= 5,99980	— ,00033
	42 to 48	- -	= 6,00020	+ ,00007
	48 to 54	- -	= 6,00010	— ,00003
	54 to 60	- -	= 6,00023	+ ,00010
Mean of all		- -	= 6,00013	

From whence it appears that the greatest probable error, without a palpable mistake, in Mr. Troughton's divisions, is = ,00033 inch; against which the chance is 9 to 1; and the mean probable error = ,00016; and that it is 4 to 1 the error doth not exceed $\frac{2}{10000}$ inch.

This accuracy is about three times as great as that of Mr. Bird's scales, and about equal to that of the divisions of my equatorial instrument, made by Ramsden, in 1791. See Phil. Trans. for 1793.

(§. 52.) I now proceed to the examination of the standard rod of Henry VII. which is an octangular brass bar, of about $\frac{1}{2}$ an inch in diameter, with one of the sides rudely divided, into halves, thirds, quarter, eighths, and sixteenths; and the first foot into inches. Each end is sealed with a crowned old English H

and from hence is concluded to be of the time of King Henry VII. *viz.* about 1490, but is now become wholly obsolete, since the

* It is not pretended, that in this and the foregoing observations, the quantity of any interval can be determined to the precision of the one-hundred-thousandth part of an inch; but it *is* presumed, that with the assistance of the microscopes, the ten-thousandth part of an inch becomes visible; and, as a mean is taken from 3 or 4 times reading off the micrometer at each trial, it has been deemed not unreasonable to set down the quantities to five places of decimals.

 introduction

introduction of the ſtandard of Queen Elizabeth; but ſuch as it is, I have thought proper to examine it, and find it as follows:

	inches.	Difference.	Error, of difference of 3 feet.
On this rod, $\frac{1}{3}$ or the 1ſt foot, is equal to	11,973 on Troughton's.		
the 2d foot is -	11,948		
the 3d foot is -	12,047		
The mean foot is -	11,989	— ,011	— ,033
$\frac{1}{2}$ yard, or 18 inches -	= 17,946	— ,054	— ,108
$\frac{2}{3}$ yard, or 24 inches -	= 23,921	— ,079	— ,118
$\frac{3}{4}$ yard, or 27 inches -	= 26,937	— ,063	— ,084
$\frac{7}{8}$ yard, or $31\frac{1}{2}$ inches -	= 31,443	— ,057	— ,065
$\frac{15}{16}$ yard, or $33\frac{3}{4}$ inches -	= 33,665	— ,085	— ,091
Entire yard, or 36 inches -	= 35,966	— ,034	— ,034
And the mean yard - -	= 35,924	Mean	— ,076

And, by ſo much, Mr. Troughton's meaſure is longeſt.

And the probable error, in the diviſions of this old ſtandard, is about $\frac{3}{100}$ inch.

(§. 53.) It may now be deſirable to ſee the comparative lengths of theſe various ſtandards and ſcales, reduced to one and the ſame meaſure, *viz.* Mr. Troughton's.

	Inches on Troughton's.	Difference.	Probable error in diviſions.
36 inches, on a mean, of Henry VII. ſtandard of 1490, are equal to - - - - -	35,924	— ,076	,03
——— of ſtandard yard of Elizabeth, of 1588 -	36,015	+ ,015	,04
——— of ſtandard ell of ditto, of 1588 -	36,016	+ ,016	,04
——— * of yard-bed of Guildhall, about 1660 -	36,032	+ ,032	
——— * of ell-bed of ditto, about 1660 - -	36,014	+ ,014	
——— * of ſtandard of clock-makers' company, 1671	35,972	— ,028	
——— * of the Tower ſtandard, by Mr. Rowley, about 1720 - - - - -	36,004	+ ,004	
——— of Graham's ſtandard, by Siſſon, of 1742, *viz.* line E - - -	= 36,0013	+ ,0013	
——— of ditto, ditto, *viz.* line Exch. -	= 35,9933	— ,0067	
——— of Gen. Roy's (Bird's) ſcale - (all made probably between the years 1745 and 1760.)	= 36,00036	+ ,00036	,0003
——— of Mr. Aubert's, ditto, ditto - (all made probably between the years 1745 and 1760.)	= 35,99880	+ ,00120	,0006
——— of Royal Society's, ditto, ditto - (all made probably between the years 1745 and 1760.)	= 35,99955	+ ,00045	,0004
——— of Mr. Bird's parliamentary ſtandard, of 1758	= 36,00023	+ ,00023	
——— of Mr. Troughton's ſcale, in 1796 -	= 36,00000	,00000	,0001

* Theſe Four quantities are taken from Mr. Graham's account, in the Phil. Tranſ. vol. XLII.

From

From hence it appears, that the mean length of the ſtandard yard, taken from the ſeven firſt inſtances in this table, agrees with the quantity aſſumed by Mr. Bird, or Mr. Troughton, to within $\frac{3}{1000}$ inch, but that the latter is the longeſt.

III.

Obſervations on the Manners, Habits, and Natural Hiſtory of the Elephant. By JOHN CORSE, *Eſq. (Concluded from p. 197.)*

DURING the time they were kept together, the male never ſhewed ſigns of his paſſions being excited, by any exudation from the ducts of the glands near his temples; which is generally conſidered as the ſign of a male elephant being peculiarly ready for the female. This, however, I am inclined to believe is a vulgar error; as not one of the male elephants I have ſeen cover, in a domeſtic ſtate, nor any of the males which were caught ſingly, or rather entrapped, by their deſires to have connexion with the tame females, had, at thoſe times, the ſmalleſt appearance of ſuch an exudation. Had this happened, in any one inſtance, during my reſidence in Tiperah, I think I muſt have known it; for when this exudation takes place, the elephant has a dull heavy look, and it is dangerous for ſtrangers to go near him. I have ſeen elephants in this ſituation after they had been many years caught; but though they were then ſaid to have their paſſions excited, I have never known one to cover during the continuance of this exudation; nor have elephants, ſo far as I have been able to obſerve, any particular ſeaſons of love, like horſes and cattle. Of five inſtances of elephants covered at Tiperah, one received the male in February, another in April, a third in June, a fourth in September, and the fifth in October. Beſides theſe, an attempt was made by a tame male to cover in the month of January a wild female, then in heat*. When the female is in heat, the parts of generation ſhew it, by an unuſual fullneſs of the labia: and, if ſhe is placed near a male, ſhe endeavours, by careſſes, to excite his deſires†.

After the female had been covered by the male, as has been juſt related, there being then no other female ready, he was placed with an elephant which had had a young one about four years before this, and ſome months ago was reported to have been in heat. It was thought, after ſome trial, that ſhe was likely to permit him to cover, as ſhe careſſed him occaſionally, and rouſed his paſſions; but ſhe would not allow him to gratify his deſire.

* Many pregnant females are taken every year at Tiperah, and produce young ones in the different months: this clearly ſhows, that there are no particular ſeaſons during which the females are in heat.

† It may be proper to obſerve, that the penis of a full-grown elephant is from two feet four to two feet ſix inches in length, and from fourteen to ſixteen inches in circumference. I cauſed the penis of two males to be meaſured, after their paſſions were excited, in order to aſcertain the real ſize. On ſome occaſions, I have ſeen the penis abſolutely touch the ground, when the elephant has been walking; but it muſt be recollected, that the hind legs of an elephant are very ſhort, in proportion to his ſize.

 The

The drivers, tired of this coyneſs, and ſtimulated perhaps by the hopes of another gratuity, were ſo brutal as to tie her, and let the male make an atttmpt upon her, while tied. His attempt, however, was to no purpoſe; though he continued his efforts till he appeared to be quite exhauſted. This being told me, I ſeverely reprimanded the people; and ordered the female to be left at full liberty to reject or receive the male, as ſhe might think proper.

Here, however, was poſitive proof, that the male would have effected his purpoſe by force, when he found he could not obtain it any other way. He remained at Comillah till October, 1793, without my being able to procure a female that was in heat; he was then ſent to Calcutta.

I now became extremely ſolicitous about the health of the female which was covered in June; and gave particular directions not to overheat her, but merely to give her as much food and exerciſe as were likely to keep her in the beſt condition, as ſhe was now known to be pregnant. In three months after ſhe was covered, ſhe became fuller, her fleſh felt ſofter, and her breaſts began to ſwell. Theſe marks of her being with young were ſo evident to the driver, that he mentioned them of his own accord; which convinced me, that an elephant, three months after conception, may be known by the keepers to be pregnant.

She had always been a favourite, from having been the gift of my worthy and reſpected friend Mr. John Buller*, as well as from her gentle and docile diſpoſition; and I now had hopes of her going her full time.

She was ſeven feet three inches high, when covered; but after this encreaſed ſo faſt, not in bulk only, but alſo in height, as to exceed ſeven feet eight inches, before ſhe brought forth. On the 16th of March, 1795, ſhe produced a fine male; juſt twenty months and eighteen days after ſhe was firſt covered.

The young one was thirty-five inches and a half high; and had every appearance of having arrived at its full time, being the largeſt I had known produced in Tiperah.

We have many young produced every year, by the females which are taken while breeding, and theſe ſeldom exceed thirty-four inches; this, however, may be owing to the weak and reduced ſtate the mothers are brought to, while breaking in.

The young of the elephant, at leaſt all thoſe I have ſeen, begin to nibble and ſuck the breaſt ſoon after birth; preſſing it with the trunk, which, by natural inſtinct, they know will make the milk flow more readily into the mouth, while ſucking. Elephants never lie down to give their young ones ſuck; and it often happens, when the dam is tall, that ſhe is obliged, for ſome time, to bend her body towards her young, to enable him to reach the nipple with his mouth: conſequently, if ever the trunk was uſed to lay hold of the nipple, it would be at this period, when he is making laborious efforts to reach it with his mouth, but which he could always eaſily do with his trunk, if it anſwered the purpoſe. In ſucking, the young elephant always graſps the nipple (which projects horizontally from the breaſt) with the ſide of his mouth.

* Now one of the Members of the Board of Revenue, at Calcutta.

I have

I have very often obſerved this; and ſo ſenſible are the attendants of it, that, with them, it is a common practice to raiſe a ſmall mound of earth, about ſix or eight inches high, for the young one to ſtand on, and thus ſave the mother the trouble of bending her body every time ſhe gives ſuck, which ſhe cannot readily do when tied to her picket.

Tame elephants are never ſuffered to remain looſe; as inſtances occur of the mother leaving even her young, and eſcaping into the woods.

Another circumſtance deſerves notice: if a wild elephant happens to be ſeparated from her young, for only two days, though giving ſuck, ſhe never afterwards recognizes or acknowledges it. This ſeparation ſometimes happened unavoidably, when they were enticed ſeparately into the outlet of the *keddah*. I have been much mortified at ſuch unnatural conduct in the mother; particularly when it was evident the young elephant knew its dam, and, by its plaintive cries and ſubmiſſive approaches, ſolicited her aſſiſtance.

Here it may be obſerved, that a female was believed to have gone twenty-one months and three days; being ſuppoſed to have been covered on the 13th of January, 1788, ſome days before ſhe was driven into the incloſure. When I made particular inquiry as to the real time ſhe was taken, the ſuperintendant * of the hunters ſaid it was in January; but the principal hunters † declared ſhe was among the herd taken in February following, and was probably the ſame elephant Mr. Buller, Captain Hawkins, and many others, ſaw covered on the 9th and 10th of that month. Perhaps, ſome days prior to this, ſhe might have been covered in the woods, before ſhe was brought into the incloſure; but as a herd was taken in each of thoſe months, and not kept ſeparate, and two years had nearly elapſed before I thought of making any inquiry, it was impoſſible for me to determine in which of thoſe months ſhe was really taken; and the only motive I then had for endeavouring to aſcertain this point, was to form ſome probable conjecture as to the period of an elephant's geſtation, which has now been aſcertained, in the inſtance before related.

Early in September, 1795, the female that had been covered, and had been bred under my own obſervation, was known to be in heat; this was leſs than ſix months after bringing forth. Learning, at the ſame time, that the Rajah of Cudwah, a principal Zemindar of the province, had a very large male that had been in the family near twenty years, from the time he was about five years old, I ſent a meſſenger, requeſting the elephant might be ſent to Comillah, which requeſt the Rajah immediately complied with.

To prevent any interruption from the number of ſpectators, the elephants were put into a ſmall incloſure, on the 17th of September; the female was picketed by one leg, and the young one, to which ſhe was giving ſuck, was tied to a tree at ſome diſtance, fearing if permitted to run about he might receive ſome injury.

After a few careſſes from the female, the male at length effected his purpoſe, and covered her twice the ſame evening. As the intention of the male elephant's viſit was known in the diſtrict, and a few days had elapſed ſince the two elephants were brought together, in

* The *Dârogah*. † The *Dydars*.

order

order to make them acquainted, the number of ſpectators was greater than on any other ſimilar occaſion.

She was afterwards covered, ſeveral times, on the 20th of the ſame month; the male, in this caſe, being admitted after an interval of three days, although formerly, in June, 1793, ſhe refuſed him when only two had elapſed. She again proved with young; and in November, 1796, being myſelf in a bad ſtate of health, and under the neceſſity of returning to Europe, I ſent her to Lucknow, together with her young one, at the requeſt of my friend Captain David Lumsden: though ſhe was then very big, ſhe was ſtill giving ſuck.

About a month before that period, I got my friend, Mr. Stephen Harris, to permit a female of his to be covered; the ſame which had, in 1793, rejected the attempts of the male to cover her contrary to her inclination. Another meſſenger was diſpatched to Cudwah, for the Rajah's elephant, which was again ſent to Comillah. He covered her repeatedly, on the 14th, 15th, and 16th of October, 1796, before many Europeans, as well as natives; and the the laſt time he covered her, it was evidently contrary to her inclination; ſo that, in fact, he uſed force to effect his purpoſe, and held her ſo firmly, that the marks of the nails of his fore-feet were deeply imprinted on her ſhoulders.

Having mentioned a ſufficient number of inſtances, to prove the ability, as well as the inclination of the elephant, to propagate his ſpecies in a domeſtic ſtate, and that without any ſigns of modeſty, and having aſcertained the period of geſtation to be twenty months and eighteen days, it may be neceſſary to obſerve, that it is a difficult matter to bring a male which has been taken about the prime of life, into good condition to act as a ſtallion; for, being naturally bolder, and of a more ungovernable diſpoſition, than the female, he is not in general eaſily tamed, till reduced very low; and it requires conſiderable time, as well as much expence and attention, before he can be brought into ſuch high order as is requiſite. He muſt alſo be of a gentle temper, and diſpoſed to put confidence in his keeper; for he will not readily have connexion with a female, whilſt under the influence of fear or diſtruſt. Of this I have ſeen many inſtances; nor do I recollect one male elephant in ten, which had been taken after having attained his full growth, much diſpoſed to have connexion with a female. This is a moſt convincing proof, that thoſe males which are taken early in life, and have been domeſticated for many years, more readily procreate their ſpecies than elephants taken at a later period. In their wild ſtate, however, they ſhew no reluctance; for, beſides all the males that are entrapped, from their deſire to have connexion with the trained females which, though not in heat, are carried out to ſeduce them, ſeveral inſtances have occurred of wild elephants covering, immediately after being taken, in the *keddah.*

On the third of April, 1795, a very fine male elephant covered a female twice, in the midſt of the herd, and before all the hunters. On the 4th I ſaw him attempting to cover a third time, when he was ſuddenly diſturbed, by the noiſe the hunters made to drive away ſome of the herd which had come too near the paliſade. In conſequence of this interruption, he threw down firſt one and then another ſmall elephant, and gored them terribly with his tuſks, though they came between him and the female only for their protection: he had, before this, killed

killed four, and wounded many others. When the poor animals were thrown down, conſcious of their impending fate, they roared moſt piteouſly; but, notwithſtanding their proſtrate ſituation, and ſubmiſſive cries, he unfeelingly and deliberately drove his tuſks through, and transfixed them to the ground; yet none of the large elephants, not even the dams of the ſufferers, came near to relieve them, or ſeemed to be ſenſibly affected. This ſavage animal had been then confined four days in the incloſure, along with the herd, upon a very ſcanty allowance of food, and could have but very little hope of eſcaping; yet here his paſſions were ſtronger than his fears. It was on account of this ſavage diſpoſition, the hunters had aſked permiſſion to ſhoot him, before I had either ſeen him or the herd, and thence judged he was a *goondah**, that had lately joined. Having never before known any elephant killed wilfully, in the *keddah*, by the larger males, and having no idea that he would commit ſuch terrible havock, I unluckily refuſed to grant their requeſt, being deſirous to ſave ſo ſtately an elephant. When the paliſade was finiſhed, I got him tied, and led out; but, not brooking reſtraint, he languiſhed about forty days, after he was ſecured, and then died.

In the courſe of this narrative, I have, in general, related only ſuch particulars concerning the elephant as came within my own knowledge, and which were either not known, or not publiſhed. To enter into a particular hiſtory of the elephant was not my intention; and, although the procreation of tame elephants has been proved, yet the expence incurred by breeding them, may deter others from making attempts of this kind. But it opens a field of curious inquiry to the naturaliſts; and, now that the facility with which it may be done is aſcertained, it ſuggeſts itſelf as a mode by which the breed of elephants may be improved, in ſize, ſtrength, and activity. In this way, any expence which might be incurred, would more than repay itſelf, in the future benefits to be derived from a ſuperior breed of elephants.

* From this inſtance, as well as many concurring circumſtances, I am convinced that theſe *goondahs* generally leave the herd of their own accord, and join it when they think proper, or are induced to it from a female being in heat; yet it has been ſuppoſed, that they are driven from the herd, at an early period of life, by their ſeniors. This appears improbable, as it is not often that very large males are taken with a herd of elephants; for, depending on their own ſtrength, they ſtray ſingly, or in ſmall parties, from the woods into the plains, and even to the villages; and it is in theſe excurſions they are taken, by means of the trained females. As theſe *goondahs* are much larger, and ſtronger, than the males generally taken with the herd, it is not probable they would ſubmit to be driven from it, unleſs at an early period. I have ſeldom ſeen, in a herd of elephants, a male ſo large as we commonly meet with among two or three *goondahs*; but, if theſe laſt were driven from the herd when young, the very reverſe would be obſerved.

On

IV.

Extract of a Letter on the Art of Bleaching, and the Effect of Friction in Water-courses for Mills. By a Correspondent.

AFTER giving a ſhort ſtatement of Mr. Higgins's proceſs for making and applying the ſulphuret of lime to the bleaching proceſs, of which the entire account is given in the following article, from the work itſelf, ſent by the ſame friend; the writer proceeds as follows:

"The advanced price of alkaline aſhes makes an economical uſe of them indiſpenſable. The above proceſs renders them unneceſſary altogether. How it may ſucceed in the large way is yet to be tried. There is another way to obtain the advantage of reducing the expence of bleaching with alkaline aſhes; that is, to apply to the liquor, when ſaturated with the colouring matter of the cloth, ſome cheap (if any ſuch there is) ſubſtance having a greater affinity for the colouring matter than the alkali. The writer of the article Bleaching, in the ſupplement to the Encyclopœdia Brittanica, No. 5, ſays, 'that if unto the alkaline ſolution 'of the colouring matter of lime or lime water be poured, a copious precipitate falls down, 'which conſiſts of the colouring matter and lime combined; lime, therefore, has a greater 'affinity for the colouring matter than the alkali.'

"This is ſurely an error. I have not been able, by adding lime water to the alkaline ſolution of the colouring matter, to obtain any conſiderable precipitate. The brown colour of the precipitate was not more changed than it would be by being ſo much more diluted; any precipitate which there was, appeared rather to be occaſioned by the carbonic acid diſcompoſing the lime water.

"The writer of the article, if he has ſucceeded as he has ſtated, has made a valuable diſcovery.

"Kirwan, in his experiments on the alkaline ſubſtances uſed in bleaching, &c.* ſays lime diſſolved very little of the colouring matter.

"Mr. Nicholſon's thoughts on this ſubject will gratify a friend to his Journal.

"*On Water Courſes for Mills.*

"Allow a water-wheel 8 feet in breadth; it is required to know what width the canal muſt be to bring on the water to the wheel with full effect; ſuppoſing the length of canal to be 100 yards, and as the friction of the water in the canal is in proportion to its length, what allowance in width muſt be given in any given length to compenſate for the obſtruction ariſing from that cauſe? The form moſt advantageous is taken to be that deſcribed by three ſides of a hexagon.

"*Londonderry.* "A."

* Iriſh Tranſactions for 1789.

It

It would give me much pleaſure if I could ſtate any thing on the ſubject of enquiry reſpecting the method of diſengaging the colouring matter of piece-goods from its combination with alkali. But the affinities of the former muſt be nearly unknown to moſt chemiſts, becauſe, in general, they can have few opportunities of ſubjecting it to experiment. There can be little doubt, however, of its total deſtructibility by heat with acceſs of air. If, therefore, the ſaturated lees were evaporated to dryneſs, and then incinerated, with or without the contact of vegetable fuel, I conclude, of courſe, that the alkali might be profitably recovered. I am aware that the ſkill and reſearches of my correſpondent render it unneceſſary for me to remind him, that there are ſome contrivances for doing this, with little expence of fuel, in the treatiſe of Pajot des Charmes, of which a tranſlation was lately publiſhed.

With regard to the queſtion concerning water-courſes, it cannot be treated in general, without including the quantity of water delivered in a given time as one of the data. I do not know that practical men have paid any conſiderable attention to the loſs which is ſuſtained by the friction of water againſt its channel or bed; but ſhall take a future opportunity of reſuming this ſubject. W. N.

V.

On Sulphuret of Lime, to be uſed as a Subſtitute for Potaſh in Bleaching with the Oxygenated Muriatic Acid. By Mr. William Higgins, *M.R.I.A. and Profeſſor of Chemiſtry and Mineralogy at the Repoſitory of the Dublin Society.**

SINCE I had the honour of being appointed chemiſt to the linen board, which is now more than three years, I have allotted a conſiderable portion of my time and attention to the inveſtigation of the principles of that ſcience, applicable to the art in which I am thus more particularly intereſted. It appeared, that, until potaſh could be diſpenſed with, we muſt for ever remain in the power of foreign nations as to our ſtaple commodity. Obſerving alſo, that all the late improvements in bleaching were excluſively confined to one object—that of imparting oxygen to the cloth, in a ſafe and expeditious manner, but that there had been no effort made to ſuperſede the neceſſity of potaſh, by far the moſt expenſive and uncertain article employed by the bleacher, and for which he is entirely dependent upon foreign markets; I directed my attention chiefly to diſcover a ſubſtitute for potaſh; which, provided it ſhould be of Iriſh production, though it might be equally expenſive, I conceived would be of the utmoſt national importance. Impreſſed with theſe ideas, I undertook a ſeries of experiments with that view.

To enumerate the many diſappointments and failures I experienced during my inveſtigation would be endleſs, and an unneceſſary intruſion upon my reader. Knowing, from

* From his "Eſſay on the Theory and Practice of Bleaching," juſt publiſhed at Dublin, and forwarded to me by the reſpectable writer of the preceding article.

an important obſervation of Mr. Kirwan, that ſaline hepars, or the combination of an alkali with ſulphur, might, from its detergent properties, be advantageouſly employed in bleaching as a ſubſtitute for mere alkali, by an obvious analogy I was led to expect a ſimilar effect from calcareous hepar, or, more properly ſpeaking, ſulphuret of lime, being a combination of lime and ſulphur.

In theſe expectations I was not diſappointed; but at that time (about three years ſince) I contented myſelf (rather through neceſſity; for large cities are very unfavourable to experiments of bleaching, by expoſure to atmoſphere) with pointing it out to ſome of the principal bleachers from the north then in town, earneſtly recommending it to them to give it a fair trial with and without potaſh. Since that time, alkaline ſalts have become progreſſively dearer, and in conſequence of a late propoſal of ſubſtituting lime for potaſh, in condenſing the oxymuriated gas, I was inſtigated to reſume the ſubject, and make further and more varied trials. The reſult of which has been, that the uſe of the ſulphuret of lime may be moſt advantageouſly combined with that of the oxymuriated lime, and that thus cloth may be perfectly whitened without the uſe of a particle of alkali. This, then, alone would ſeem to give it a decided preference over the methods at preſent in uſe, while at the ſame time it poſſeſſes peculiar advantages, and is exempt from the principal objections to which other *ſubſtitutes* are liable; for firſt, quicklime and ſulphur, the materials of which the *calcareous hepar* conſiſts, are both articles of trivial expence, eſpecially as the latter enters but ſparingly into the compoſition; 2dly, their combination is effected in the eaſieſt and moſt expeditious manner poſſible, and perfectly level with the capacity of the meaneſt workman; 3dly, as the manner of its application is by ſteeping the cloth in it cold, the ſaving of fuel is a matter of great magnitude; and laſtly, there is no danger to be apprehended in the uſe of it from the unſkilfulneſs or negligence of the workmen, as it appears to be incapable of injuring the texture of the cloth.

The *ſulphuret of lime* is prepared in the manner following: Sulphur or brimſtone, in fine powder, four pounds; lime, well ſlacked and ſifted, twenty pounds; water, ſixteen gallons: theſe are all to be well mixed and boiled for about half an hour in an iron veſſel, ſtirring them briſkly from time to time. Soon after the agitation of boiling is over, the ſolution of the ſulphuret of lime clears, and may be drawn off free from the inſoluble matter, which is conſiderable, and which reſts upon the bottom of the boiler *. The liquor in this ſtate is pretty nearly of the colour of ſmall-beer, but not quite ſo tranſparent.

Sixteen gallons of freſh water are afterwards to be poured upon the inſoluble dregs in the boiler, in order to ſeparate the whole of the ſulphuret from them. When this clears (being previouſly well agitated), it is alſo to be drawn off, and mixed with the firſt liquor: to theſe again thirty-three gallons more of water may be added, which will reduce the liquor to a proper ſtandard for ſteeping the cloth.

* Although *lime* is one of the conſtituent principles of the *ſulphuret*, yet being ſo intimately united to the ſulphur, it has no longer the property of lime; upon the ſame principle that *ſulphuric acid* in ſulphate of potaſh has not the property of that acid.

Here

Here we have (an allowance being made for evaporation, and for the quantity retained in the dregs) sixty gallons of liquor from four pounds of brimstone.

Although sulphur itself is not in any sensible degree soluble in water, and lime but very sparingly so, water dissolving but about one seven hundredth part of its weight of lime, yet the sulphuret of lime is highly soluble *.

When the linen is freed from the weaver's dressing, it is to be steeped in the solution of sulphuret of lime (prepared as above) for about twelve or eighteen hours, then taken out and very well washed; when dry, it is to be steeped in the oxymuriate of lime for twelve or fourteen hours, and then washed and dried. This process is to be repeated six times; that is, six alternate immersions in each liquor, which I found sufficient to whiten the linen.

When I submitted the linen to six boilings in potash, and to six immersions in the oxygenated liquor, it was not better bleached than the above.

The three first boilings in potash, it is true, produced a somewhat better effect than as many steeps in the sulphuret; but towards the conclusion, that is, when the linen was bleached, the smallest difference was not observable as to colour. The linen bleached with the potash was thinner, or more impoverished, than that treated with sulphuret; and the latter stood the test of boiling with soap much better than the former, although it did acquire a slight yellowish tinge, which I should suppose a week's, or at most a fortnight's, grass, as they term it, would remove.

I contrasted the effects of hot and cold sulphuret in various temperatures, and although the difference appeared in favour of the hot liquor, yet it was so trifling as not to deserve consideration, or the expenditure of the smallest quantity of fuel.

When I steeped the linen in the sulphuret first, and afterwards boiled it in potash, and then immersed it once in the oxygenated liquor, a better effect was produced than from two previous boilings in potash, or from two steeps in the sulphuret; so that the two substances seem to co-operate with each other.

Indeed, from what I have seen, two successive steeps in fresh sulphuret previous to the immersion in the oxygenated liquor, seemed to afford very little better effect than a single one, which is not the case with respect to potash.

It was observable, that the cloth was invariably thicker, or more swelled, coming out of the sulphuret, than after being boiled in potash, and remained so when even washed and dried.

It appears to me that the sulphuret opens the fibres of the linen more speedily and better than the latter, by softening and swelling, rather than by dissolving the resinous or colouring matter. This accounts for the better effect of potash upon the linen when previously steeped in the sulphuret, than when used by itself.

* When the above proportion of lime and sulphur is boiled with only twelve gallons of water, the sulphuret partly crystallizes upon cooling, and when once crystallized, it is not easy of solution.

Probably

Probably thofe bleachers who do not at prefent find it convenient to ufe the oxygenated liquor, but continue to bleach by expofure to air, may derive fome advantage from this, by ufing the fulphuret and potafh conjointly or alternately.

Mr John Duffy of Ball's-bridge (who, from his knowledge of chemiftry, is very well acquainted with the principles of bleaching), was kind enough to repeat the above experiments, and his report to me correfponded with my own obfervations.

It is almoft impoffible to afcertain to the full extent, more efpecially by fmall experiments in an elaboratory, the many advantages any fubftance, not hitherto ufed in bleaching, will afford by varying the mode of application.

The experimenter does a great deal by difcovering the efficacy, proving the practicability, and afcertaining the fafeft and moft economical method of directly ufing it, and alfo the beft proportion of it. Before he can arrive at one of thefe, many a round of changes are neceffary; indeed a greater number than any man, who is not ufed to experiments, can be aware of. But I fhould hope that the bleacher need not hefitate to ufe it in the ftate in which I prefent it to him, more efpecially as he runs no rifque of injuring the cloth with it. If he can make more of it hereafter, I fhall feel happy upon the occafion; no difcovery was ever brought to perfection at once.

How gradually, and yet how progreffively, the fteam-engine, from its firft invention by the Marquis of Worcefter, was brought to its prefent degree of perfection! Undoubtedly it was juft fo with refpect to alkalies, the fubftances now ufed by the bleachers; it muft have taken a confiderable time, after their firft application in bleaching, before they could be made the moft of.

I will now conclude, by pointing out the advantage likely to accrue from the ufe of the fulphuret to the nation, and alfo the faving to the individual.

By the information I have had from the Cuftom-houfe, it appears that the average importation of potafh and barilha the laft twelve years amounts to about 5066 tons annually; about one half of this (2533 tons) is barilha. The average price of barilha, the laft three years, has been 40 l. a ton; fo that the value of the quantity imported is 101,323 l.; of this only half, or thereabout, I underftand, is ufed in bleaching, the remainder being converted into foap.

Moft of the pot and pearl-afh is confumed by the bleachers, and the average price of it the laft three years has been 65 l. a ton, confequently the value of 2,533 tons is 164,645 l.

Hence it feems that the quantity of foreign alkalies imported into the kingdom every year amounts to 265,968 l.; and that the quantity ufed in bleaching alone amounts to about 215,307 l. annually.

The average price of brimftone for the laft three years is about 25 l. a ton, which is at the rate nearly of two pence farthing a pound; four pounds of brimftone, and twenty pounds of lime, as already obferved, will produce fixty gallons of liquor. In this country, twenty pounds of lime may be valued at about four pence, fo that the bleacher may have the fixty gallons at the expence of 1s. 1d.

By

By what I could learn from different bleachers, the common allowance of alkali for ſixty gallons of water is ſix pounds of barilha, or four pounds of potaſh, at the very leaſt, and moſt bleachers uſe more than this. The price of four pounds of potaſh, at the rate of 65 l. a ton, is about 2s. 4d.; which is 2d. more than double the price of the ſulphuret: but as the brimſtone muſt be ground, an allowance ſhould be made for it, and being eaſy of pulverization, a farthing per pound is an ample conſideration for the expence attending it.

The ſaving of fuel only now remains to be taken into conſideration; and as this cannot be calculated with any degree of accuracy, I ſhall content myſelf by particularizing the facts. In the firſt place, but ſixteen gallons of liquid are to be boiled in preparing ſixty gallons of the ſulphuret, while the whole ſixty gallons muſt be boiled when the alkali is uſed; hence it might appear that two thirds of the fuel are ſaved in the quantity of liquor, but it is not quite ſo much; ſuppoſe we eſtimate it at one half, which is rather under-rating it? Let us add to this the time neceſſary to boil the different liquors; the ſulphuret requires but about half an hour, and the alkaline lixivium at the very leaſt ſeven hours, to boil the linen in it, which is in the proportion of one to fourteen.

The ſaving altogether to the bleacher from this ſtatement is obviouſly very conſiderable; and as the Wicklow copper mines are ſufficient to ſupply the whole kingdom, or indeed two ſuch kingdoms, with abundance of ſulphur, let the conſumption be ever ſo great, the entire of the alkali, or 215,307 l. muſt be annually ſaved to the nation.

But ſuppoſe two thirds only of the quantity of alkali generally conſumed in bleaching were diſpenſed with by the uſe of the ſulphuret (which is a ſuppoſition not warranted by any experiment), ſtill the ſaving to the nation, and to the individual, muſt evidently be great indeed.

VI.

On the Decompoſition of the Acid of Borax, or Sedative Salt. By LAURENCE DE CRELL, *M.D. F.R.S. London and Edinburgh, and M.R.I.A. Tranſlated from the German.**

THE ſalt called Borax, ſo uſeful in various manufactures and arts, and hitherto imported only from Thibet and Perſia, or in ſmall quantities from Tranquebar†, has ever excited the attention of natural philoſophers. This attention was principally directed to the acid (called the ſedative ſalt) contained in it; its other component part, the alkaline ſalt (ſoda or natron), being better known, and found in many other natural productions, either alone, or in conjunction with other acids. The acid above mentioned has hitherto been diſcovered only by Hofer in the Lagone of Caſtelnuovo; by Martinovich in the petroleum of Gallicia ‡,

* Phil. Tranſ. 1799. † Demachyms in Laborant in Groſſen, part II. p. 89.
‡ Crell's Annalem. 1791. t. I. p. 162.

mixed

mixed with alkaline earth; and by Mr Weſtrumb near Luneburg. The ſcarcity of this acid, and its being found only in the ſubſtances and ſituations above mentioned, occaſioned a ſuppoſition in the minds of thoſe who minutely obſerve and examine the courſe of nature, that it is not a ſimple ſubſtance, but is formed afreſh from a variety of other ſubſtances previouſly decompoſed by a ſingular coincidence of operative cauſes, and conſequently that it belongs to compounds.

Numerous have been the experiments made by chemiſts who ſuppoſed they had formed this ſalt by compoſition. Some deſcribed experiments, which they declared to have ſucceeded with them, though they always failed when attempted by others *; from which Leonhardi concludes, that nothing more can be expected from any ſimilar attempts to produce ſedative ſalt †.

I was ſurprized that theſe chemiſts had never (ſo far as I knew) examined the ſubject by the way of analyſis, and endeavoured to decompoſe the ſedative ſalt already formed by nature. Indeed no great hopes of ſucceſs could be entertained; as daily experience ſhows, that though this ſalt be kept fluid in the hotteſt fire for many hours together, till it becomes a vitrified ſubſtance, yet when it is afterwards diſſolved in diſtilled water, the ſolution is complete without any reſiduum, and it then ſhoots into cryſtals of preciſely the ſame ſalt as before. Notwithſtanding all this, when I reflected that borax is generated only in certain climates of the eaſt, and that its acid is found only in particular ſubſtances and ſituations, as has been already mentioned, I could not but ſuppoſe the latter to be the produce of a new formation. This being premiſed, I conſidered maturely in what manner the decompoſition of this new and extraordinary compound might be attempted. Admitting the compoſition to be formed by the coalition of a number of different ſubſtances, it ſeemed not improbable but that an acid, penetrating into and diſſolving the whole maſs, would rather aſſociate with ſome than with others of its various component parts, and thus produce a ſeparation or change of the latter. Beſides, as the ſedative ſalt, ſtrong as its operation is (in a high degree of heat) upon almoſt all neutral ſalts, has but a faint taſte of acid, it might be ſuppoſed that its acid is contained within ſome unknown ſpecies of earth intimately combined, or within ſome ſort of inflammable matters; or, according to a phraſe uſed in the new ſyſtem, there might be a deficiency of oxygen; that therefore ſome more powerful acid would probably ſeparate and diſſolve the earthy particles, deſtroy or change the inflammable matter, or impart the oxygen it might be ſuppoſed to want.

My choice among the different acids was fixed upon that particular one, which, though not always quick in its operation, never fails to penetrate deep into all ſoluble ſubſtances, has a ſtrong affinity to all inflammable bodies, and poſſeſſes abundance of oxygen; I mean the oxygenated muriatic acid prepared with mangane ſe. In the application of this menſtruum, I reſolved to follow the practice eſtabliſhed by the conſtant experience of both ancient and

* See Fuchs Geſchichte des Boraxis.

† Macquer's Dictionary tranſlated by Leonhardi, ſecond edition, vol. V. p. 588.

modern

modern chemifts; which has taught us, that difficult decompofitions of parts clofely united, are more eafily effected by a gentle long-continued digeftive heat, and repeated diftillation of the fame menftruum, than by a heat which is more violent, and operates more quickly.

I firft made fome preliminary experiments, in order to judge what probability there might be of fuccefs.

Experiment I. I poured an ounce and a half of the abovementioned acid upon two drams of fedative falt in a retort, to which I adapted a proper receiver, and then placed the mixture in a gentle digeftive heat of from 140° to 200° of Fahrenheit. The fluid was diftilled over very flowly, and the falt was dry on the third day. The falt in the retort feemed unchanged; nor had the marine acid loft any thing of its ufual fmell.

Exper. II. I poured the diftilled fluid out of the receiver upon the fame falt, and expofed them to the fame degree of heat as before. The falt again became dry on the third day, but there was yet no appearance of any change.

Exper. III. I repeated the fame procefs a third time. I now perceived, during the diftillatory digeftion, feveral bright yellow fpots upon the falt as it afcended the fides of the retort, refembling well-formed ammoniacal flowers of iron; more of which I difcovered after the entire exhalation of the fluid.

Exper. IV. The above change induced me to repeat the diftillation; and I then perceived, not only as many, but a much greater number of bright yellow fpots, fome of which were even much darker in colour, and approaching to brown. A change had now evidently taken place, which change encreafed upon every repetition of the procefs; I therefore judged I might follow this indication with confidence. But with a view to ufe the greateft accuracy and precaution in my proceedings and obfervations, I refolved to begin my work over again.

Firft, I procured fome ounces of fedative falt, which had been obtained from borax by means of vitriolic acid, and then prepared two quarts of the abovementioned oxygenated muriatic acid, by diftilling three parts of muriatic acid with one part of the pureft manganefe in the ufual manner; this I preferved in a cool dark place. Thus the fubftances ufed in the following experiments were always of the fame nature.

Exper. V. I poured three ounces of the oxygenated muriatic acid upon half an ounce of the fedative falt in a white glafs tubulated retort. I ufed fuch a retort that (in frequently pouring back the diftilled fluid) I might not have to lute afrefh the feveral veffels after every diftillatory digeftion. For the fame reafon alfo I chofe a tubulated receiver, the tube of which gradually terminated in a point in fhape of a funnel. This tube paffed into a phial placed in fuch a manner, that all the fluid paffing into the receiver dropped immediately into the phial, the joinings of which were clofed with bladder. To clofe the tube of the retort I did not think it right to ufe a waxed cork (though it clofes very tight), becaufe it might be corroded, and alfo becaufe the vapours dropping from the cork might carry fome fat and oily matter back into the retort. For the fame reafon I would not ufe any greafy lute; but clofed the joints of the glafs ftopper (which fitted remarkably clofe) with a ring of fine fealing-wax, clofely preffed upon it, but which could be eafily difengaged after

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my work was done, while the retort was ftill warm: and as I was even afraid of an oily lute about the joints of the receiver, I clofed them up with a ring of very fine white clay, which I fitted to them as exactly as poffible by preffure, letting it ftand feveral days to dry, and then carefully filling up all the cracks. Having made this previous arrangement, and put the abovementioned ingredients together, I fuffered them to remain cold for twenty-four hours; at the end of which the falt was not entirely diffolved, but upon the application of heat the whole became a clear fluid *. The degree of heat in the fand was from 180 to 240, by which the fluid evaporated very flowly. During this operation there afcended, or rather crept up the fides of the retort, a confiderable quantity of falt in very loofe flowers, rifing pretty high above the fluid, increafing by degrees, and chiefly occupying that half of the retort which received a greater degree of heat than the other, but never the oppofite or colder half. In four days, the fire being extinguifhed, towards the evening of the laft the fluid had evaporated fo as to leave the falt apparently dry. After cooling for fome time the bladder upon the phial was moiftened by water, and the veffels were feparated; the fealing-wax alfo having been removed, and the ftopper taken out, the diftilled fluid was poured back through a glafs funnel upon the falt, without difturbing the lute.

Exper. VI. As foon as the fluid was added, the falt at the bottom began by degrees to diffolve; that on the fides of the retort did the fame after it was heated, but foon began to form again: the folution appeared of a yellowifh hue. In general, however, the whole experiment took the fame courfe as in exper. V. and the fmell both of the falt and the fluid feemed to be unchanged. The only difference was, that the former did not appear like falt, the cryftallization on the fides excepted, and in fingle detached cryftals, but fomething like a white uniform fpongy, and, as it were, earthy mafs. The fluid was now again taken from the phial, as in exper. V. and poured back upon the falt.

Exper. VII. VIII. and IX. During the third diftillation, bright yellow fpots began to appear upon the white flowers; and after the falt at the bottom had become dry, fimilar fpots appeared upon it, particularly upon the lower furface. The fluid was again for the fourth time poured upon the falt, and diftilled, when the yellow fpots and flowers increafed in number. This was alfo the cafe in the fifth diftillation.

Exper. X. The fluid obtained by the laft experiment, which had changed a little in fmell and had acquired a particular fcent, almoft as if fome febacic acid had combined with the muriatic, was poured upon the falt as before. The number of yellow fpots, which had alfo become of a darker hue, was confiderably increafed. The falt had now been expofed ever fince the fifth exper. for thirty-two days, to the digeftive diftillation, and the intermediate time between each diftillation had been longer or fhorter in proportion to the degree of heat

* This appeared to me fo ftriking, that I endeavoured to obtain a confirmation of it. I made a fimilar mixture in the fame proportions, which was not diffolved fo long as it remained cold, but was diffolved by heat. When the folution cooled, a fmall part of the falt (and a larger as the cold increafed) precipitated, which was diffolved again by a frefh application of heat. But with the degree of heat I employed, no more than one part of falt would diffolve in fix parts of acid.

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and to the time of kindling and extinguifhing the fire. I now found that bufinefs of importance would prevent me from continuing my labours for fome months; I poured two other ounces of the muriatic acid upon the falt, befides the fluid fo often drawn off by diftillation, and left the mixture at reft.

Exper. XI. XII. XIII. XIV. When my bufinefs was finifhed, I again undertook the diftilling of the mixture, which had been fo long digefting in the cold, for the feventh time, and obtained the fame refults as in *Exper.* X. Nor was there much difference obferved in the XII. XIII. and XIV. experiments.

Exper. XV. I now poured the fluid obtained by the XIV. experiment upon the falt (which had acquired here and there yellow fpots brighter in hue), and then proceeded, as before, till the falt became dry; upon which, when the retort was cool, I poured one ounce and three drams of the muriatic acid in addition, and allowed the mixture to digeft gently for fome days.

Exper. XVI. In this twelfth diftillation there appeared a large quantity of flocculent fublimate, looking almoft like branches hanging down, and in many places of a yellow colour; it extended even into the neck of the retort, and almoft covered the interior aperture of the tube.

Exper. XVII. The thirteenth diftillation produced the fame phenomena. Upon the lowermoft furface of the mafs of falt, many light-brown fpots appeared as foon as the fluid was fo much evaporated that no more of it could be feen upon the falt.

From all thefe circumftances I now believed the mafs of falt, by a digeftion of twenty-two days, and feven diftillations, from experiment XI. to XVII. (that is, by a digeftion of fifty-four days, and thirteen diftillations, in the whole), to be fo far decompofed as to admit of a feparation of fome of its conftituent parts. I therefore fuppofed I might leave off applying only a digeftive warmth, and proceed to a greater degree of heat.

Exper. XVIII. Having poured out the fluid obtained by experiment XVII. and replaced the phial, I increafed the degree of heat. By this the retort became quite obfcured, firft by fumes, and afterwards by a quantity of white fublimate, attaching itfelf to all its fides, which however had not the appearance of common fedative falt. As I increafed the heat, the fublimate grew darker in colour, afterwards became black and frothy, and at length ran down the fides of the retort, being almoft wholly blackened by it.

Exper. XIX. While the retort was ftill warm, I poured into it the fluid obtained by experiment XVII. having firft watered it a little; when almoft in the fame inftant a very agreeable phenomenon took place: cryftals perfectly white fhot forth fuddenly, and all at once, from every fide of the black mafs, covering the fides of the retort. The diftillation being continued, thefe cryftals were at length diffolved and entirely removed. The fupernatant fluid was as ufual almoft colourlefs. When the mafs of falt appeared dry, the fire was increafed, as in experiment XVIII. and the fame appearances, as above related, took place; firft, the fublimate appeared white; then black frothy, and flowing down the fides.

Exper. XX. I proceeded as in experiment XIX. to pour back the diftilled fluid. Inftantly a

number

number of the whiteſt cryſtals ſhot forth from the black ground, forming ſmall groups; but the retort was cracked.

Exper. XXI. I therefore took all the veſſels aſunder, and ſhook the retort well, till whatever hung upon its ſides was diſſolved; then diſtilled the fluid in another retort, till the maſs of ſalt appeared quite dry. I now put the retort into a crucible, ſurrounded it with ſand, fitted another receiver to it, and placed the crucible in an open fire. Firſt ſome ſublimate was produced towards the neck of the retort, (but which vaniſhed as the heat increaſed), and then a ſmall portion of fluid (hardly more than a dram, or a dram and a half), which appeared to ſmell a little of the ſebacic acid. At the bottom of the retort was a blackiſh maſs *a*, and likewiſe ſome ſublimate *b*, which by its varied appearance ſeemed to be of a two-fold nature.

Exper. XXII. The reſiduum taken out of the broken retort had a ſpongy appearance, and ſwam upon water. It had a blackiſh colour, and weighed three drams and ten grains. Being expoſed to the air, the blackiſh colour became lighter, and inclining to grey. When digeſted in ſixteen parts of diſtilled water in the uſual temperature for two days and a half, it did not at all ſink to the bottom; and after being digeſted with heat for twenty hours, it was not entirely diſſolved: that part which ſunk was of a blackiſh-brown. More water was then added, and it was made to boil for two hours; it was afterwards placed upon a paper filter (the weight of which was previouſly aſcertained), and edulcorated with boiling diſtilled water, till at laſt a proportion of twenty-ſix parts of water to the ſubſtance had been uſed. After all the fluid α had paſſed through, and the filter with the reſiduum had been dried in a heat of 212° for an hour and a half, the reſiduum β weighed, excluſive of the filter, nineteen grains.

Exper. XXIII. The fluid α obtained by exper. XXI. was ſuffered to evaporate gradually, and yielded three drams and ten grains of a white tranſparent ſalt.

Exper. XXIV. This ſalt (obtained by exper. XXIII.) was put into a ſmall retort, and expoſed in a crucible filled with ſand to an open fire. It became of a blackiſh brown colour, yielded ſome ſublimate *a* (about five grains), a ſmall portion of fluid *b*, and a blackiſh brown reſiduum *c*, which grew lighter in colour on being expoſed to the air.

Exper. XXV. The fluid *b* (of exper. XXIV.) ſmelt like marine acid, and precipitated nitrate of lead.

Exper. XXVI. The reſiduum *c* (of exper. XXIV.), by the addition of ſome water, became whiter, and was diſſolved; more water having been added, it was digeſted with heat, by which the matter was diſſolved. The ſolution being afterwards filtered, I obtained two drams and four grains of white ſalt: the reſiduum upon the filter weighed four grains.

Exper. XXVII. This ſalt (exper. XXVI.) I again expoſed to the fire, when it yielded from twenty to thirty drops of acid liquor, four grains of ſublimate, and a reſiduum, which being diſſolved, yielded one dram and thirty-three grains of ſalt, and left two grains and a half *c* upon the filter.

The ſame ſalt (obtained by exper. XXVI.) being diſtilled, became of a browniſh-grey colour, and beſides a few drops of fluid yielded not quite two grains of ſublimate. On heat-

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ing the residuum with water, it yielded sixty-eight grains of salt, and there were not quite two grains left upon the filter.

Exper. XXVIII. On treating these sixty-eight grains of salt in the same manner, they yielded a few drops of fluid, and two grains of sublimate; after filtration, there remained forty-eight grains of salt, and a residuum of hardly one grain and a half.

Exper. XXIX. The same salt, treated in the same manner, yielded a few drops and a little sublimate; and after filtration thirty-five grains of salt and a residuum of hardly one grain.

Exper. XXX. On treating these thirty-five grains of salt in the same way, they yielded, besides a very small quantity of fluid and of sublimate, twenty-four grains of salt, and about three quarters of a grain of residuum.

As I now discovered that the quantity of salt was continually decreasing, and some coal separating from it, I thought it superfluous to endeavour to decompose the above twenty-four grains any farther.

Exper. XXXI. The residuum β of exper. XXII. was light, blackish, and like coal. I now poured common concentrated muriatic acid upon three grains of it, and digested the mixture for forty-two hours in a considerable degree of heat, but no dissolution was apparent. I then added smoking nitrous acid, and digested it for twenty-four hours, till it boiled without any apparent dissolution. I added some sugar (about two grains), but without effect, except that its colour grew yellowish. I now boiled the fluid till it all evaporated in reddish-yellow vapours; there remained a very black, thick, glutinous mass, smelling like burnt sugar. Having added three ounces of water, the greatest part of the blackish matter rose to the surface, and the water appeared only a little tinged: the fluid part, indeed, became brown by boiling, but after rest and subsidence, it again grew clear. I filtered it *a*; then poured two ounces more distilled water upon the residuum, and after digesting, boiling, and filtering, added the filtered fluid *b* to the former *a*. After this treatment there remained two grains of residuum *c*.

Exper. XXXII. Having caused the fluid *a b* of exper. XXXI. to evaporate, it yielded a salt greyish yellow mass, which very quickly attracted the moisture of the air. Being again dissolved in water, and saturated with potash, a considerable quantity of whitish earth was precipitated very much resembling talc.

Exper. XXXIII. The residuum *c* of exper. XXXI. which, besides its insolubility and lightness, had much of the external appearance of coal, was now thrown upon melted nitre, and it deflagrated. I placed a second crucible with melted nitre close to it, and after having at the same moment thrown into one the abovementioned residuum, and into the other a quantity of common charcoal pulverised, I could not observe the smallest difference in effect. Very little difference was also apparent as to the residuum β of exper. XXII. *c* of exper. XXIV. and that of the following experiments.

(To be continued.)

VII.

VII.

Reflections on the Qualities of Pottery, with the Result of some Analysis of Earths, and of Common Pottery. By CIT. VAUQUELIN.*

THE quality of pottery may be influenced by four things; 1. The nature or composition of the material; 2d, The preparation; 3d, The dimensions of the vessels; and, 4th, The heat to which it is subjected in baking.

By the composition of the material, the author understands the nature and proportions of its elementary parts; and these elements in most potteries, whether valuable or common, are silex, alumine, lime, and sometimes a small quantity of the oxide of iron. Hence it is evident that good pottery differs from bad, less in the diversity of its elements, than in their proportions.

Silex, or quartz, constantly forms at least two thirds of most potteries; alumine from a fifth to one third; lime from five hundredth parts to twenty hundredth parts; and iron from the minutest quantity, or none, to twelve or fifteen per cent.

Silex gives hardness, infusibility, and unchangeableness; alumine communicates tenacity, and ductility to the paste, so that it may be kneaded, moulded, and turned at pleasure. It likewise undergoes a commencement of fusion, by the heat which connects its parts with those of the silex; but its quantity must not be too abundant, because it would render the pottery too infusible, and disposed to break by heat.

Experiment has not yet shown that lime is a necessary ingredient in pottery; the traces which are constantly found of its existence in this manufacture arise from its forming part of the ingredients from which it has not been washed or properly separated. When this earth does not exceed five or six per cent, it appears at least to do no harm to the quality of the pottery; but when more abundant, it communicates too great a degree of fusibility.

The oxide of iron possesses the inconvenience of affording a red or brown colour according to the degree of heat, and has besides the property of being very fusible; more so even than lime.

Among the various kinds of pottery, some of them being applied to contain very penetrating substances, such as salts, metallic oxides, glasses, &c. in a state of fusion, require to be formed of a fine paste, which is only to be obtained by mechanical division of the earths; others intended for the fusion of metals and other matters which act very little upon them, and being required to support sudden and extreme variations of temperature without breaking, require to be formed of a mixture of ciment, or burned clay, with the raw clay. By this means a kind of pottery is obtained, the coarse texture of which, in some measure, resembles the breccias or pudding-stones, which easily undergoes those rapid changes of temperature.

The baking of pottery is likewise an object of extreme importance. It is necessary that

* Communicated to the Philomathic Society at Paris, and inserted in their bulletin, No. 26, in the year vii.

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the heat should be such as to expel the moisture, and agglutinate the parts which enter into the composition of the paste; but incapable of effecting the fusion, which if too far advanced, will render the ware of so homogeneous a texture, as to become brittle. The same effect happens with regard to the fine pottery, because the extreme division of the earth puts them nearly in the same state as if the materials had been fused. Hence it is that porcelains which have been strongly baked are more or less brittle, and cannot well be subjected to changes of temperature: and hence also it is that coarse porcelains, into which a certain quantity of baked clay is mixed, retorts, crucibles, porcelain tubes, and common coarse pottery, are much less brittle than plates or dishes formed of the same materials in a more divided state.

The general and relative dimensions of the different parts of pottery are also of great consequence to the manner in which they support the fire. In some cases the glaze, particularly when too thick, and of a very different nature from the body of the pottery, likewise occasions it to break.

It is therefore essential in all cases in the fabrication of pottery, 1st, To observe the best proportions of the ingredients; 2d, To divide the materials more or less finely, according to the use to which the ware is to be applied, and to give all the parts as nearly as possible the same dimensions; 3d, To bake with as strong a heat as can be given without fusion; and 4th, To apply a thin coat of glaze, the fusibility of which ought to be as nearly as possible approaching to that of the body, in order that the combination may be more intimate.

From a conviction that the goodness of pottery principally depends on the due proportions of earths which enter into their composition, Citizen Vauquelin has thought it might prove interesting to those who are employed in this important manufacture, to be acquainted with the analysis of the different natural clays employed for this purpose, and of the pottery made from some of them, in order that when any new clay is discovered, it may be known by simple analysis which kind of pottery it may the nearest resemble.

	Hessian crucibles.	Clay of Dreux.	Capsules of porcelain.	Pyrometers of Wedgwood.
Silex,	69	43,5	61	64,2
Alumine,	21,5	33,2	28	25
Charcoal,	1	3,5	6	6
Oxide of iron,	8	1	0,5	0,2
Water,	-	18	-	6,2

Crude kaolin 104 parts contain,—silex 74,—alumine 16,5,—lime 2,—and water 7.—One hundred parts of this earth afforded eight of alum, after treatment with sulphuric acid.

Washed kaolin 100 parts afforded,—silex 55,—alumine 27,—lime 2,—iron 0,5,—water 14.—This kaolin, treated with the sulphuric acid, afforded between 45 and 50 per cent of alum.

Petuntze afforded silex 74,—alumine 14,5,—lime 5,5;—loss 6.—One hundred parts of this substance treated with the sulphuric acid, afforded 7 or 8 parts of alum. But this quantity does not account for the loss which was sustained.

Porcelain retorts, silex 64,—alumine 28,8,—lime 4,55,—iron 0,50;—loss 2,77.—When treated by the sulphuric acid, this porcelain did not afford alum.

Description

VIII.

Description of a Thermometer, which marks the greatest Degree of Heat and Cold from one Time of Observation to another, and may also register its own Height at every Instant. By ALEXANDER KEITH, *Esq. F.R.S. and F.A.S. Edinburgh.* *

THERMOMETERS have hitherto been defective for meteorological purposes, in so far as they only point out the degree of heat at the moment of inspecting them, but do not show what the difference of temperature has been from the time of one observation to that of another. Nor has any instrument been yet constructed, so far as I have been able to learn, which will record the intermediate degrees of heat.

The ingenious Robert Hooke, in the end of the last century, mentions his intention of making a thermometer for the same purpose; but it does not appear that it ever was executed; neither does he explain how it was to have been done.

The thermometer invented by Mr. James Six, as described in the 72d volume of the *Philosophical Transactions* of the royal society of London, is made to show its greatest rise or fall from one period of observation to another. This is done by means of two small pieces of black glass which float on two different surfaces of mercury, within two glass tubes hermetically sealed. These floats, when raised to their greatest height, adhere to the side of the tube by means of a spring of glass, and become stationary, although the mercury falls.

After the observer has taken a note of the temperature, he, by a magnet held in his hand, draws down the float to the surface of the mercury, in consequence of a small bit of steel wire inclosed in the float, and the instrument is prepared for another observation. This is an ingenious invention, but requires too delicate workmanship to be fit for common use; besides it cannot be made to record the degrees of heat at intermediate periods. The thermometer lately invented by Dr. Rutherford of Balilish, and described in the third volume of the *Transactions* of this society, is also an ingenious contrivance; but has the same defect of marking only the extreme points to which the liquor has risen or fallen in two separate glass tubes.

Several years ago it occurred to me, that an air thermometer might be used for the purposes required, provided the weight of the atmosphere could be excluded, or a counter balance formed to it; and as the whole instrument could be made to rise or fall by the temperature of the atmosphere alone, it might be adapted to a piece of clock-work, which would record the degrees of heat at every instant through the year; and accordingly I read to this society a description of the instrument. But having formed another instrument of a more simple construction to answer the same purpose, I beg leave to give a description of it.

A B is a tube about fourteen inches long (pl. XII.) and three fourths of an inch caliber, of thin glass, sealed or closed at top. To the bottom, which is bent upwards, there is joined a glass tube seven inches long, and four tenths of an inch caliber, open at top. The tube

* Edin. Transf. vol. IV.

A B

A B is filled with the ſtrongeſt ſpirit of wine or alcohol, and from B to E is filled with mercury.

It will be evident from inſpection, that if the ſpirit of wine is expanded by heat, the mercury in the ſmaller tube will riſe; and if the ſpirit of wine is contracted by cold, the mercury will fall; and although they are both ſubjected to the preſſure of the atmoſphere, yet as liquids are incompreſſible by weight in any perceptible degree, neither the ſpirit of wine nor mercury will be altered in bulk by the different weights of the atmoſphere.

F D is a ſcale of braſs or ivory about ſix and a half inches long, divided in the uſual way.

E is a ſmall conical piece of ivory or glaſs of a proper weight, made to float on the ſurface of the mercury in the ſmaller tube; to which float is joined a wire reaching to H having a knee bent at a right angle, which raiſes one index, and depreſſes another index, according as the mercury riſes or falls, which wire ſhall be termed the *float wire.*

I I is a glaſs tube ſeven inches and a half long, cloſed at top, and open at bottom, ſo wide as to ſlide eaſily over the ſcale; and by means of a braſs rim cemented to it is made to fit exactly to the circular baſe of the ſcale, ſo that when this tube is put on, it covers the whole ſcale, and hinders and defends them from wind or rain. This cover need not be taken off, except when the inſtrument is to be prepared for an obſervation.

The operation of the float and indexes will be better underſtood from fig. 2, which repreſents them of the full ſize.

F G is the ſcale fixed to a circular piece of wood or braſs through which the top of the ſmall tube is made to paſs.

From G to K is a piece of the ſmalleſt harpſichord wire, or rather of the ſmalleſt gold wire, ſtretched along the ſcale, fixed at the ends by two braſs pins.

L, L, are two indexes formed of thin black oiled ſilk, pierced by the ſmall wire in ſuch a manner as to ſlide upwards and downwards with a very ſmall force, not more than two grains.

H, the knee of the float wire, before deſcribed, is made to encompaſs the ſmall wire between the two indexes, ſo that when the float riſes, the upper index is moved upwards, and when it deſcends, it leaves the upper index ſtationary, and puſhes down the lower index, which is alſo left ſtationary when the float riſes.

When the inſtrument is to be prepared for an obſervation, the one index is to be pulled down, and the other raiſed, by means of a bit of wire bent for the purpoſe, until both indexes touch the knee of the float wire; and when it is again obſerved, the upper index will point out the greateſt degree of heat, and the lower the greateſt degree of cold.

If this thermometer is to be adapted to a piece of clock-work in order to record the degrees of heat at each hour and minute of time, it ought to be made of larger dimenſions. The large tube may be forty inches long, and not increaſed in diameter, but the ſmall tube ought to be enlarged in diameter, and not in length. By enlarging the tube which contains the ſpirit of wine in length only, it will be affected by heat and cold in as ſhort a time as that before deſcribed.

It is unneceſſary at preſent to explain the clock-work. It is ſufficient to ſay, that a hollow cylinder,

cylinder, of any light fubftance, feven inches long, and five inches diameter, is made to revolve upon a vertical axis once in thirty-one days, or a month; a piece of fmooth or vellum paper is put round this cylinder, pafted only at the joining, but fo as to make it adhere clofe to the cylinder; on this paper are drawn thirty-one equal perpendicular divifions, numbered at the top 1, 2, 3, &c. to correfpond to the thirty-one days of the month, each of which is fubdivided into fix parts to anfwer to four hours. The length of this cylinder is divided by lines furrounding it, or zones in fuch number as correfpond to the fcale of Fahrenheit's thermometer, namely, from 0 to 100 degrees. Thefe divifions ought to be engraved on copper-plate, and a great number of impreffions thrown off on fmooth or vellum paper, in order that one may be ready to put on each month.

Fig. 3, M N reprefents the cylinder covered with one of thefe impreffions. P P is the fcale fixed to the frame on which the cylinder turns. This fcale is divided into 100 of Fahrenheit's degrees, exactly correfponding to the divifions of the cylinder.

Q is a piece of black-lead pencil joined to the end of the float-wire in the place of the knee before mentioned. This pencil is made to prefs lightly on the cylinder by means of the fmall weight R. And as the pencil rifes or falls by heat and cold, it will mark the degrees on the fcale of the cylinder; and the cylinder being conftantly revolving, the divifion for each day and parts of a day will fucceffively be marked by the pencil, which will leave a trace defcribing an undulated line, diftinctly delineating the temperature of each day through the month. Thefe papers, when taken off and bound together, will make a complete regifter of the temperature for the year; or, if they are pafted to one another, they will form a thermometrical chart, by which the variations of heat and cold during the year may all be feen and compared by one glance of the eye.

By infpecting fig. 3, the effect of the inftrument may be feen. It appears that the papers had been put on the cylinder the firft day of the month at mid-day, when the thermometer ftood at 45°; that it fell gradually till midnight to 25°; thereafter it rofe till the 2 at 1 P. M. when it ftood at 42°; then it defcended at midnight to 35°; then on the fourth at mid-day it was to 50°; and at noon the tenth of the month it ftands at 40°.

If three inches be added to the length of the cylinder, it may be made to delineate the variations of the barometer as well as the thermometer, and thereby to form a complete chart or view of the progrefs of both of them. And if inftruments of this kind were kept in different parts of the country, and their charts compared together, it would afford much information with regard to meteorology.

On

IX.

On the Effects of apparent Attraction, or Repulsion, between Bodies floating upon or immersed in Fluids. By M. Monge.*

THE motions of apparent attraction or repulsion which take place between floating bodies on the surface of water or other fluids, are sufficiently remarkable to excite the curiosity of the most common observers. Men of considerable eminence in the sciences have ascribed them to direct forces acting between the bodies themselves, and have overlooked the agency of the fluid, which, indeed, performs the whole effect; while others have pointed out the true cause. Our author is among the former, and I have no doubt but his clear and perspicuous explanation will be acceptable to the reader.

If two light bodies, to which water is disposed to adhere, be suffered to float on the surface of that fluid, and left to themselves at the distance of several inches from each other, they will either remain at rest or obey the impulse of the wind or other accidental forces; but if they be placed at a small distance from each other, such, for example, as half an inch or less, they will rush together with an accelerated motion. A very perceptible force will be required to separate them, and whenever they are left to themselves they will rush together as before. Or otherwise the experiment may be made with a vessel containing water, which, by its adhesion to the sides, forms a kind of eminence or ring all round: if in this vessel a globe of cork be placed, this floating body will remain at rest while at a distance from the sides; but when brought within a certain small distance of the side, it will rush to it, and adhere in the same manner as one of the small bodies in the former experiment.

That these effects are not produced by the immediate operation of an attractive force in the bodies themselves, is proved by suspending them to long slender strings; in which situation they are not found to be affected by their vicinity to each other, or to the sides of the vessel, provided they do not touch the fluid. Or in the last experiment, if water be poured in while the floating body lies in contact with the side, it will be found that the apparent attraction will continue no longer than till the surface is perfectly level to the very border. And when, by the continuation of pouring, the fluid becomes convex towards the side, the floating body will be so far from adhering, that it will be repelled.

Again, if two bodies be suffered to float on the surface of water, one of which only is capable of being wetted; as for example, a ball of cork and another ball, of the same material, but blackened over by holding in a candle; these bodies will avoid each other, and if urged together they will fly asunder as soon as they are left at liberty. If the ball with the blackened surface be suffered to float alone, it will be repelled by the sides of the vessel, while the fluid remains elevated in contact with those sides; but, on the contrary, it will be attracted, when, by the act of filling, the border of the fluid is rendered convex.

* Abridged from the Memoirs of the Parisian Academy for 1789, p. 506.

If these balls be also suspended by strings, it will be found that the effects do not arise from any immediate attractive or repulsive force.

Lastly, If two bodies be suffered to float on the surface of the liquid, neither of which are capable of being wetted, an apparent attraction will take place between them; they will be attracted by the sides of the vessel when the fluid is convex, or repelled when its termination is concave.

These circumstances may be summed up by observing, 1. That two bodies susceptible of being wetted will appear to attract each other when floating on the surface of water. 2. When two bodies, not capable of being wetted, are either floated or immersed in a fluid, they will still have the same apparent attraction: and, 3. When the two bodies, either immersed or floating, are one of them capable of being wetted, and the other not, they will exert an action productive of the same effect as if a mutual repulsion were exerted between them.

As the prejudice the two drops of water mutually attract each other is generally received, the author gives a number of instances which tend to overthrow it. He observes, that two such drops are very far from exhibiting any attraction; insomuch that they may even in a dry state of the air be struck against each other, so as to alter their figure without uniting together. These separate drops may be shewn to advantage by causing the fluid to fall in drops through a small tube, from the height of a few lines, upon the surface of a mass of the same fluid. The tube must be slightly inclined, in order that the drop may run away in the horizontal direction. Spirit of wine is better adapted to this experiment than water. The spherical drops move along the surface with the utmost ease and rapidity; sometimes striking each other so as to change their figure without uniting, and it is a considerable time before they confound themselves with the general mass. M. M. thinks the evaporability of the spirit and the mutual attraction of its parts are the causes why this fluid exhibits the phenomenon in a more striking manner than water; in which last fluid it is, however, often seen when agitated by the dashing of oars, the f.ll of violent rain, and the like. It is very easy to distinguish these solid globules from the vesicles or hemispherical bubbles filled with air, because the former are much more moveable.

This appearance of drops rolling on the surface of a liquid is more durable the smaller the drops. When any heated liquor of a brown or dark colour, coffee for example, is exposed to the air, the stratum of heated air in contact with the surface becomes loaded with vapour, and rises to an elevation, where it cools and deposits part of the water in the form of a smoke or vapour, which condenses into drops that fall on the surface of the coloured liquid, and constitute a kind of scum. These globules being small retain their situation a long time, and are very distinguishable from bubbles by their great mobility, as the slightest breath of wind is sufficient to disperse and arrange them along the sides of the vessel.

Our author adopts the explanation of Mariotte for the second general fact or law, which is, that floating bodies not capable of being wetted will apparently attract each other. In fig 1. plate XI. the globes A and A′ float upon the fluid, in which each makes a cavity of considerable

...derable extent, very eafily obfervable, by attending to the reflection of a ftrait body, fuch as the frame of a window from the furface. In fig. 2, where the bodies are brought nearer, the cavities do not communicate, and, confequently, the reaction of the water all round each ball is the fame, and they have no tendency to move in one direction more than another. But when they are brought ftill nearer, as in fig. 3, the effect of the repulfive powers of both depreffes the furface between them, and the reaction of the fluid becomes lefs in proportion to its diminifhed height. The confequence is, that the bodies move towards each other from the external preffure or reaction which continues undiminifhed.

When two bodies of this kind are fubmerged, as in fig. 4, the fluid will be difplaced from between them, if the air can have accefs, and the bodies will be preffed together by the force on the oppofite parts. If the air cannot enter between them, the liquid will not be excluded unlefs the force of repulfion be equal to that of the preffure of the atmofphere; but the tendency to feparation is productive of the fame effect. Philofophers who operate with the pneumatic apparatus over mercury, have frequent occafions to notice this fact, when they plunge their hands in that denfe fluid. For, upon attempting to feparate the fingers in this fituation, a refiftance is felt, which does not arife from any difficulty of moving the fluid, but fimply from this caufe. It is a perception as if the fingers attracted each other, and is not felt except when they are near each other.

Fig. 5 reprefents two bodies, A′ and A, in the fituation of the third general fact or law. One of them being wetted has a mafs of the fluid hanging round it, and the other forms a furrounding cavity by its repulfion. At the diftance here expreffed the bodies do not affect each other. In fig. 6 the general furface B is ftill unaltered: but at fig. 7 the curves interfere. The body A may be confidered as if on a moveable inclined plane of water, down which it will flide, while the plane itfelf, with the body A′, to which it is attached, will be moved in the contrary direction. The bodies will feem to repel each other.

As this effect alfo takes place beneath the fluid, that is to fay, that the body which is difpofed to be wetted, or which attracts the fluid, will exert a kind of repulfion upon any other body which repels the fluid, our author infers, that it may be confidered as a pretty accurate reprefentation of what happens in chemical precipitations. This, however, feems fcarcely to apply to the cafe, as both principles in chemical folution appear to have an attraction to the folvent.

The firft law or general fact, where apparent attraction is produced in two bodies capable of being wetted, employs a large portion of his refearch. If (fig. 8) a flip of glafs be fufpended at the extremity of a thread fixed to the point K, fo that its lower end fhall be plunged in the water, reprefented by MN, it is well known that the fluid will attach itfelf to the fides of the glafs, fo as to form two concave furfaces, EDC, HGF, higher than the common furface. As we are not acquainted with the laws of the force by which thefe effects are produced, and no obfervations have been made on the dimenfions of the curve itfelf, little can be faid

 concerning

concerning its figure or dimensions. It is only known that it rises according to the nature and temperature of the fluid, and the material of which the slip itself is composed.

The two masses of water raised by the plate of glass, tend to give it motion downwards by adding to its weight, and sideways by the action of the water, which tends to move in the horizontal direction. M. Monge compares this to the action of a chain, having the figure of the curve, but variously loaded in its different parts. In the present figure the horizontal actions are precisely alike, and consequently equal; but it is easily to conceive that they may become unequal, and also to shew the same by experiment. FBA, fig. 9, represents a drop of water lying on a smooth plane: to this the plate CA, supposed to be moveable on the centre D, is applied. The effect of the action, which is now either wholly or chiefly on the side EF, tends to carry the extremity A towards B, and would cause the plate to place itself in the position represented in fig. 10, if gravity or other circumstances did not interfere.

But the attraction is more particularly seen by suspending two slips or plates of glass, as in fig 11. These will retain the vertical position as long as they are more distant from each other than the limits of this capillary attraction; but when, as in fig. 12, the curves interfere, other consequences must follow. Suppose the water to acquire the figure AHB at its surface, and the quantity which the two plates suspended before will be greater by the portion CHD than it is at present. The power of suspension is not, therefore, saturated, and the water will rise, for example, to IGK, and this elevated water, independent of any increase of attraction from the diminished distance, is considered by the author as a weight suspended to the chain which connects the two plates, and producing a greater reaction on the plates themselves. The plates will not, therefore, continue in equilibrio, but will come together and strongly adhere.

This effect of fluids upon the sides of capillary vacuities, whether it be well accounted for or not by the comparative argument of the weight, is of considerable moment in the phenomena of nature. M. Monge observes, that as these plates come together and are retained, not by any direct attractive force, but indirectly by the operation of the fluid; so likewise by extending the argument to submerged bodies we may account for the strong adhesion of the parts of the crystals of salts in their separation from water, such as the sulphate of lime, which loses its water of crystallization and its tenacity by heat, sulphate of soda, and many other hard crystals, which fall into powder by drying, &c.

The laws of attraction, when two plates are plunged in mercury, as in fig. 13, is also considered by our author. This pressure will be equal to the weight of a column of the fluid, whose base is the surface DH of the glass, and height is half the depth between D and H. If the plates be placed horizontally beneath the fluid, as in fig. 14, the pressure will be twice as great, and constant, provided the depth AB be at least equal to the limit of repulsion IH. It may also be remarked, that if a small hole M, fig. 15, be made at less depth, the mercury will not flow out, though it would issue from the hole M, fig. 14, below that depth. Hence

it

... feen why a certain degree of preffure is required to force mercury through a fmall aperture, and a fmall quantity of this fluid may be retained in a cloth without paffing through.

A few experiments were made by the author to determine the elevations and reactions when the plates were feparated by the interpofition of a wire of known thicknefs. When the diftance was four thirty-third parts of a line, the elevation of the water was 15 lines and a half.—When it was four forty-ninth parts the elevation was 33 lines and a quarter; and when it was one twenty-eighth part the elevation was 74 lines. But, from fome difficulties which prefented themfelves, it was not practicable to meafure the adhefion farther than to determine that they were much lefs than the preffure of a column of water, having the furface of the plate for its bafe, and the column of liquid for its height.

X.

Obfervations on the Proportion of real Acid in the three ancient known mineral Acids, and on the Ingredients in various neutral Salts and other Compounds. By RICHARD KIRWAN, *Efq. LL.D. F.R.S. and M.R.I.A.* *(Continued from page 215.)*

Of the Ufe of thefe Tables. (Without abridgement.)

PROB. 1. AN extratabular fpecific gravity being given, but intermediate between fome of thofe in the table, *to find the quantity of real acid in* 100 parts of fuch acid liquor.

1ft. Find the difference betwixt the next higher and lower tabular denfities = D, and alfo the difference betwixt their acid contents = D′.

2d. Find the difference betwixt the *extratabular* fp. gravity and the next upper or next lower, whichever it is neareft to, = *d*, and let the difference betwixt its acid contents (or quantity of real acid) and thofe of the next upper or lower = *d*′, which is the quantity fought; then as D. D′ : : *d*. *d*′, then $d' = \frac{D'\ d}{D}$, confquently *d*′ added to the acid contents of the lower tabular fp. grav. or fubtracted from the upper, *is the quantity fought.*

Note. In general when *d*, that is, the difference between the extratabular fp. grav. and any tabular fp. grav. does not exceed $\frac{2}{10000}$ it is infenfible, and the acid contents of the lower or upper, whichever is neareft, may be afcribed to it.

PROB. 2. The *quantity of real acid* in 100 parts of an acid liquor being given but *extratabular*, being intermediate between fome of the quantities in the tables, *to find the fp. grav: of* fuch acid liquor.

Find D, D′ and *d*′ as in the foregoing problem; then $d = \frac{D\ d'}{D'}$; then *d*, added to the lower tabular fp. grav. or fubtracted from the upper, gives the fp. grav. fought.

But

But with regard to the *marine acid* its ſp. grav. is to be inveſtigated according to the ordinary mathematical rules.

Prob. 3. To find how much water muſt be added to 100 parts of an acid liquor of a given ſp. grav. to bring it down to another lower given ſp. grav.

1ſt. Find by the table the quantities of acid and water in 100 parts of each of the acid liquors reſpectively, each being ſuppoſed to be in the table, let the quantity of water in the denſer be W, and the quantity of acid $= A$, let the quantity of water in the leſs denſe $= w$, and the quantity of acid $= a$, and the quantity of water to be added to 100 parts of the denſer $= m$: then $W + m$ muſt be to A as w to a. And $W a + a m = A w$. And $a m = A w - W a$. And $m = \frac{A w - W a}{a}$.

Prob. 4. Given weights of two *or more acid liquors* of different ſp. gravities being mixed, to find the quantity of real acid in 100 parts of the mixt liquor and its ſp. grav.

Find the ſum of the quantities of real acid in 100 parts of the mixture, then find the reſulting ſp. grav. by the 2d problem; if the given ſp. gravities be extratabular, the operation muſt be more tedious, as the acid contents of each muſt be found.

Prob. 5. The quantity of an acid liquor requiſite to ſaturate 100 *parts* of *any baſis* being found, to find the ſp. grav. of that acid liquor.

1ſt. Find by the 4th table the quantity of real acid requiſite to ſaturate 100 *parts* of the given baſis, it is then plain that the *given* quantity of acid liquor contains the requiſite quantity of *real acid*, ſince it is ſuppoſed to ſaturate 100 parts of the baſis, and hence we may ſee how much 100 parts of ſuch acid liquor contains of *real acid*, and if this laſt found quantity be in the table, its ſp. grav. will be ſeen, but if extratabular, its ſp. grav. muſt be ſought by the 2d problem.

Prob. 6. The quantity of real acid requiſite to ſaturate 100 parts of any baſis being known, to find how much of one acid liquor of any given ſp. grav. is requiſite to ſaturate that, and conſequently any other given quantity of ſuch baſis.

If the given ſp. grav. of the acid liquor be *tabular* the quantity of real acid in 100 parts of it is apparent, and conſequently the quantity of ſuch acid liquor containing the required quantity of real acid is eaſily found by the rule of proportion. But if the given ſp. grav. is *extratabular*, the quantity of real acid in 100 parts of the acid liquor muſt be ſought by the firſt problem.

Prob. 7. The quantity of real acid in a given quantity of an acid liquor being known, and alſo the quantity requiſite to ſaturate 100 parts of any given baſis. To diſcover the quantity of ſuch a baſis contained in any ſolution, or in any powder, by which the given quantity of acid liquor is ſaturated.

If the baſis be ſingle (that is unmixed with any other baſis to which the acid may unite) or combined only with fixed air, the ſolution is eaſy, but if the given baſes be mixed with other baſes combinable with the ſame acid, the ſolution is more complex and varies according to the variety of caſes.

Prob.

PROB. 8. To find how much of an acid liquor of *one ſort* will hold as much *real acid* as is held by a given weight of an acid liquor of *another ſort*, whoſe ſp. grav. is alſo given:—for inſtance, how much vitriolic acid will contain the ſame quantity of real acid as is contained in 100 grains nitric acid, whoſe ſp. grav. is 1,3925.

1ſt. Firſt find by the table the quantity of real acid contained in the given quantity of the ſecond acid, whoſe ſp. grav. is given, or if not in the table it muſt be found by problem 1ſt.

2d. It is apparent that the quantity of the firſt acid liquor muſt vary with its ſp. gr. thus, in the inſtance given, as 100 parts nitrous acid of the ſp. grav. 1,392 contains 50 parts real nitrous acid, ſo 100 parts vitriolic acid, whoſe ſp. grav. is 1,5202 contains by the table the ſame quantity of real acid, *v. z.* 50 parts, but of the vitriolic acid, whoſe ſp. grav. is 1,800, only 64 parts are requiſite to contain 50 parts of real acid, whereas 200 grains are requiſite of the vitriolic acid, whoſe ſp. grav. is 1,2320.

Note. The ſolution of this problem may hereafter be found of uſe in comparing the quantities and affinities of oxygen in different acids.

PROB. 9. To find the ſp. grav. of ſuch vitriolic acid as that 100 parts of it ſhall contain the ſame quantity of real acid as 100 parts of the nitrous.

This can be found only by *inſpection* on conſulting the tables; an example has been ſeen in the laſt problem; ſo alſo 100 parts vitriolic acid 1,3102 contain the ſame quantity of real acid as 100 parts of nitrous acid, whoſe ſp. grav. is 1,2687. And 100 grains vitriolic acid, whoſe ſp. grav. is 1,1746, contains the ſame quantity of real acid as 100 grains ſp. ſalt, whoſe ſp. grav. is 1,159.

And 100 grains nitrous acid 1,1963 contains the ſame quantity of real acid as 100 grains ſpirit of ſalt whoſe ſp. grav. is 1,187.

Hence it ſhould ſeem that the ſp. grav. of the *real marine acid* is ſmaller than that of the *real nitrous*, and that of the real nitrous ſmaller than that of the *real vitriolic*, ſince when the weight of each acid, and alſo the weight of real acid in each is equal, the vitriolic acid is ſpecifically heavier than the nitrous, and the nitrous than the marine; but this, perhaps, may ariſe from penetration.

PROB. 10. To find how much of a neutral ſalt of one ſort holds as much real acid or baſis as a given weight of the *ſame* neutral ſalt in another ſtate, or as a given weight of *another* ſalt in any given ſtate.

Theſe queſtions are reſolved by the 4th and 5th tables. Thus, if it be aſked how much nitre contains as much acid as 20 grains of vitriolated tartarin? By the 4th table I ſee that 221,48 parts of vitriolated tartarin and 227,22 parts nitre contain equal quantities of acid ſince both contain 100 parts, then as 221,48 . 227,22 : : 20 . 20,5.

Again, How much deſiccated ſoda will hold as much alkali as 30 parts cryſtallized ſoda? In the 5th table I ſee that 541,1 parts of the cryſtallized hold as much alkali as 227,4 parts of the deſiccated, then as 541,1 : 227,4 :: 30 : 12,6.

PROB. 11. How much of a given baſis will be requiſite to ſaturate the acid contained in a given

given quantity of a given neutral ſalt; thus, how much deſiccated ſoda will be requiſite to ſaturate the acid contained in 50 parts cryſtallized Epſom?

By the 4th table I ſee that 100 parts real vitriolic acid are contained in 340 parts cryſtallized Epſom. Then if 340 : 100 :: 50 : 14,7, then by the 3d table I ſee that 100 grains of real vitriolic acid ſaturate 78,32 of ſoda. Conſequently if 100 ſaturate 78,32 :: 14,7 would ſaturate 11,51 of ſoda.

Laſtly, In the 6th table I find that 100 grains deſiccated ſoda contain 60 of ſoda. Then if 100 : 60 :: x : 11,51; then x = 19,1 parts deſiccated ſoda. Then 19,1 parts deſiccated ſoda will ſaturate the acid contained in 50 parts cryſtallized Epſom.

Note 1ſt. This problem is of uſe in determining the quantity of any precipitating ſubſtances to be employed in decompoſitions, operated either by a ſingle or double affinity. But in moſt caſes *more* of the precipitant muſt be employed than the exact quantity neceſſary for ſaturation, and particularly when decompoſitions are attempted in the dry way, as otherwiſe a complete contact with the ſubſtance to be decompoſed will not be attained, or if volatile it may be ſublimed before the decompoſition takes place.

PROB. 12. Some analyſts have denoted the ſtrength of their acids by expreſſing the quantities of each neceſſary to ſaturate a *certain quantity* of alkaline liquor (and ſometimes of another baſis), without even telling whether the alkali was mild or cauſtic, or the quantity of it contained in the alkaline liquor. This problem is conſequently indeterminate. However, a method of giving ſome ſolutions of it may be underſtood from the following example; and circumſtances will generally ſhew whether the application to particular caſes be juſt.

Link tells us that 240 grains of a vitriolic acid, which he employed, ſaturated 6,5 times its weight of tartarin (he muſt mean in a liquid ſtate, as no vitriolic acid will ſaturate ſix times its weight of real alkali), and that 240 grains of the nitrous acid he employed ſaturated 2,5 times its weight of the ſame alkali. *Quere*, the ſpec. gravity of both acids?

1ſt. It is plain, that ſince 240 grs. of the nitrous acid ſaturated 2,5 times its weight of the alkali, 624 grs. of that acid would ſaturate 6,5 times its weight of the alkali; and ſince 624 grs. of the nitrous acid would ſaturate as much alkali as 240 of the vitriolic acid, then 260 grs. of it would ſaturate as much alkali as 100 grs. of the vitriolic acid could ſaturate. Therefore, ſuppoſing 100 of the vitriolic acid to contain 75 of real acid, ſince more real nitrous acid is required to ſaturate a given quantity of tartarin than of vitriolic acid, in the inverſe ratio of 1214 to 1177 (as appears by the third table), then denoting the quantity of real nitrous acid in 260 grs. of the nitrous liquor by x, we have the following equation, as 1214 : 1177 :: x : 75. and x = 77,55. Then 260 grs. of the nitrous acid contain 77,55 of real nitrous acid, conſequently 100 grs. of it contained 29,82 real acid. And, therefore, its ſp. grav. was nearly 1,234, and that of the vitriolic about 1,800.——The quantity of alkali in the alkaline liquor might alſo on this ſuppoſition be determined.

So if it be required to know how much common ſalt is requiſite to decompoſe a ſolution of nitrated ſilver containing 176,25 grs. of ſilver.

1ſt. I find

1st. I find by the 6th table that 75 grs. silver take 16,54 of marine acid, consequently 176,25 grs. silver take up 38,87.

2d. By the 4th table, I find that 100 grs. muriatic acid are contained in 257,2 of common salt, consequently 38,87 are contained in 99,973, that is 100 grs. common salt, then 100 grs. of it are necessary to precipitate the silver.

XI.

Experiments on certain Principles obtained from Animal Substances treated with the Nitrous Acid. By Cit. Welter.*

THE author having treated silk with the nitric acid in order to obtain the oxalic, was surprized to find none, and to obtain at the end of the operation a silky salt of a golden yellow colour, which acted in the same manner as gunpowder by the contact of an ignited coal. As he made this experiment only once, he has thought fit to describe it, that others may repeat it.

He poured upon one part of silk six parts of common nitrous acid, to which a small quantity of concentrated nitrous acid was added. After two days' repose, he distilled the mixture, and adding the contents of the retort and receiver together, he threw the whole on a filter. The oxalic acid having crystallized on the filter, he returned the whole into the retort, together with a considerable quantity of water which was used in washing the filter. A quantity of the water was then distilled off, but the residue not crystallizing, he returned the distilled liquor; and after repeating this operation several times, he obtained for a residue an acid liquor of the same weight as the silk, which contained small crystalline grains.

This fluid afforded no signs of oxalic acid. It was yellowish, and tinged the fingers and silk of the same colour, which was not weakened by washing with water.

Cit. Welter saturated this fluid with lime; and after having concentrated it, he poured alcohol to it, which separated a substance of a gummy appearance. The alcohol diluted with water was then evaporated, and left a yellow substance mixed with the solutions of calcareous nitrate and muriate. He decomposed these salts by the carbonate of potash, and the fluid separated from the carborate of lime was submitted to evaporation. It afforded gold-coloured crystals as fine as silk, which detonated like gunpowder with a black smoke. These crystals are soluble in water and in alcohol, and crystallize by cooling. The oxygenated muriatic acid renders them colourless. The sulphuric acid disengages a smell of nitric acid. The muriatic acid occasions in these solutions a precipitate of small whitish micaceous crystals, which are volatile, and in the fire emit a bitter and inflammable fume.

* Abridged account of a memoir read before the French National Institute. Bulletin de la Société Philomatique, no. 25. Germinal vii.

This golden, yellow, detonating, and cryſtallizable ſubſtance, is called the bitter principle by the author. Its cryſtals appear to be octahedrons.

As animal ſubſtances become yellow by the contact of nitrous acid, citizen Welter endeavoured to extract the latter principle from beef; but he found it combined with another ſubſtance likewiſe unchangeable by the nitric acid. This combination, which is ſoluble in the concentrated nitric acid, is ſeparated by water in the form of a yellow powder, which does not change by expoſure to the air, and may probably be uſeful in painting.

Cit. Welter was induced to ſuppoſe, that this powder is compoſed of the bitter principle, and another ſubſtance, from the circumſtance of his having obtained the latter ſubſtance by treating ſponge with the nitric acid. It is colourleſs, ſoluble in the concentrated nitric acid, and precipitable by water like the other powder.

Theſe facts ſeem to ſhow, that animal matters treated with the nitric acid, afford two ſubſtances as a reſidue, which are not changed by this acid, and are found either in a combined or ſeparate ſtate. It appears that ſilk affords the bitter principle in a ſtate of purity. Sponge affords the ſecond ſubſtance pure; and beef affords a combination of both. The bitter principle is yellow and ſoluble in water; the combination of both is alſo inſoluble in water, but coloured.

Cit. Welter remarks, that having made his experiment only once, and not being able yet to determine with preciſion the circumſtances upon which the production of the bitter principle depends, he has thought it neceſſary to relate the whole.

XII.

A ſhort Account of the Life and Writings of Deſauſſure. By A. P. Decandole. *

HORACE Benedict Deſauſſure was born at Geneva in the year 1740; his father, an enlightened cultivator, to whom the public is indebted for ſome memoirs concerning rural economy, reſided at Conches, a country-houſe ſituated on the banks of the river Arve, half a league from Geneva. This habitual reſidence in the country, together with an active education, was undoubtedly the cauſe which developed in Deſauſſure that natural ſtrength of conſtitution ſo neceſſary to the practical cultivator of natural hiſtory. He went every day to the town, in order to profit by the advantage of public education. Reſiding at the foot of the Salêve, a mountain he has ſince rendered famous by his reſearches, it was an entertainment to him to climb its rugged paths. Living thus ſurrounded by the phenomena of nature, and poſſeſſing the advantage of ſtudy, he became attached to natural hiſtory, without imitating thoſe learned men who form theories without leaving their cabinets, nor thoſe men of mere practice, who being continually ſurrounded by natural ſcenes, become incapable of admiring their beauty.

* Decade Philoſ. An vii. 327.

His

His first passion was for botany. A varied soil, producing numerous different plants, invited the inhabitant of the borders of the Leman lake to cultivate this agreeable science. This taste of Desaussure led him to form a connection with the great Haller. He paid him a visit in 1764, during his retirement at Bex, and gives an account in his travels of his admiration for this surprizing man, who excelled in all the natural sciences. Desaussure was still more excited to study the vegetable kingdom by his connections with Charles Bonnet, who had married his aunt, and who soon perceived the value of his nephew's increasing talents. Bonnet was then employed on the leaves. Desaussure also studied these organs of vegetables, and published the result of his enquiries under the title of "Observations on the Bark of Leaves." This little work, which appeared soon after the year 1760, contains some new observations on the epidermis of leaves, and in particular on the miliary glands which cover them *.

About this time the place of professor of philosophy became vacant. Desaussure, then just in his twenty-first year, obtained it. Experience proves, that if very early recompences extinguish the zeal of those who exert themselves merely for the sake of reward, on the contrary they increase the industry of those who are in search of truth. At that time the two professors of philosophy taught by turns natural philosophy and logic. Desaussure filled these two offices with equal success. He gave a practical, we may say an experimental, turn to the science of logic. His course, which began with the study of the senses, in order to arrive at those general laws of the understanding, shewed that he was even then a close observer of nature.

Natural philosophy being the object of his attachment, led him to study chemistry and mineralogy; and soon afterwards he recommenced his travels in the mountains, not only to examine the plants, but to observe the mountains themselves, whether he considered their composition or the disposition of their masses. Geology, a science then scarcely known, gave a charm to his numerous walks in the Alps. Here it was that he discovered himself to be a truly great philosopher. During the fifteen or twenty first years of his professorship he was employed in performing the duties of his office, and in surveying the mountains in the neighbourhood of Geneva. He extended his excursions on one side as far as the banks of the Rhine, and on the other to Piedmont. About this time he made a journey into Auvergne, to examine the extinct volcanoes; and another to Paris, Holland, and England, and afterwards to Sicily. These voyages were not merely excursions from one place to another. They had only one object, namely, the study of nature. He never travelled without being provided with every instrument that might be useful to him; and always before he set out, he sketched the plan of the experiments and observations he intended to make. He often mentions in his works, that he found this method of great utility to him.

In 1779 he published the first volume of his Travels in the Alps. We there find a complete description of the environs of Geneva, and an excursion to Chamouni, a village at the

* He resumed this subject eighteen months before his death.

foot of Mont Blanc. Natural philosophers will read with pleasure the description of his magnetometer. The more he observed the mountains, the more he perceived the importance of mineralogy. In order to study it to greater advantage, he learned the German language; and in the last volumes of his Travels, we may easily perceive how much new mineralogical knowledge he had acquired.

During his numerous excursions among the Alps, and even in the midst of the political troubles of Geneva in 1782, he found opportunities to make his experiments on hygrometry, which he published in 1783 under the title of "An Essay on Hygrometry." This work, the best he ever wrote, completed his reputation as a natural philosopher. We are indebted to him for the invention of an hygrometer. Deluc had already invented an hygrometer of whalebone, on which subject a dispute was maintained between him and Desaussure, which was even attended with a considerable degree of earnestness.

In 1786 Desaussure resigned the place of professor, which he had held for nearly 25 years, to Pictet, his disciple and colleague, who performed with reputation to himself the difficult task of succeeding this great philosopher.

Desaussure being called upon by his office to attend to public education, made it a particular object of his attention. He presented a plan for reforming the course of education at Geneva. He proposed to teach children very early the natural sciences and mathematics; he was even attentive to their physical education; and, that it might not be neglected, proposed the adoption of gymastic exercises. This plan excited great attention in a town where every one is aware of the importance of education. It found both admirers and censurers. The mediocrity of their pecuniary resources was a great obstacle to every important innovation. They were apprehensive that in changing the form they might lose sight of the principle, and that an alteration, even for the better, might destroy the good they possessed. The Genevese were attached to their form of education, and they had cause, for it had not only introduced general information among them, but had given the first spring to the talents of several distinguished mathematicians *, and natural philosophers †.

Public education did not alone claim the attention of Desaussure. He attended himself to the education of his two sons and his daughter, who have shown themselves worthy of such an instructor. His daughter unites to the accomplishments of her sex, an extensive knowledge in the natural sciences. His eldest son is already known by his works in natural philosophy and chemistry.

The second volume of his Travels was published in 1786. It contains a description of the Alps which surround Mont Blanc. The author considers them as a mineralogist, geologist, and natural philosopher. It contains, in particular, some very interesting experiments on electricity, and a description of his electrometer, which is one of the most complete we possess. We are likewise indebted to him for several instruments of measurement; his cyanometer, designed to measure the intensity of the blue of the heavens, which varies according to

* Abauzit, Cramer, l'Huiler, F. Trembley, &c.

† Jalabert, A. Trembley, Bonnet, Lesage, Deluc, Senebier, Prevost, Pictet, and Desaussure himself.

its

its elevation; his diaphanometer, or his method of meaſuring the diaphaneity of the air; and his anemometer, in which, by means of a kind of balance, he weighs the power of the wind.

Some years after the publication of his ſecond volume, Deſauſſure was received as a foreign aſſociate of the academy of ſciences, and Geneva could boaſt of having two of its citizens in theſe ſeven eminent ſituations. Deſauſſure not only honoured, but was deſirous of ſerving his country. He founded the ſociety of arts, to which Geneva is indebted for that proſperity it has gained through its induſtry within the laſt thirty years. He preſided in this ſociety to the very laſt; and it was one of his principal objects to ſupport that uſeful eſtabliſhment.

He alſo ſhowed his zeal to ſerve his country while he was member of the Council of Five Hundred, and of the National Aſſembly. It was from his aſſiduous labour in that aſſembly that his health firſt began to fail; and in 1794 a paralytic ſtroke deprived him of the uſe of one ſide of his body. However painful his ſituation might then be, he loſt nothing of the activity of his mind; for it was after this accident that he drew up the two laſt volumes of his Travels, which appeared in 1796. They contain an account of his travels in the mountains of Piedmont, Switzerland, and in particular of his aſcent to the ſummit of Mont Blanc. Theſe two laſt volumes, ſo far from appearing to partake of the weakneſs of his condition, offer a conſiderable maſs of important facts and obſervations in natural philoſophy.

He gave the laſt proof of his attachment to ſcience in publiſhing the Agenda, which completes the fourth volume. Here this great man has ſurpaſſed himſelf. He conducts the young naturaliſt amidſt the mountains, and teaches him to obſerve them to advantage. This Agenda is a proof of his genius, and the ſtrength of mind he preſerved amidſt all his ſufferings. During his illneſs, he alſo publiſhed his obſervations *on the fuſibility of ſtones with the blowpipe*; and he directed the *experiments on the height of the bed of the Arve.* When he was at the baths of Plombieres for his health, he obſerved the mountains at a diſtance, and procured ſpecimens of the ſtrata he perceived in the moſt ſteep rocks. He had announced to the public, that he intended to complete his Travels by his ideas on the primitive ſtate of the earth; but the more new facts he acquired, and the more he meditated on this ſubject, the leſs could he determine with regard to thoſe great revolutions which have proceeded the preſent epoch. In general, his was a Neptunian, that is to ſay, he attributed to water the revolutions of this globe. He admitted it to be poſſible that elaſtic fluids, in diſengaging themſelves from the cavities, might raiſe mountains.

Though his health was gradually impaired by degrees, he ſtill retained the hope of re-eſtabliſhing it. The French government having appointed him profeſſor of natural philoſophy in the ſchool of Paris, he did not deſpair of poſſeſſing that honourable office at ſome future day; but his ſtrength failed him, and a general want of energy ſucceeded the activity he had formerly enjoyed. His ſlow and embarraſſed pronunciation no longer diſplayed the activity of his mind, but formed a ſtriking contraſt with the agreeable vivacity which formerly diſtinguiſhed him. It was an affecting ſight to behold this great man ſo worn out at a time of life when the mind is moſt active in meditation, or at leaſt when he ſhould have enjoyed the fame and knowledge he had acquired.

It

It was in vain he tried all the remedies which medicine, aſſiſted by the natural ſciences, could offer. Life and ſtrength abandoned him by ſlow and painful degrees, and towards the end of the 6th (republican) year, his decay became more evident; his memory failed; and at length, on the 3d of Pluvoiſe, in the 7th year, at the age of 59, he completed his brilliant career, much regretted by a family who loved him, a country to which he was an honour, and Europe, whoſe knowledge he had increaſed.

By his ſide, and at the ſame moment, a violent death robbed the ſciences of a young man whoſe induſtry and talents had afforded the moſt flattering hopes. (Qu. ?)

I muſt here conclude this ſhort account; and it may eaſily be perceived that I am very far from making the eloge of my illuſtrious countryman. I had neither the neceſſary materials, nor ſufficient means; that intereſting taſk is reſerved for one who has been the companion of his travels and labours, and who, by living habitually with him, has had the advantage of obſerving his manner of acting and thinking.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

On Steam.

AN apparatus has been exhibited to the Linen Board in Dublin, which, added to the boilers uſed in bleach-greens, will collect the ſteam, and communicate it to the machinery for turning the mill-work in room of, or in aid of, water-mills.

The truſtees of the linen manufactory are ſo well ſatisfied with this invention, that they give a conſiderable ſum in aid of thoſe who will firſt make trial of it in the large way.

There can be no doubt but that the machinery uſed in the linen manufacture may be wrought by the power of ſteam: and it is certain alſo, that a conſiderable quantity of ſteam is produced by the common mode of boiling the linen; which if collected, and applied to proper machinery, would do ſomewhat of work. Whether the power of this ſteam, as a firſt mover, is adequate to the coſt of the machinery, is a point on which I have yet heard nothing concluſive. To determine this point, we require to know what quantum of ſteam is equal to any known power; and what quantum of ſteam a boiler of a certain ſize will generate, kept a given time in a boiling ſtate? Or, in other words, how many cubic feet of water converted into ſteam, is equal to the power of one horſe? In the ſtate of boiling, what quantity of ſteam will be generated from a given quantity of water in a given time?

Mr. Nicholſon will oblige a conſtant reader, by referring in his next Journal to where the above data are to be found. A.

Londonderry, Auguſt, 1799.

On the theory of the dilation of ſteam and other elaſtic fluids, there is a valuable treatiſe by *Prony*, in the ſecond cahier of the *Journal de l'Ecole Polytechnique*; and the ſame author has treated the ſubject very ably in his *Architecture Hydraulique*. *Deſaguliers* has given ſome good documents of *Beighton*, in his *Courſe of Lectures*, with regard to the atmoſpheric ſteam-engine.

engine. Much practical information is to be found in *Curr's Coal-viewer, and Engine-builder's Practical Companion*, sold by Taylor, in Holborn. *Dr. Hutton* has also written on this subject, in his *Mathematical and Philosophical Dictionary*, art. *Steam*; and professor *Robison* more amply in the *Encyclopedia Britannica*, art. *Steam*, and *Steam-engine*.

W. N.

Pasigraphy, or Universal Writing.

A numerous meeting of friends to the arts and sciences was held the 25th of Prairial, at the Republican Lyceum at Paris, where C. Demainieux explained the twelve rules of Pasigraphy, and the three rules of Pasilaly, two arts which only make one, and of which he is the inventor. The former, as the term implies, is a method of universal writing, which can be read in all languages at once. That is to say, its characters, denoting ideas and not words, may be read at the same time, by six persons speaking each a different language, by communicating the very same ideas to each, who will express them to himself in his own terms. The second art is an universal language, of which that writing is the basis. The perspicuity of the exposition, the simplicity of the means, and the manifest utility of the results were much applauded. It is known that the National Institute has directed its attention to this discovery, and have appointed a commission to examine it. C. Ræderer, who is charged with this report, has already read it, in a private sitting of the class of moral and political sciences, of which he is a member. He treats the subject philosophically, not merely as an universal method of conveying our ideas in every language, but likewise in a still more important point of view, as a method of extending our researches, and carrying them to a higher degree of perfection.

Several foreign philosophers attended, and among them, M. Van Swinden, of Amsterdam, and M. d'Ossuna. *Mag. Encyclop.*

French Weights and Measures.

I have deferred rectifying the tables of French weights and measures, as notified on the wrapper of Number 27, till I shall receive the new report of the commission appointed for that purpose. Dr. Delametherie informs us, Journal de Phys. V. 460. that the metre is settled at 3 French feet, 11 lines, and 0.296 of a line.

Royal Institution of Great Britain,

For diffusing the knowledge, and facilitating the general introduction of useful mechanical inventions and improvements, &c. is now incorporated by charter, under the patronage of his majesty, by the title of the Royal Institution of Great Britain. The present officers are:

President,

President, the Earl of Winchilsea.—Vice-presidents: the Earl of Morton; the Earl of Egremont; and the Right Honourable Sir J. Banks, Bart. K.B. P.R.S.——Committee of managers.—For three years: Earl Spencer; Count Rumford; and Richard Clarke, Esq. Chamberlain of the City of London.——For two years: the Earl of Egremont; the Right Honourable Sir J. Banks; and Richard Joseph Sulivan, Esq.——For one year: the Earl of Morton; the Right Honourable Thomas Pelham; and Sir J. C. Hippisly, Bart.---Treasurer, Thomas Barnard, Esq.---Secretary, the Rev. S. Glasse, D.D. F.R.S.——Committee of visitors.—For three years: the Duke of Bridgewater; the Lord Bishop of Durham; and Thomas Bernard, Esq.——For two years: the Lord Viscount Palmerston; Lord Teignmouth; and Rowland Burdon, Esq.——For one year: the Earl of Besborough; Lord Somerville; and Samuel Thornton, Esq.——State of the subscription lists, the 6th of July, 1799:---138 persons had given in their names as subscribers of 50 guineas each, and hereditary proprietors of the institution; 103 persons, as subscribers for life, at 10 guineas each; and 97 persons as annual subscribers, at 2 guineas each;---338 subscribers.

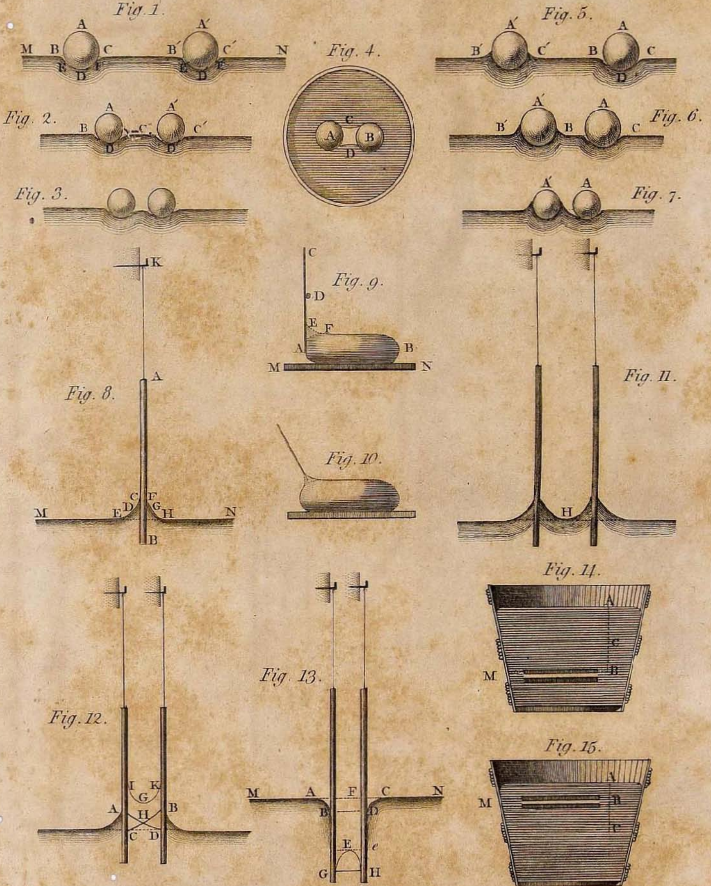
As many ingenious tradesmen and artificers may be desirous of having their inventions and their work exhibited in the repository of the institution, the managers, desirous of bringing forward into public view all such mechanical inventions and improvements as are worthy of the public notice, have resolved:

That the gratuitous assistance of such respectable and wealthy artists, tradesmen, and manufacturers, as deal in any of the articles that will be wanted in the arrangement of the institution, or as are proper to be exhibited in the repository, be accepted by the managers; and that the articles so furnished *gratis*, be marked with the names and places of abode of the donors. The managers have also resolved:

That the assistance of such ingenious and well-informed persons be accepted, as may offer their gratuitous services in the capacities of draughtsmen, artists, artificers, overseers of the work, &c. in the arrangement of the various details of the institution; and that persons so giving their voluntary assistance for a certain time, be permitted to have free access to the institution, and to attend, *gratis*, all the philosophical lectures during a certain period.

Persons who are desirous of availing themselves of either of these resolutions, are requested to send in their proposals in writing, to the managers of the institution, stating specifically, what they are willing to undertake.

Mr. Kirwan's Essay on the Analysis of Mineral Waters, and his Geological Essays, will both appear early this month (September).



Barlow sculp.

Fig. 1.

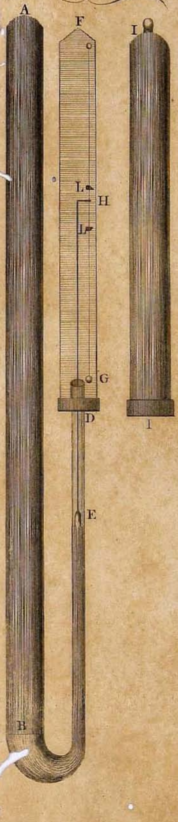


Fig. 2.

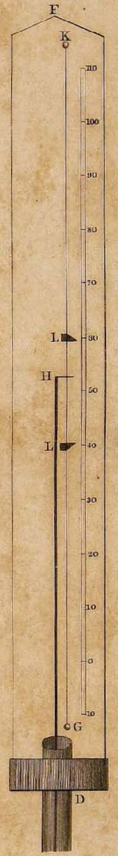
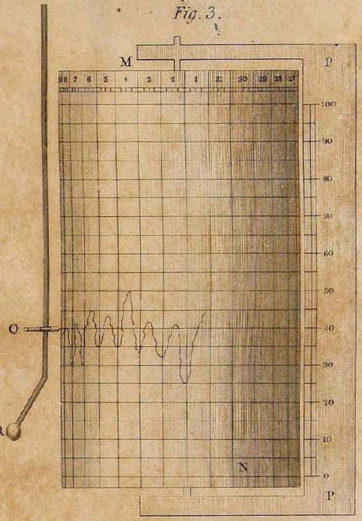


Fig. 3.



Barlow sculp.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER 1799.

ARTICLE I.

*Geological Obſervations on the Vicinity of Hull and Beverly. By JOHN ALDERSON, M.D.**

To Mr. Nicholson.

Sir,

In conſequence of the appearance, in your truly valuable miſcellany, of Dr. de Serra's account of a ſubmarine foreſt forming iſlets on the coaſt of Lincolnſhire, I have incloſed you a ſection of this part of Yorkſhire, which I made ſome years ago, to explain the origin and ſituation of our ſprings, and have tranſcribed my deſcription of the country as publiſhed in the Hull Advertiſer, October, 1795.

This neighbourhood (by this neighbourhood I mean the environs of Hull and Beverly, including all Holderneſs) may be divided into two diſtinct regions, or diſtricts—the high and the low lands. The high land is a ridge of hills, forming an arc or bow, extending from Flamborough-Head to the Humber, where it is divided; and is then continued down the Lincolnſhire ſide of that river to the point oppoſite the Spurn at the mouth of the Humber.

The eaſt ſide of the ridge is entirely compoſed of chalk and flint, in moſt places in regular horizontal ſtrata, the flint being from ſix to eight inches thick, the chalk from two to three feet; in a few places the flint is in nodules, and here and there in the chalk rock are found nodules of iron ore and thin ſtrata of fullers'-earth. Towards the baſe of the ridge, the particles of theſe earths are to be found waſhed down and forming gravel; this operation has

* Communicated by the author.

been evidently performed under water; for at a certain height the gravel ceaſes to be found, and the line is diſtinctly marked by the particles at that level being rounded, the conſequence of agitation, as on the ſea-beach.

The depth of the chalk rock is not known; it has been bored into 50 feet below the Humber; the height at Riplingham, or top of the wolds, is about 400 feet.

The low land, which ſtretches from the baſe of the hills to the ſea, whoſe cliff forms the ſtring of the bow, may again be divided into warp-land and alluvial: the former is uniformly level, and would be covered every high tide by the Humber, were it not reſtrained within proper limits by embankments. The latter, or alluvial, is uneven ground, and riſes by a gentle aſcent to the ſea cliff. This ground is partly gravel, partly clay, variouſly mixed with ſhells, and with, here and there, particles of culm or powdered coal. The cliff, in ſome parts, is near 100 feet above the level of the ſea.

The depth of the warp is nearly the difference of the tide, being about 22 feet; below this warp is found a ſtratum of vegetable mould, or moor earth, in which have grown trees of a great ſize: this ſtratum is about three feet deep, and may be found continued on the other ſide of the Humber, and may be ſeen at low ebb of ſpring-tides on both ſides at the ſame level. Below this moraſs is found a bed of ſand mixed with ſmall particles of carbonated wood, chalk, clay, and ſalt water, ſufficient to form what is called a * quick-ſand, varying in depth according to the diſtance from the hills. Below the bed of ſand is found a ſtratum of clay more or leſs thick and compact, and beneath this the chalk rock (a continuation of the loweſt ſtratum of the hills), which declines towards the Humber and the ſea about five yards in the mile; generally under this clay, and on the ſurface of the chalk rock, lies a reſervoir of water kept down by the clay: when this clay is perforated, the water riſes to its level, and, if well piped, will flow over the warp. This water breaks out at the baſe of the hills, at a place called Spring-Head, and is conveyed to Hull for the ſupply of the inhabitants.

The moor earth has been univerſally found at the ſame level wherever the ground has been bored, and may be found along the courſe of the Humber within low-water mark, and along the ſea coaſt from the Spurn to Bridlington. Although I have no poſitive facts that the moraſs extends under the alluvial part of Holderneſs, yet I have ſome reaſon to conclude it does from the following circumſtance: the moraſs at Seathorn or Withernſea, along the coaſt, which is only viſible at the loweſt ebb, is not one hundred yards from the cliff. Sixty yards of the cliff have gone into the ſea within the memory of many now alive, and there are writings, &c. of many cliff cloſes being transferred or aſſigned, whoſe identity is beyond the memory of any one at preſent living, and therefore I conclude the cliff extended over this moraſs. It may be ſuggeſted that this bog is the bottom of a mere or mar, ſimilar to that at Hornſea, where trees have been found: but there is no mention of any ſuch mar

* When Mr. Dodd, in the year 1797, ſuggeſted to me the poſſibility of making ſubterraneous paſſages under rivers, I ſhewed him the impracticability of it here, from the annexed ſection, owing to the nature of this quick-ſand, which defeats every attempt at well-making, by blowing up in the night the labour of the day.

near

near this place. From the regular falling of the cliff on the average of the laſt fifty years, this ſpot may have been uncovered within the preſent century.

The ſame may be preſumed at various places along the coaſt, where this ſubmarine wood, &c. are found within a ſhort diſtance from the cliffs that have been for many years gradually waſting. In this ſubmarine bog have been found the horns of the red deer, the head of an animal of the ox kind, oak, fir, and hazel; the nuts of the laſt quite perfect when firſt taken out. This wood is here called, not unaptly, Noah's Wood. A few years ago the trunk of a large tree was taken out, evidently cut off at both ends; and hollowed out ſo as to form a canoe capable of carrying a man; it was ſo perfect when found, that I am credibly informed it would have anſwered that purpoſe. I have alſo ſeen the head of an inſtrument reſembling an axe, formed of one ſtone; round one end a groove has been hollowed out, in which the handle of the inſtrument has been twiſted—in the ſame manner as the blackſmiths do at this day round ſome of their tools—and the other is wrought to a fine edge.

Another circumſtance which inclines me to believe that this part of Holderneſs lies upon this moor earth is, that a mile from the ſea, in ſinking a well, the workmen met with a boggy earth, which emitted ſo noxious a ſtench that they eſcaped ſuffocation with difficulty: this was at the level of the ſea. From the nature of the ground, which I have called alluvial, conſiſting of ſpecimens of every kind of rock, from Flamborough-Head to the North of Scotland, I have been led to conclude the whole of that part has been brought there by ſome great irruption of the Atlantic Ocean, and precipitated upon the ſurface of this vegetable mould.

I have alſo been of opinion, that there was a time when this country was inhabited at the level of this moraſs, before this irruption of the ſea, and that the Rhine and the Humber met, and mixed their waters, without the interference of the German Ocean. The manner in which the warp land has been formed from the Humber, ſince the ocean retired within its preſent limits, is very obvious: it is, at preſent, the practice in ſome parts of this country to ſuffer the Humber to paſs freely over the land, in order that the proprietors may obtain a new ſoil at will. From certain data, which might eaſily be obtained, and the calculation of Wallerius, that level ſoil left to itſelf riſes half an inch in a century, it would be no difficult matter to calculate the period of this deluge.

There are, as I mentioned before, traces of the water when it firſt broke in having riſen up the ſides of theſe hills, or wolds, ſomething more than the height of the higheſt of the lands in Holderneſs; for in many parts, both in Yorkſhire and Lincolnſhire, I have found the rounded pebbles at the level of 110 or 120 feet above the ſea; and I am of opinion, that, if ſearched for, at the ſame level there will be found * gravel, or other marks of the action of the ſea, in almoſt every part of the iſland. Similar inferences ſeem to have been drawn by Linnæus, Wallerius, and Pallas, in their obſervations on the Baltic.

Many of the hills of gravel in Holderneſs evidently ſhew that they were formed by a periodic ſubſidence from water; ſome of them appear to have been conical, and every diſtinct

* What height is Kenſington gravel pits?—A.

layer is conical: various sea shells are found in their natural state; others evidently the product of some more ancient hill being agatized. We also find culm, or the washings from some stratum of coal in the north, and the alum schistus from Whitby, all blended and mixed in one heterogeneous mass.

I am, Sir,

Your most obedient and very humble servant,

Hull, Aug. 14, 1799. JOHN ALDERSON, M.D.

II.

A circumstantial Description of the Method of cultivating the White Beet (Runkelrübe), in order to obtain the greatest Quantity of Saccharine Matter, and to prepare it for the Manufacture of Sugar. By F. C. ACHARD, Director of the Physical Class in the Royal Academy of Sciences. (Concluded from p. 243.)

18. IN support of these two positions, deduced from my own experiments, I wish to add a few well-known facts. With regard to the first, it is notorious, that not only the tender roots of asparagus, as well as those of hops, liquorice, and cucumber, possess a sweet taste so long as they are protected against the influence of light by being covered with earth; and that they lose it as soon as, by sprouting out from the ground, they are exposed to the light and its action: in which case they acquire the peculiar sharp and acid taste of the plant.—In the same manner the uncovered part of the lower cole-rape has a sharp taste, in comparison with those of its parts which are covered by the soil. Carrots sown between hemp and poppies, if all other circumstances, such as soil and manure, be alike, become always sweeter than those which are cultivated on a soil not shaded by other overhanging vegetables.

19. Some may, perhaps, ascribe the decrease of sweetness in the root-germs pushing out from the earth to the *access of air* thus facilitated, and not to the *matter* of light. But to refute this assertion, I shall only mention, that the air undoubtedly penetrates, in every instance, through the uppermost stratum of the ground, unless the soil be thoroughly argillaceous, and of course very compact. To this I add the fact known to every gardener, that asparagus coming forth from the ground and covered with a *glass*, acquires, in a short time, a green head, and changes its agreeable sweetness for a bitter taste; just as if it had not been covered. But if the asparagus be covered with an *opake* vessel, such as a flower-pot, &c. it continues white in its subsequent growth, and retains all its sweetness. By this it is fully proved, that the agency of the matter of light, with an exclusion of the action of air, is the only cause of the loss of sweetness in asparagus. The heads of carrots, parsnips, beet-root, and perhaps of all vegetable root, which are not covered with the earth, lose the sweet taste naturally belonging to them. And hence the lower part of the root, on which the light cannot

act,

act, is often very ſweet, while the upper part, not covered by the earth, and thus expoſed to action of light, is not in the leaſt ſweet, but even poſſeſſes a bitter taſte.

20. The very reverſe holds good with reſpect to fruits, more eſpecially from trees, at leaſt for the moſt part, if not with all of them. In this part of the plant the *matter of light* cauſes an *increaſe* of ſaccharine matter; and its influence is directly oppoſite to that which it exerts on the roots. I think it ſuperfluous to offer proofs of this phenomenon, as every amateur of gardening knows that the pleaſant and ſweet taſte of fruits from trees is moſt certainly produced by bringing them to a place where they are moſt expoſed to the ſun. Branches of one and the ſame tree yield fruits of different agreeable flavour and ſweetneſs, according as they are in various degrees acted upon by light; ſo that the fruits from ſhaded branches are much inferior to thoſe that are not ſhaded.

21. It is no leſs eaſy to prove, that by the abſorption of light in the roots, the *extractive, ſaponaceous*, and *farinaceous* principles are increaſed, which give to the juice of the root the quality of leaving, on inſpiſſation by fire, a tenacious, pulpy maſs, which, on account of its conſiſtence and other properties, throws great, and often inſurmountable, difficulties in the way of the ſeparation of the ſugar, and likewiſe makes it impoſſible to produce a ſyrup of a pure ſweetneſs.

22. To be thoroughly convinced of this, nothing more is required than to examine a runkelrübe, a carrot, a parſnip, a parſley-root, or any other root, in ſuch a manner, that the part above the ground may be compared with the part that remained beneath. It will then be found, that the latter not only contains more juice than the former, but, in general, alſo that both theſe ſeveral parts are materially different as to texture, weight, compactneſs, and colour. The ſweetneſs of the roots covered by earth may be diminiſhed, only by cauſing the light to act on the upper uncovered part. By ſuch procedure a conſiderable change in the ſaccharine matter, and other conſtituent parts, even in the lower part of the root, is effected.

23. Since the *chemical analyſis*, as well as the *phyſiology of plants*, evinces that all this deviation in the nature of the parts of one and the ſame plant, accordingly as it ſtands above or below the ground, is imputable to the increaſe of thoſe of its conſtituent parts which, taken together, impart to the inſpiſſated juice of the root the property of reſiſting the ſeparation of the ſugar by cryſtallization; it follows from thence, if applied to the runkelrübe, that by preventing, in the beſt poſſible manner, the acceſs of the matter of light to that part of the root which lies under the heart, or the place from which the leaves take their riſe, the runkelrübe muſt be produced, not only richer in ſugar, but alſo containing leſs of ſuch other parts as impede the ſeparation of the ſaccharine matter.

24. Upon the full proof of theſe aſſertions it is that my method of cultivating the runkelrübe is eſtabliſhed. I found it perfectly confirmed and eſtabliſhed by experiments, partly in my own culture, partly by comparing the ſaccharine contents of ſuch roots as had grown in various places, and under different management. I therefore affirm that the runkelrübe only which is produced in the manner I have here indicated, will be ſo rich in ſugar, and ſo

poor

poor in other parts, that render the ſeparation of the ſugar difficult, as to be employed for making ſugar with extraordinary profit. On this account, I give to ſuch economiſts as may cultivate the runkelrübes for that purpoſe the well-meant advice, to beſtow the moſt careful attention on its culture, and to follow the method pointed out in this treatiſe; becauſe they would otherwiſe be greatly diſappointed by obtaining a ſweet mucilage of a bad taſte inſtead of cryſtals of ſugar. I ground this aſſertion on my own experiments, as well as thoſe of others acquainted with this ſubject. And for this reaſon, the proper culture of the runkelrübe may be conſidered as the baſis of its uſeful application in the profitable manufacture of European ſugar; ſince from roots of this kind whoſe ſaccharine matter has not been greatly increaſed by proper cultivation, no ſugar can be obtained to advantage. Sugar may, indeed, be prepared from roots cultivated in a different manner from mine; but not with profit, though the loſs may prove variable. This fabrication of ſugar would thus continue, what it has long been, a ſubject of ſcientific reſearch; in the ſame manner, as the preparation of ſugar from green-peas, cabbage-leaves, melons, and many fruits, is, even now, and will probably remain, an object of ſuch inquiries.

25. The aſſertions here deduced from experience will eaſily admit of a theoretical explanation. And, in particular, according to the theory given by the worthy counſellor of the mines, *Hombolt*, in his *Aphoriſms*, which is founded on the moſt diligent obſervations, and a well-ſtudied phyſiology of plants: this learned man proves that the ſeparation of oxygen from plants is prevented by the abſence of the matter of light. But oxygen is neceſſary to the formation of any acid, and the formation of acid is required to the production of ſaccharine matter. From this it is evident, that the influence of the matter of light, as a means of ſeparating the oxygen, is, and muſt be, injurious to the formation of ſaccharine matter in ſuperabundance. On the other hand, it follows from the ſame principle, that the oxygen retained in larger quantities in the root by all poſſible excluſion of the matter of light, muſt, of courſe, contribute towards increaſing the ſaccharine matter *.

26. This application of the runkelrübe to the making of ſugar in the large way, with variations in the products, according to its different culture, has already been perfectly confirmed by the experiments which I am now making, under the inſpection of the royal committee. Of the runkelrübes from Magdeburg and Halberſtadt, only thoſe have yielded ſugar which (1.) were of a ſmall ſize, and conſequently had grown cloſe to one another; (2.) thoſe had not large, rounded heads, but whoſe heads were buried under ground, and had been thoroughly covered with earth; (3.) thoſe which had flat heads, and, therefore, from which the leaves had not been taken off; and (4.) thoſe that poſſeſſed a conical or ſpindle ſhape;

* A contradiction ſeems to prevail in this place; as the matter of light rather promotes the *increaſe of ſaccharine* matter in the *fruits of trees*.—The reaſon of this muſt be ſought for in the different organic ſtructure of all the parts of plants, and in the conſequent elaboration of their conſtituent parts; which, of courſe, muſt be different in the *eſculent fruits* from that of the *roots* and their *germs*. Hence, the preſence or abſence of the matter of light muſt exert a different influence on the various parts of plants—namely, roots, leaves, fruits, &c. &c.—with regard to the formation of different parts in greater or ſmaller quantity.

which

which confequently had not been tranfplanted, but had received their full growth in the places where they germinated from feed.

On the contrary, thofe roots from Magdeburg and Halberftadt afforded only pulp and ill-tafted fyrup, in which no cryftals of fugar were formed. 1. Thefe were ftrong and thick, and confequently grew at a greater diftance from each other, by which they increafed in bulk. 2. Their heads were large, and the marks where the leaves had been cut off were diftinctly perceivable by the ftalks which remained. 3. They poffeffed the round figure which is the effect of tranfplantation; and is very feldom obtained even by the ufe of feeds obtained from tranfplanted roots. This laft event takes place only when the feed is procured through *feveral generations* from tranfplanted roots. For if this has been done only by *one* generation, the variety of a rounded root by that means produced immediately paffes again into a root of a fpindle or long conical form.

27. It remains now to recapitulate the contents of this paper in a few words, with fuch explanations as may be neceffary.

In my method of cultivation, the ground is wholly covered with leaves, and confequently fhaded, by reafon of the nearnefs of the plants to each other; but, on the contrary, thofe roots which have been cultivated to feed cattle, are fown or planted at a much greater, and ufually double that diftance. This very neceffary adumbration is maintained by taking care not to cut the leaves till the roots themfelves are gathered. The accefs of light to the furface of the field, to the great injury of the formation and accumulation of the faccharine matter in the root, is not the only bad confequence; there is another noxious effect, namely, that it promotes the drying of the ground in hot feafons, which is always very detrimental. Moreover, the natural growth of the roots is, by this means, neceffarily difturbed, and cannot be productive of good confequences. Again, if the earth be not removed from the plant, the action of light on the top of the root is checked; and the feparation of the ground, which is done in many places, tends only to increafe the fize of the root. Laftly, by producing the roots from feed which has been fown on the fpot where the plant is to remain, this advantage is obtained, that the root acquires a fpindle fhape, penetrates deeper into the ground, and therefore acquires more fweetnefs, for it is always fweeter in the lower than upper parts. To conclude, the projection out of one part of the root out of the ground, which obtains in the growth of almoft all roots, and efpecially of the runkelrübe, is by this method prevented. The caufe of this prominence confifts in this: that the earth—which it is impoffible to avoid—is loofened on the fpot where the plant is inferted. It finks again on becoming firm, and hence the upper part of the root becomes prominent. Another and the principal caufe of this effect arifes from the circumftance, that on tranfplanting the plant, either the point of the root is taken off, or, on account of its tendernefs, unintentionally injured. Nor does it again acquire an upright pofition; fo that, for all thefe reafons, it cannot proceed in growing downwards perpendicularly. Whence the root does not continue in the flender conical form which it had, and would have preferved, if it had not been tranfplanted; it then forms a more roundifh and nodular root, which in the progrefs of its vegetation (not being able to fpread

ſpread downwards from the too great reſiſtance of the ſoil) riſes upwards, and protuberates more or leſs from the earth, in proportion to its ſtronger or weaker growth. Thus circumſtanced, the prominent part becomes ſo modified in its conſtituent parts by the action of light, that it not only yields leſs of ſugar, but alſo adulterates the ſaccharine matter, copiouſly contained in the lower part, with ſo many noxious principles, that the preparation of ſugar from the root is rendered very difficult, and ſometimes even impoſſible.

28. From theſe conſiderations we may deduce the reaſon why the manufacturing of ſugar in our country has not ſucceeded before, though it might be carried on in the large way with a real and laſting profit, notwithſtanding the low prices of the raw ſugar from India. The cauſe conſiſts merely in the want of an acquaintance with the various modifications under which the runkelrübe ought to be cultivated, if intended for the manufactory of ſugar to a decided advantage, as has been ſeveral times mentioned. Before this time, and before the diſcovery of the means of increaſing the ſaccharine matter in this kind of root, enquirers could only prepare ſugar from the runkelrübe and the various ſpecies of the *beta vulgaris Linnæi* with a ſcientific view, rather than for purpoſes of public utility.

To aſcertain the proper method of culture, by which this root might be produced abounding in ſugar, and deficient in mucilage, was therefore the only expedient to obtain the latter advantage to the public. And it is only from ſuch a root, highly abundant in ſaccharine contents, that raw ſugar can profitably be made, and freed from its mucilaginous and other parts, by means of a very ſimple, and not in the leaſt expenſive, proceſs, of preſſure and cryſtallization; which I ſhall hereafter communicate.

III.

On the Decompoſition of the Acid of Borax, or Sedative Salt. By LAURENCE DE CRELL, *M.D. F.R.S. London and Edinburgh, and M.R.I.A. Tranſlated from the German.*

(Concluded from page 257, Vol. III.)

EXPER. XXXIV. To obviate the objection, that ſedative ſalt alone would perhaps deflagrate with melted nitre, I made that experiment alſo, but in vain. Not the ſmalleſt deflagration took place, even when both were melted together for many hours.

Exper. XXXV. Another objection may be made, namely, that in diſtilling the muriatic acid from manganeſe, part of the latter had paſſed over with the acid; and, in the frequent diſtillations of the ſedative ſalt, had been depoſited upon it, and thus deflagrated. But, on throwing freſh pulverized or ſolid manganeſe, either ſuch as is uſually ſold, or quite pure, heated to redneſs, into melted nitre, not the ſmalleſt deflagration took place.

Exper. XXXVI. to L. Inſtead of the interrupted heat uſed in the foregoing experiments, I now expoſed half an ounce of the ſalt, with three ounces of the oxygenated muriatic acid, to a continued heat, of between 200° and 300° of Fahrenheit. The fluid had nearly evaporated

in

in twenty-four hours. I changed the phial, towards the close of the operation, for another, that the former might be gently heated, and the fluid, by that means, be poured back, with the greater safety, upon the warm salt, through the tube of the retort. In this manner, during an uninterrupted fire of fourteen days, the acid was fourteen times distilled, and returned upon the salt. On the third day, yellow spots appeared. On the fourth, some particles of oil or fat were discovered, swimming on the surface of the fluid in the phial; which particles, after cooling and emptying the phial, adhered to its sides, so as to obscure its transparency. More or less of these oily particles were discovered in every successive operation; and the oily matter, adhering to the inside of the glass, increased considerably.

Exper. LI. When the fluid was distilled, the receiver was changed, and the fire increased. A considerable quantity of sublimate was obtained, pretty white in colour, as was likewise the surface of the mass of salt at the bottom of the retort; but lower down, it was almost of a light ash-grey. After the sublimate ceased to arise, I diminished the fire.

Exper. LII. Upon the mass of the former experiment, I poured the fluid obtained by *Exper.* XLIX. and continued a gentle digestion. I very soon perceived something rising towards the surface, and swimming upon it: after some hours, it appeared to be a thick wrinkled skin, like fat, or a skin of mould, increasing in size, until it covered the whole surface. White spots of sublimate appeared upon it, but it did not sink. It assumed gradually a fine lemon colour; and some yellow matter, though not in large quantity, ascended the sides of the retort. The fluid having been gently distilled, and the receiver changed, I placed the retort in an open fire; on which more sublimate soon appeared; but, not long after, it all vanished, and the retort lost its transparency. The mass contained in it began to rise, first gently, and then violently, especially in the centre, in large frothy bubbles. The distillation was finished, after obtaining one dram of fluid, and when the frothy bubbling had ceased. The retort being broken, that part where the bubbling had been strongest was found to be black; the upper surface being covered with a thin greyish matter, under which a solid, compact, and almost vitrified substance appeared. Upon this I poured water, and dissolved it in the usual manner; filtered it, let it evaporate, and treated it as described above, *Exper.* XXII.—XXX.

Exper. LIII. I obtained a white salt, *a*, and some coal, *b* (which deflagrated briskly with nitre), in nearly the same proportions as throughout the series of experiments described from *Exper.* XXII. to XXXIII. which I will not repeat, on account of the little variety observed in them; one of them, however, deserves to be distinguished from the rest.

Exper. LIV. I put six grains of the coal, *b* (of *Exper.* LIII.), in three drams of common muriatic acid, and digested them for two days, till the acid had gradually evaporated. I then added one dram of the same acid, with one scruple of nitric acid, and, when they had evaporated, boiled the residuum full half an hour in distilled water. By this process I obtained a red solution; and having saturated it with mild alkali, a sort of skin rose to the surface, with

ſome ſmall pieces of a fat ſlippery ſubſtance, *a*. A conſiderable quantity of looſe earth was alſo precipitated, of a light brown colour.

Exper. LV. On throwing the floating pieces, *a* (*Exper.* LIV.), into a ſolution of cauſtic alkali, they diſſolved; the ſolution had a reddiſh-brown colour.

Exper. LVI. With the ſame ſolution of cauſtic alkali I covered the light brown earth of *Exper.* LIV. As the ſolution changed its colour to a reddiſh brown, the earth gradually became perfectly white.

Exper. LVII. To obſerve the affinity of other acids to the ſedative ſalt, I poured ſix drams of nitrous acid upon two drams of the ſalt, with ten drams of the forementioned oxygenated muriatic acid; digeſted the mixture, and diſtilled it, in twenty-four hours, with a gentle heat. Upon the fluid ſwam a white compact ſubſtance, and ſome ſmall particles of the ſame kind lay at the bottom, which however roſe on the application of heat, and ſwam about with the reſt.

Exper. LVIII. to LXIII. I poured the whole diſtillation back upon the ſalt, and, by means of a digeſting heat, again drew off a fluid, which appeared covered with a thin fat ſkin. I then poured the fluid back, diſtilled it again, and thus repeated the proceſs three times more. No phenomenon particularly remarkable appeared, except that the thin fat ſkin grew more inconſiderable, and at laſt ſeemed almoſt to vaniſh.

Exper. LXIV. The ſalt ſeparated from the fluid by the gentle diſtillation in *Exper.* LXIII. emitted now, by the force of additional heat, dark red vapours, as is uſual in ſtrong nitrous acid. When the diſtillation was at an end, the retort was expoſed to an open fire; but, during this operation, no black matter appeared; nor was any coal ſeparated from the maſs, upon diſſolving it in diſtilled water*.

Exper. LXV. I now tried the effect of a mixture of four drams of ſtrong vitriolic acid and twelve drams of the muriatic acid, repeating the uſual digeſtion and diſtillation ſix times. I will paſs over other circumſtances, and only mention, that after the ſixth diſtillation of the fluid, a ſtronger heat, and at length an open fire, was applied; but hardly any fluid was produced, though the fire was ſo violent, that the whole maſs appeared to be melted down into one uniform compact ſubſtance.

Exper. LXVI. The veſſels having cooled, the maſs was of a light milky colour throughout, without the leaſt mixture of brown or black, or any other indication of coal †. Being ſome time expoſed to the air, it became moiſt, and for a long time attracted much water, which I cauſed to run off. At laſt it remained pretty dry; but the maſs ſeemed to have diminiſhed, by at leaſt one-fourth part.

Here I will ſtop, for the preſent, in the deſcription of my experiments, which ſufficiently tend to prove, in a general way, the decompoſition of ſedative ſalt, and to ſhow, that one of

* Here the nitrous acid ſeemed to deſtroy, and carry off, the inflammable matter, ſooner than it could become coal; as it had before occaſioned the oily and fat ſubſtance to vaniſh, in the beginning of this experiment.

† Perhaps here alſo the remark contained in the former note holds good: yet I am rather of opinion, that the vitriolic acid did not operate with ſufficient ſtrength to ſeparate the component parts.

its

its component parts is inflammable matter, which may be converted into coal. I obtained of true coal (mixed with fome earth, *Exper.* XXXII. and LIV.), according to the above-defcribed experiments (*Exper.* XXII. XXVI.—XXX.), thirty grains and three quarters, in the whole; and by other experiments, often repeated, in general, one grain and a half, more or lefs. Every other fubftance liable to be changed into coal (as gum, tartar, fugar, &c.) fuffers this change by a gentle heat, and deflagrates with nitre, in the degree of heat neceffary to melt the former. But fedative falt can bear a red heat for many hours, without fhewing any figns of becoming coal, of burning, or of deflagration. Aftonifhing phenomenon! What menftruum preferves it fo fecurely againft the affault of force, in a diffolved ftate, and yet fuffers itfelf to be feparated from it by more gentle means? What power exifts here, to protect the inflammable particles (which afterwards turn to coal) fo effectually againft a degree of heat which nothing elfe can refift? Of what nature is the falt obtained in conjunction with the coal? Thefe are all queftions which excite great intereft, but which are not eafily anfwered. How far I have been fuccefsful in refolving them, fome fubfequent effays will fhow; which I fhall have the honour to lay before the Royal Society, as foon as I fhall have fufficiently repeated the experiments I have already made.

IV.

Obfervations on the Means by which the Mountains in the Cevennes are rendered fertile. *By* CIT. CHAPTAL.

INDUSTRY is the child of want: this axiom, of which the truth is eftablifhed on the experience of every age and climate, directs our fearch after prodigies of agriculture to thofe places only which appear to be abfolutely deprived of natural advantages. In no place has this truth received a more ftriking degree of confirmation than in the dry and barren chain of mountains called the Cevennes. They are almoft entirely formed of fteep rocks; but the power of man has fucceffively converted them into fertile lands; and this foil, which in paft ages would not have afforded fubfiftence for one family of favages, does, at this moment, fupport two or three hundred thoufand inhabitants. In this diftrict, every thing is the product of art; and here it is that we may, with the greateft effect, ftudy the creative powers of human induftry. I fhall here relate the means by which thefe changes have been made.

I fhall confine myfelf at prefent to the defcription of two proceffes, which are ftill daily practifed, and which may hereafter be adopted with advantage in many parts of France.

It is a well-known fact that the waters which flow down the fides of a mountain carry the earth along with them, and wear furrows or ravins of a greater or lefs depth, according to the hardnefs of the rock, and fteepnefs of the defcent. Thefe two effects are conftant and inevitable. By a feries of thefe progreffive degradations, the hardeft rock is laid bare; deep

ravins are cut in its face, and every reſource of which the cultivator might avail himſelf is utterly deſtroyed. The inhabitants of the Cevennes have found means to correct this double effect of the waters; and to reſtore, by proceſſes no leſs ſimple than ingenious, the grounds which had been loſt.

I ſhall firſt deſcribe the method of filling the ravins, and converting them into fertile land; and afterwards ſpeak of the method of covering the bare flanks of the mountains with vegetable mould.

The firſt Proceſs.

To fill up a ravin, they begin by raiſing, at the very foot of the mountain, a wall of looſe ſtones quite acroſs the ravin, and two or three metres (or yards) in height, towards the middle, according to the depth of the ravin itſelf. This wall forms a kind of dyke, which oppoſes its flank to the current of the waters, and ſuffers them to paſs through while they are clear; but when, after a ſtorm or heavy ſhower of rain, they become turbid, and bring down earth and ſtones, theſe ſubſtances are depoſited againſt the wall while the water eſcapes between the ſtones nearly pure. By the continuance of this proceſs, the triangular ſpace above the wall at length becomes filled.

At the other extremity of this plat of newformed ground, another wall is then built like the firſt, which in the ſame manner detains the earth and vegetable mould, and forms a ſecond piece of ground. By a ſucceſſion of ſimilar operations, other platforms are produced; and by this ingenious proceſs, the ravin is converted, to the very top of the mountains, into a number of platforms of good ground, forming ſteps one above the other. Under theſe circumſtances, the waters no longer ruſh in deſtructive torrents down the ſides of the mountains, but flow gently along the level ground, or are filtered through the porous earth depoſited againſt the walls of ſupport. The mountain, which formerly preſented a ſcene of deſolation in every point of view, is thus made to exhibit amphitheatres of vegetable ground capable of the richeſt cultivation.

Thus far the labours of the huſbandman are employed in the conqueſt of natural difficulties; but this being accompliſhed, he directs his attention to obtain the products of the ground. In the former inſtance, he claims our admiration; in the latter, we feel an emotion of gratitude for benefits conferred. He plants the vine againſt the upper part of the wall, and cauſes it to creep along the external ſurface, that it may not uſeleſsly employ a ſpace deſtined to other purpoſes. On theſe ſmall platforms he plants mulberry-trees; he cultivates Indian-corn, potatoes, every kind of grain; and varies his culture to the greateſt advantage on this virgin ſoil, which is well watered, and in general of the higheſt fertility. Theſe vines, theſe trees, and other vegetable products, give firmneſs to the ground, and bid defiance to the floods of ſubſequent times. Seldom indeed does it happen that this valuable work of human ability is conſiderably damaged by the fury of the tempeſt.

The

The ſecond Proceſs.

The induſtry of the inhabitant of the Cevennes is no leſs ſurpriſing, when his exertions are directed to give fertility to the ſlope of a calcareous mountain. Moſt of theſe mountains are formed by beds of ſtone about a demi-metre, or half a yard, in thickneſs. Theſe different ſtrata form ſhelves one above another, according to the inclination of the mountain. But the cultivator gives an equal width to all theſe ſtages by breaking away the rock, the fragments of which he employs in conſtructing a low wall on the edge of the platform itſelf. He fills this cavity with a bed of vegetable earth taken out of the clefts of the rock, or conveyed upon his back from the very foot of the mountain, where it has been gradually depoſited by the waters. In this manner, after unremitted labour, the ſide of the mountain becomes covered with low walls parallel to each other, which confine beds of vegetable earth from one to three yards in width.

It often happens that theſe beds of earth are carried away, and the walls overſet, in conſequence of a violent wind, or ſtorm of rain. On theſe occaſions, the determined ſpirit of the cultivator is ſhewn in repairing the miſchief. The life of the inhabitant of the Cevennes is one continued ſtruggle againſt the elements, which ſeem to conſpire againſt his efforts. I was acquainted at St. Jean de Gardonenque, with Cit. Peſtre, an induſtrious man, an enlightened cultivator, and ſkilful phyſician. This man, at the firſt threatenings of a ſtorm, clothed himſelf in a long garment of oil-cloth, with an enormous hat of tinned iron, firmly fixed by means of ſtraps. Thus defended, he haſtened into the midſt of his poſſeſſions, where alone, with a mattock in his hand, he directed the water to the feet of his trees, and collected the ſurplus in cavities which he had formed in the rock itſelf. By theſe laborious exertions he conſtantly prevented inundations, and procured water for his grounds at ſuch times as the burning heats rendered them neceſſary. His neighbours, who, as uſual in ſuch caſes, at firſt derided his extraordinary ſolicitude, were conſtrained to admire his induſtry, and envy his gains. They have all admitted to me, that by this labour, of which few of them are capable, he quadrupled the uſual product of his grounds. Such inſtances of prodigious agricultural exertion are not unfrequent in the Cevennes; but my preſent object being to ſpeak of general methods, it is ſufficient that I have deſcribed the ingenious method of giving fertility to a mountain. We cannot avoid feeling a ſentiment of admiration, and even perſonal vanity, when we conſider one of theſe mountains converted by the hands of man from a ſtate of abſolute barrenneſs to a high degree of fertility, and covered from its baſe to its ſummit with trees, fruits, grain, and other uſeful productions.

He who doubts the extenſive powers of labour and induſtry in the affairs of agriculture, needs only to viſit the Cevennes to become convinced of his error*.

* This practice of converting the ſides of mountains into platforms for the purpoſes of agriculture is common in China, and gives a ſingular aſpect to the country around Canton. But it did not appear to me that the practice was directed to the ſame object, namely, that of giving fertility to barren mountains. On the contrary, theſe laſt were left in a ſtate of nature.—N.

Account

V.

Account of certain Experiments and Inferences respecting the Combustion of the Diamond, and the Nature of its Composition. By CITIZEN GUYTON.*

THAT the diamond is a combustible body was long ago conjectured by Newton, from its strong attraction to the rays of light, and this truth has since been verified by many experiments. With regard to the nature of this gem, the first experiments of Guyton, published in 1785†, on its total combustion in fused nitre, seemed to shew that it was similar to charcoal, because it left an effervescent alkali; this suspicion was rendered still more probable by the examination by Lavoisier of the gas remaining after burning it in close vessels, which proved to be carbonic; and very lately Mr. Tennant has given a new verification of this important fact, by repeating the combustion of the diamond in a crucible of gold, as Guyton had pointed out in order to obtain a residue perfectly exempt from foreign matter ‡.

There were nevertheless many reasons for doubting the perfect identity of the diamond and carbone. Independent of the extreme difference of their external characters, the author of the present communication stated several facts to the institute, which shew that the chemical habitudes of the two bodies are not consistent with this identity. For if the diamond were pure carbone, why does it not detonate with the oxygenated muriate of potash, and disoxygenate sulphur, arsenic, phosphorus, and such metals as are sufficiently fixed to determine its combination with oxygen? Why does it not form carburets? or conduct the electric fluid? It is known that the power of aggregation sometimes counterbalances the power of chemical affinity; but this does not happen when the bodies are sufficiently fixed, and the temperature sufficiently elevated to give effect to the weakest attractions of composition.

It remained, therefore, that some discoveries should be made to reconcile and explain facts apparently so discordant §. Our author judged that the explanation was to be sought by a diligent research into the phenomenon of the combustion of this substance, and he hopes that the narrative of his experiments made at the Polytechnic school, in conjunction with the citizens Clouet and Hatchette, in the fifth and sixth republican years, will shew that his expectations were not ill founded.

The first experiment was made on the 9th Fructidor, in the year 5 (August 26, 1797). An inverted vessel of white glass, 18,3 centimetres in diameter, and 5580 cubit centimetres (A. fig. 1. plate 14.) in capacity, was placed on the shelf of a trough containing mercury. Near this trough was fixed an air-pump B, to extract the common air by means of a re-

* Abridged from the communication read at the sitting of the first class of the French National Institute, 26 Prairial, in the year 7 (June 14, 1799); inserted in the Annales de Chimie, XXXI. 72.

† French edition of Bergmann's Treatises, vol. XII. p. 124.

‡ Philos. Trans. 1797; or this Journal, I. 177, 199.

§ Cit. Berthollet, in his Lectures at the Normal School, considers it still as a matter of doubt whether the diamond be crystallized carbone, or that principle combined with some other matter.

curved

curved tube. On the other ſide was a pneumatic trough C, containing water, in which was a large pneumatic veſſel D, communicating with the interior of A, by means of a tube properly bended. The communication could be regulated at pleaſure by the cock E. On one ſide of the trough of mercury was a bar F F, to which a clamp G could be fixed at pleaſure; and through the upper part of this clamp was paſſed a rod of hard wood H, which paſſed beneath the mercury, and roſe again to afford a ſupport for the diamond at I. By altering the ſituation of the clamp G, by elevating or depreſſing the rod H, or by turning this laſt on its axis, it was poſſible to place the extremity I in any required ſituation within the glaſs veſſel A. At I was placed a ſmall porcelain crucible, in which the diamond was placed. This crucible was provided with a ſtopper, which could be put in and out by means of a wire.

The diamond which was the ſubject of this experiment was an incomplete octahedron, of a dull water, inclining to yellowiſh-grey, ſomewhat rounded on the edges. It weighed exactly 142 milligrammes. In this ſituation of the apparatus, the crucible being cloſed, the pump was worked until the mercury roſe to within leſs than one millimetre from the orifices of the communicating tubes, which terminated quite at the upper part of the veſſel at A. The cock E was then opened, and oxygen gas, obtained from the oxygenated muriate of potaſh, and previouſly put into D, was paſſed into A. The firſt portions of this gas which mixed with the minute reſidue of common air at A, were drawn off by the air-pump, in order to exhauſt the common air as much as poſſible; and at laſt the veſſel A was filled with the oxygen gas, which might be conſidered as nearly pure.

The ſolar heat was applied to the diamond by means of the great lens belonging to the Polytechnic ſchool, the diameter of which is 40,59 centimetres, and its focus 135,3. The effect of the ſudden heat upon the glaſs veſſel was moderated by occaſionally intercepting the rays with paper for a few inſtants, after which the diamond being expoſed to the focus for twenty minutes did not take fire. It appeared at firſt farinaceous, and afterwards perceptibly blackened on its ſurface, when examined through coloured glaſſes, while the focus was upon it. When the focus was intercepted by an opaque body, in order to examine the diamond more accurately, the diamond appeared to have undergone no alteration but that of having acquired a yellowiſh ſhade, perfectly reſembling clear amber. At this period the experiment was diſcontinued, on account of the weather becoming obſcure.

On the following day the ſolar focus was thrown upon the diamond, and at the end of one hour and fourteen minutes, the cone of light being intercepted, the diamond was ſeen diſtinctly; tranſparent and ſurrounded by a weak radiation. When it was cold its edges appeared to have been ſoftened. A black point was viſible; but the diamond had recovered its white colour by the loſs of the yellow tinge it had acquired the day before.

The experiment could not be reſumed till the 15th. On this day the weather was unfavourable, but nevertheleſs a ſlight ſcintillation appeared.—To the aſtoniſhment of the operators, the diamond did not continue its own combuſtion, which even metallic ſubſtances do when ſufficiently heated in oxygen gas. This ſeemed the more remarkable on account of the contrary

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having been announced by Landriani. To determine whether the aggregation of the ſtone were the cauſe of this, another diamond was ſubjected to experiment, ſcarcely exceeding the eighteenth part of the former; but this gave no ſign of inflammation, nor underwent the leaſt change.

On the 23d the diamonds were taken out and carefully examined, when the larger was found to have loſt 54 milligrammes, or 0,38 of its weight, having preſerved its original form, though the angles were blunted, and its ſurface tarniſhed and full of ſmall inequaliti s, which under the magnifier preſented either pointed projections, or the ſections of parallel plates. In ſeveral of the cavities were ſeen a greyiſh kind of ſpot; and, in particular, in a cavity ſomewhat larger than the reſt, where the focal ſpot appears to have been moſt ſtrongly active, there was a blackiſh ſtreak not terminated, as if drawn externally, but fuſed, and penetrating by a degradation of colour into the body of the ſtone.

This diamond was preſerved for inſpection, and another of a clearer water than the former, of an octahedral form, and the weight of 200,1 milligrammes, was expoſed to the focus of the great lens of Tſchirnhauſen, which is 32 French inches in diameter, and 73 inches in its focal length: but the focal length was ſhortened by the interpoſition of another lens. The firſt experiment ſcarcely exhibited any ſigns of combuſtion, and failed by the breaking of the receiver. In the following year, 6, it was therefore found expedient to reſume the proceſs with a large globe, in order that its ſurface might be at a ſufficient diſtance from the denſer part of the luminous cave. The globe made uſe of meaſured 28,63 centimetres in diameter, and its capacity was 12325 cubic centimetres; and, in order to determine the quantity of ſpace occupied by the riſing mercury, there was a piece of paper paſted on the outſide, having marks thereon to expreſs every 100 cubic centimetres, determined by pouring in water by a meaſure of that magnitude.

As it was impracticable to exclude the air from ſo large a veſſel of glaſs, by means of the vaſt quantity of mercury it could contain, our operators had recourſe to an ingenious expedient derived from the conſideration that the ſpecific gravity of oxygen gas is about one ninth part greater than that of common atmoſpheric air. It conſiſted in admitting the gas by a tube which reached from the neck (then turned upwards) to the lower part of the inſide, while the common air was ſuffered to eſcape through another ſhort tube in the cemented cork by which the orifice was cloſed. It may eaſily be imagined, that the oxygen did not diſplace the common air without mixing with it and becoming more or leſs contaminated with azote; for which reaſon it was neceſſary that the contents ſhould be thus diſplaced for ſeveral ſucceſſive times. By this treatment the laſt portions of gas extruded were found to be pure, and the veſſel continued perfectly clean and dry. The authors give a detail of the methods by which the oxygen gas was obtained, and its purity aſcertained.

Fig. 2 repreſents the globular veſſel inverted over an iron mortar, containing mercury, and ſecured in its place between two pieces of wood, which formed a cavity to receive it. The diamond was previouſly ſecured in a ſmall veſſel of baked clay, by means of a ribband, which was afterwards ſlipped off when the veſſel was inverted. There was a ſmall tube of commu-

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nication through the cemented cork to the mercury, and through this tube a portion of the gas was drawn in order that the mercury might ſtand a ſmall elevation within the veſſel.

The diamond which had, by the former treatment, loſt a ſmall part of its weight, now weighed 199,9 milligrammes, or one 32d part leſs than a jeweller's carat. On the 5th of Fructidor, in the 6th year, the experiment was re-commenced by directing the focus of the great lens of the Inſtitute upon this diamond, the value of the oxygen gas having been previouſly determined in cubic centimetres, and reduced to a known temperature and preſſure.

The diamond at firſt exhibited a black ſpot at the angle immediately ſtruck by the ſolar rays, after which it became entirely black and of a coaly appearance. An inſtant afterwards brilliant and, as it were, boiling ſpecks were diſtinctly perceived on the black ground. The ſolar rays being then intercepted for a moment, it appeared of a tranſparent red; and afterwards, when a cloud obſcured the ſun, the ſtone was ſeen of a much clearer colour than at the commencement of the operation.

When the ſun reſumed its force the ſurface of the diamond aſſumed the appearance of metallic brilliancy. It was then perceptibly diminiſhed, ſcarcely one fourth part remaining; its form oblong, without marked angles or edges, but ſtill very white and of a beautiful tranſparency.

A ſlight crack appeared at the bottom of the veſſel which ſupported it, but without any ſeparation of parts.

At the commencement of the combuſtion a purpliſh cone was thought to be perceived riſing from the ſupport in the cone of ſolar rays; but this phenomenon, according to the writer, was merely an optical effect, depending on the poſition of the obſerver.

The apparatus was left in its place, without alteration, till the ſeventh, when the ſolar focus being again thrown upon the diamond, the ſame appearances were obſerved; namely, blackneſs at the ſurface, and brilliant boiling points, which diſappeared and re-appeared, according to the power of the ſolar light. An appearance of metallic brilliancy was alſo ſeen, or rather a leaden colour, according to the expreſſion made uſe of by one of the aſſiſtants to characterize this phenomenon.

The diamond was entirely conſumed after twenty minutes expoſure this ſecond time. After a careful examination of the ſupport, which was withdrawn, a ſolution of barytes was introduced above the mercury. It immediately became milky, while the volume of the gas was diminiſhed. This barytic water was then, for the moſt part, withdrawn by means of the inverted bottle (fig. 3.) filled with mercury, which was raiſed into the globe by means of an iron ſtem, conſiſting of ſeveral pieces ſcrewed together. A portion of the diſtilled water was alſo introduced to waſh out the remaining precipitate; and this was afterwards added to the barytic water. It was neceſſary to add more of the barytes, in order to ſaturate a portion of carbonic acid, which was more in quantity than the operators expected. The valuation and reduction of theſe quantities, and the precautions made uſe of, are given in the Memoir. The reſult was, that inſtead of 28 parts of combuſtible ſubſtance, with 72 of the acidyfying principle,

as obferved in the combuftion of charcoal, the proportion in the combuftion of diamond was 17,83 of carbone, and 82,12 of oxygen.

The learned reporter proceeds to offer his remarks upon the theory of thefe facts. Though he could not doubt the facts, he, at firft, felt a degree of repugnance to admit fo confiderable a difference in this combuftible and charcoal, the latter of which contains little more than half the quantity of oxidable matter, which is capable of being burned at a temperature prodigioufly below that which is required by the diamond. But, upon reflection, he foon obferved that this is not the firft example of acidifiable bafe, the firft degree of oxidation of which is effected with extreme difficulty, though the fubfequent acidification is very eafy. 2. That many fubftances of the fame genus, likewife, prefent thefe two characters; namely, a greater abundance of true carbone, accompanied with a greater refiftance to inflammation. On thefe heads, he remarks how difficult it is to form a commencement of union of azote and oxygen in the direct way, and the high temperature it requires; whereas nitrous gas cannot be brought in contact with oxygen, without immediately paffing to the acid ftate. Charcoal will therefore be, with regard to the carbonic acid, what nitrous gas is to the nitric acid; and the diamond will be to charcoal, what azote is to nitrous gas. It will not, therefore, appear furprizing that more oxygen is required to combine with a fubftance which has yet none of that principle, than with a fubftance which has taken up the quantity which is neceffary for its firft point of faturation.

The fecond confideration refts on facts no lefs conclufive. Plumbago is a carbonic combuftible which does not burn at a very elevated temperature, or in nitre in fufion. It produces carbonic acid, and, like the diamond, is richer in combuftible than charcoal itfelf. The incombuftible coal, which was defcribed by Guyton in the Dijon Memoirs for 1783, is a fubftance of the fame kind, as is alfo the anthracolite; and the celebrated Klaproth has likewife eftablifhed by experiments, that a foffil defcribed by Widenman, under the name of incombuftible coal, is of the fame nature. The Kilkenny coal, defcribed by Kirwan as poffeffing the metallic brilliancy, and refufing to burn, unlefs in a ftate of ignition, is alfo capable of decompofing 9,6 of nitre. All thefe are confidered as true oxides of carbone, which, like charcoal, poffefs the property of conducting the electric fluid, of cementing iron, of depriving certain acidifiable bafes of oxigen, but are not fufficiently oxided to exert this divellent affinity at a low temperature.

No good account has yet been given, why certain animal and vegetable matters afford coals of fuch difficult incineration? Why charred pit-coal, known by the name of coke, though half burned in its preparation, is neverthelefs fo powerful in combuftion? Why turf, the weakeft of all fuel, acquires, by a good carbonization, the property of welding large pieces of iron better than charcoal? and why, laftly, charcoal itfelf, after expofure to very ftrong fire in a veffel impenetrable to the air, becomes to a certain point incombuftible? The anfwer to all thefe queftions is referred, by our author, to the theory he has developed; that thefe are all carbones at the firft degree of oxidation, having been deprived of part of their oxygen by the elevated temperature to which they have been expofed. Hence he concludes,

cludes, that practical conſequences will, no doubt, be deduced for the reduction of metals; the cementation of ſteel, which probably takes up only the oxide of carbone, ſince it is ſeparated in this ſtate*; for the incineration of the coaly reſidues of our analyſes; for the carbonization of wood, pit-coal, and turf; and laſtly, for the uſeful application of mineral coals, which, though difficult to be burned, are capable of affording a ſtrong and laſting heat when mixed with a due proportion of other materials which burn more readily.

The general recapitulation of facts and inferences, in the words of the author, are as follow:

1. It is not merely by its colour, weight, hardneſs, tranſparency, and other ſenſible qualities, that the diamond differs from charcoal, as has hitherto been thought.

2. Neither does the conſtitution of the diamond depend ſimply on its ſtate of aggregation.

3. Nor do the diſtinctive properties of charcoal depend on the two hundredth part of reſidue, which it leaves in the form of aſhes, nor the ſmall quantity of hydrogen which it contains.

4. But the moſt eſſential difference conſiſts in the chemical properties.

5. The diamond is the pure combuſtible ſubſtance of this genus.

6. The product of its combuſtion, or combination with oxygen, carried to the point of ſaturation, is carbonic acid without reſidue.

7. Charcoal burns at a temperature which may be eſtimated at 188° of the centigrade thermometer †. The diamond does not burn but about pirometric degrees, which, in the ſyſtem of Wedgwood's ſcale, affords a difference between 188 and 2765.

8. Charcoal ſet on fire in oxygen gas, does itſelf maintain the temperature neceſſary for its combuſtion; but the combuſtion of the diamond ceaſes, as ſoon as it is no longer maintained by the heat of the furnace, or the concentration of the ſolar rays.

9. The diamond requires, for its complete combuſtion, a much greater quantity of oxygen than charcoal does, and likewiſe produces more carbonic acid. For one part of charcoal abſorbs 2,527 of oxygen, and produces 3,575 of carbonic acid. But one part of diamond abſorbs ſomewhat more than four of oxygen, and really produces five of carbonic acid.

10. There are ſubſtances which exiſt in an intermediate ſtate of compoſition between the diamond and charcoal. Theſe are plumbago, or native carburet of iron; the incombuſtible foſſil coal; the carburet of alumine of Dolomieu; the anthracolite of Werner; the black matter united to iron in the ſtate of caſt-iron and ſteel; the coaly reſidues of difficult incineration; and charcoal itſelf unburned by the action of a ſtrong heat, without the contact of air.

11. Theſe ſubſtances, mixed or weakly combined with three or four hundred parts of

* It is a ſtrong fact in favour of this theory, that the charcoal which has been uſed for the cementing proceſs is rendered incapable of being again uſed for the ſame purpoſe. See Duhamel, in the Encyclopédie Méthodique, art. Acier.—N.

† Dictionnaire de Chimie de l'Encyclopédie, method. I. 714.

their weight of iron, or alumine, afford, by their combuftion, carbonic acid like charcoal, and the diamond.

They approach charcoal by their colour, their lightnefs, and their opacity. Like that fubftance, they decompofe water, cement iron, difoxigenate metals, fulphur, phofphorus, and arfenic ; and, like charcoal, they conduct the electric fluid.

They approach the nature of the diamond in containing much more combuftible matter than charcoal, which is manifefted by their abforbing more oxygen, and producing more carbonic acid ; in their decompofing more nitrous acid ; in their refufing to burn, even in fufed nitre, unlefs at a more elevated temperature ; and by their combuftion ceafing when that temperature is diminifhed.

They appear to differ from each of thefe bodies, by their property of producing the Galvanic irritation with zinc, as well as filver does ; an effect which is not produced either with the diamond, or with charcoal.

12. The diamond, therefore, is pure carbon, the pure acidifiable bafis of the carbonic acid.

Its combuftion is effected in three terms, which require three different temperatures.

At the firft and moft elevated temperature, the diamond affumes a black leaden colour. This is a firft degree of oxidation ; it is the ftate of plumbago, and the anthracolite.

At the fecond temperature, which may be eftimated at eighteen or twenty pyrometric degrees, there is a new, flow, and fucceffive combination of oxygen. It is a progrefs of oxidation, which conftitutes the habitual ftate of charcoal, or rather that in which it is found after the action of a ftrong heat in clofed veffels has difengaged part of its oxygen.

Plumbago is, therefore, an oxide of the firft degree ; charcoal, an oxide of the fecond degree ; and the carbonic acid is the product of the complete oxigenation of carbon.

Suppofing, therefore, that we could operate with fufficient precifion to take from the furface of the diamond the black matter gradually as it is formed, by withdrawing it fuddenly each time, from the action of the folar heat, we fhould undoubtedly convert it into charcoal, or at leaft into plumbago, if the too rapid tranfition of the laft degree of oxidation to oxigenation fhould not permit us to obtain it in this ftate.

13. Laftly, from thefe principles flow many important confequences to chemiftry and the arts.

After this conclufion, it will, no doubt, be demanded, how it happens that the fimple matter, the pure carbon, or diamond, is fo fcarce, while its compounds in different ftates are fo abundantly difperfed. To difpel the aftonifhment of thofe who might confider this a ground of diftruft, I fhall remind them that the aluminous earth is likewife one of the commoneft fubftances, though the adamantine fpar, no lefs rare than the diamond, is neverthelefs alumine ; that iron exifts every-where, under every form, excepting in the ftate of purity ; for the exiftence of native iron is ftill doubtful. The wonder confifts only in the oppofition between facts and our opinions ; it difappears in proportion as we difcover and appropriate the powers of nature to produce the fame effects.

Thofe who have never turned their attention to the philofophical fciences—at leaft, for the

purpofe

purpoſe of eſtimating their influence on public happineſs—are diſpoſed to conſider ſuch reſearches, as are not directed towards an object immediately related to ſome new enjoyment as a matter of vain curioſity. What would have been the aſtoniſhment of ſuch men, if they had been told that reſearches into the nature of the diamond ſhould one day afford truths leading to the happieſt ameliorations in the practice of the moſt familiar arts, and the uſe of the commoneſt combuſtibles. Such, however, are the conſequences which are promiſed by our better knowledge of the eſſential coaly principle in its different ſtates.

VI.

On the apparently ſpontaneous Combuſtion of living Individuals of the human Species. By CIT. LAIR.*

WE find accounts, in ſeveral works, of the combuſtion of human individuals, which appear to be ſpontaneous. They have been reduced, in ſhort, to a maſs of pulverulent fatty matter, reſembling aſhes. Theſe accidents have been accompanied by phenomena ſimilar to thoſe which are obſerved in the proceſs of combuſtion, and this deſtruction has not been produced by the combuſtion of the ſurrounding bodies.

The author of this memoir has collected all the circumſtances of this nature which he has found diſperſed in different books, and has taken care to reject thoſe which did not appear to him to be ſupported by reſpectable teſtimony.

Theſe narratives are nine in number, taken from the Acts of Copenhagen, 1692; from the Annual Regiſter, 1763 and 1775; Philoſophical Tranſactions, 1744; Obſervations of Lecat, in the years 1725 and 1749; and from the Journal of Medicine for 1779 and 1783.

To theſe the author has added ſome others, related by perſons ſtill living at Caen, and on the teſtimony of a ſurgeon of that town, who drew up a verbal proceſs, containing an account of the circumſtances of an event of this kind.

Several members of the ſociety, who were preſent at the reading of this memoir, and had travelled in the North, had frequently heard ſuch accidents mentioned. Doctor Swediaur related ſome inſtances of porters at Warſaw, who, having drank abundantly of malt ſpirits, fell down in the ſtreet, with the ſmoke iſſuing out of their mouths; and the people came to their aſſiſtance, ſaying that they would take fire; to prevent which they made them drink a great quantity of milk, or uſed a more ſingular expedient, by cauſing them to ſwallow urine immediately on its evacuation.

However difficult it may be to give credit to ſuch narratives, it is equally difficult to reject them entirely without refuſing to admit the numerous teſtimonies of men for the moſt part worthy of credit, or attributing to them criminal views; when we reflect on the difficulty of

* Communicated to the Philomatic Society at Paris, and inſerted in the Bulletin, Thermidor, an. 5. No. 29.

reducing

reducing the body of an animal to aſhes, the time and quantity of wood this incineration would require; and eſpecially when we take notice, as Cit. Lair has done, of the reſemblances which exiſts between the circumſtances which almoſt conſtantly precede and accompany theſe ſingular accidents. Theſe circumſtances may be reduced to the nine following facts.

1. The perſons who have experienced theſe effects of combuſtion were generally much addicted to the drinking of extremely ſtrong ſpirituous liquors. It has accordingly been remarked that the inhabitants of the North are moſt ſubject to theſe accidents.

2. They were uſually veryfat.

3. This combuſtion has happened more frequently in women.

4. Theſe women were old.

5. Their bodies did not appear to have been burned by a combuſtion perfectly ſpontaneous; but it appears that the fire had taken place in conſequence of ſome very ſlight external cauſe, ſuch as the fire of a taper, candle, or a pipe.

6. The extremities of their bodies, ſuch as the legs, the hands, or the cranium, eſcaped the fire.

7. Water, inſtead of extinguiſhing the fire of the burning parts of the body, gave it more activity, as alſo takes place in fat that is burnt.

8. The fire very ſlightly damaged, and, in many inſtances, did not injure, the combuſtible objects which were in contact with the body at the moment it was burning.

9. The combuſtion of theſe bodies left a reſidue of oily and fetid aſhes, with a greaſy ſoot of a very penetrating and diſagreeable ſmell.

The author afterwards endeavours to aſcertain the cauſe of ſo ſurprizing a phenomenon. We ſhall not (ſay the editors) follow him in the hypotheſes he advances, as the facts are not ſufficiently numerous nor well known to eſtabliſh a ſatisfactory theory concerning the ſpontaneous combuſtion of human bodies. We ſhall content ourſelves with remarking, that the author appears to attribute this combuſtion to a particular ſtate of the fat produced by ſpirituous liquors, and he founds his opinion principally on the perſons being very corpulent who have fallen victims to theſe accidents, and on the obſervation that has been made, that the parts which were not ſo fat, ſuch as the extremities and the head, have eſcaped. And, laſtly, he ſupports his theory by the well-known fact of the ſpontaneous combuſtion of a mixture of animal ſoot and lintſeed oil, which is a mixture ſimilar to that of a fat body containing charcoal in a very minute ſtate of diviſion.

VII.

*Remarks on the Conflagration of the Odeon. By B. G. Sage, Profeſſor and Director of the firſt School of Mines.**

THE effects of conflagration depend upon the degree of fire; and this laſt depends upon the quantity and nature of the combuſtible matters. A fire is active, rapid, and unextinguiſh-

* Jo de Phyſ. v. 534.

able,

able, if the wood happens to be refinous, or if it be found to contain oil; for the fire partly converts them into inflammable air, which immediately, on the accefs of the atmofpheric air, becomes inflamed, and cannot be extinguifhed by water, which only ferves to extend and direct the flames. Hence it happens that water can have no effect on fuch a conflagration, till the wood becomes converted into charcoal.

It is faid that the fire of the Odeon (at Paris) was firft difcovered in a room near the curtain of the theatre, where it communicated to the decorations and deal frames which fupport them. The great quantity of air which was contained in the hall of the Odeon ferved, for a confiderable time, to maintain the fire which had decompofed a confiderable part of the wood and oil with which it was incorporated, and reduced itfelf into fmoke and inflammable air, which entirely filled the hall. As foon as the atmofpheric air was introduced it mixed with the inflammable gas, and exploded. At that inftant the roof of the Odeon fell in, and the flames burft forth in all parts at once, becaufe the inflammable air occupied the whole capacity of the hall.

The fulphur, which continued to burn in feveral parts of the Odeon three days after the fire, gave rife to a notion that the fire had been caufed by malevolent incendiaries. I was myfelf greatly furprized at the quantity of fulphur which I faw in different parts; but, on examining it attentively, I difcovered that it was perceivable only where there had been a great number of laths, timber, and burnt plafter, as in the corridors, the front of the periftyle of the Odeon, which formed four ftories, and near the fituation of the two ftaircafes of wood which were erected near the middle of the hall.

I obferved that the fulphur fublimed into a yellow duft, and that fome fcoriæ had cryftallized; but the greateft quantity appeared to be in the ftate of calcareous liver of fulphur, blueifh, friable, which took fire with decrepitation as foon as it came in contact with the air. It alfo emitted the fmell of decompofed liver of fulphur; which confirmed me in the opinion that it was calcareous pyrophore, formed by the decompofition of plafter, which (as is well known) is itfelf compofed of fulphuric acid combined with calcareous earth. This acid, by the affiftance of the heat, united with the inflammable principle of the charcoal, and formed the fulphur which was found in the ftate of pyrophorus or carbonated calcareous liver of fulphur in the remains of the fire of the Odeon.

I fhewed, about twenty years ago, that the pyrophore afforded by the coal of turf is owing to a portion of felenite or plafter-ftone contained in the turfs of France. During the carbonization of thefe turfs, the fulphuric acid of the felenite *, combining with the inflammable principle of the coal of the turf, forms fulphur and a calcareous pyrophore.

On vifiting the ruins of the Odeon, I found Monette, the architect of the department and of the Central Bureau, who informed me, that he had obferved, in the ruins of two different theatres deftroyed by fire, the fame fmell of the liver of fulphur, and the fame effects as took place at the Odeon.

* The reader may perceive that M. Sage does not follow the new fyftem of chemiftry.

This

This production of fulphur by the calcination of plafter, in the large way, is a conftant fact, fimilar to that which I difcovered about twenty years fince, and is confirmed by this unfortunate event.

State on the fubftances which were found after the fire of the Odeon.

A mafs of calcinated iron mixed with flate and brick partly vitrified.

Hexagonal tiles and bricks changed by the fire, and mixed with black porous fcoriæ and an earthy yellow friable matter.

A blackifh martial fcoria tuberculated and brilliant in its fracture.

Cellular martial fcoriæ of a reddifh-brown colour, covered with a kind of white compact frit.

Slates partly vitrified.

Calcareous and gypfeous earth impregnated with pyrophorus and coaly liver of fulphur, which gave it a blueifh colour; this earth, though penetrated with water, being taken from under the ruins, emitted a fmell of decompofed liver of fulphur, and acquired a tinge of a violet from the combuftion of the fulphur.

This earth had a flight ftyptic tafte.

Expofed to the flame of a taper the fulphur which it contained burned very vifibly.

A brick which had upon its top and bottom fome of this earth, coloured by the pyrophorus, had been ignited to fuch a degree as to become vitrified.

Plafter partly decompofed penetrated with liver of fulphur, which manifefted itfelf when the nitric acid was poured upon it. A ftrong effervefcence was occafioned by the calcareous earth contained in a difengaged ftate.

An hexagonal tile covered with a yellowifh glafs of lead: this tile was alfo impregnated with liver of fulphur, as was obferved by the application of nitric acid.

Slates partly vitrified and agglutinated, which had affumed a reddifh colour.

VIII.

On the chemical Action of different Metals on each other at the common Temperature of the Atmofphere. By CIT. FABRONI.*

THE peculiar fenfation difcovered by Sultzer, and which is manifefted on the tongue when touched by two metals in mutual contact, though none is excited, when they are applied feparately to this organ, is ranked among the galvanic phenomena. Cit. Fabroni, far from attributing thefe effects to an almoft unknown agent, fuch as the electrical fluid, is of opinion that they depend on a chemical operation, in the fame manner as the fenfation of tafte moft probably does. He endeavours to prove this by a number of obfervations and experiments.

* Communicated to the Philom. Society at Paris, Bulletin, Thermidor VII. No. 29.

He

He has remarked that ſeveral metals, ſuch as mercury, tin, and lead, preſerve their metallic luſtre as long as they are pure; but that their alloys are quickly tarniſhed and oxided; and that the mere contact of two different metals appears to haſten their oxidation. In this manner the alloy employed to ſolder the plates of copper which cover the obſervatory of Florence was ſoon changed into a white oxide, at the place of its cloſe contact with the copper, &c. He thinks, that in this caſe the metals have a mutual action on each other; and that this action, though more efficacious and perceptible when the aggregate attraction of the metals is deſtroyed by fuſion, does not the leſs exiſt in ſolid metals when they touch each other.

If we attribute to a particular fluid, not galvanic, or to the electric matter, thoſe effects which with the quickneſs of lightning are produced on the tongue by two metals brought into contact, it is becauſe we do not recollect that the chemical action is alſo exerted between bodies with the utmoſt rapidity. The ſigns of electricity which are ſometimes obſerved when two metals are ſeparated from contact, are rather the conſequence of that action than the cauſe. For we know that moſt chemical operations change the electric equilibrium of bodies, and conſequently muſt produce the phenomena of electricity. And hence, without entirely excluding electricity from all the facts relating to galvaniſm, Cit. Fabroni thinks this fluid has no part in the ſenſation experienced on touching the tongue with the two metals. This action of the metals in contact is proved by the following experiment:—Cit. Fabroni placed different pieces of metal in glaſſes full of water; one in each glaſs. In other glaſſes he put two different pieces of metal, but he ſeparated the metals by a plate of glaſs. In a third ſet of glaſſes he alſo put two pieces of different metals; but ſo as to touch each other. He did not obſerve any alteration in the metal in the two firſt ſets of glaſſes; but the moſt oxidable metals of the third ſet became loaded with oxide, which was conſiderably increaſed in a few days, and the metallic pieces adhered together very ſtrongly. The quantity of caloric diſengaged during theſe combuſtions is too ſmall to be meaſured; but we may perceive the light which proceeds from it if the eye itſelf be made an inſtrument in the experiment, by holding a piece of ſilver in the mouth, and applying a piece of tin to the ball of the eye. As ſoon as the two metals come in contact, a faint but diſtinct light appears, which vaniſhes in a few moments, becauſe the eye accuſtoms itſelf to this weak ſenſation; but it might be renewed on paſſing the metal firſt over the opake, and then over the tranſparent cornea. The author attributes to a convulſive ſenſation, the flaſh of lightning which ſome perſons imagine they ſee at the moment the two metals touch, when one is applied on the tongue and the other between the gums and upper lip.

The preſence of air is required for the oxidation of the two metals in contact beneath the water. Cit. Fabroni thinks that the air is of uſe in theſe circumſtances, to add oxygen to the water in ſufficient quantity to be received by the metal, as ſilver is added to gold in the parting aſſay.

If a piece of ſilver and a piece of tin are put ſo as to touch each other in water, in a veſſel

of flint-glass, hermetically closed, the pewter is oxided, but the oxide of lead of the flint-glass is decomposed, and the glass becomes black.

The philosophers who have attributed these phenomena to electricity, assert, in support of their opinion, that they take place when the metals are joined by a considerably long chain. Cit. Fabroni has determined the limits of this chain at 6 or 7 metres. Beyond that distance these phenomena are no longer perceptible; on the contrary, the electrical fluid is propagated at unlimited distances.

If the phenomena in the experiments of Sultzer be really produced by electricity, they should take place with all metals, what relation soever these metals might otherwise have to each other. Cit. Fabroni mentions a great number of these combinations, in which no effect was produced, and of other combinations of the same metals which have produced very distinct sensations. Thus, if silver be placed on the eye, gold on the tongue, and they are united by means of copper, the sensation is extremely slight; but, on the contrary, it is very evident, if iron touches the eye, silver the tongue, and copper be used to form the communication.

With regard to the hydrogen of the decomposed water, the author of this Memoir thinks it may also be absorbed by the metal. He even considers the octahedral crystals which he has observed on the surfaces of the pieces of tin he employed in his experiments as the hydrogenated oxide of tin.

"It is evident from these results which I have obtained by the simple contact of metals in water," says Cit. Fabroni, "that is to say, the oxide and saline crystals, that the operation is chemical, and that we ought to attribute the sensations which are felt on the eye and tongue to a chemical cause.. It therefore appears probable to me, that it is to these new compounds and their elements we owe that mysterious stimulus which produces the convulsive motion of the animal fibres in a great part, at least, of the galvanic phenomena."

IX.

On the Harp of Eolus. By MATTHEW YOUNG, *B. D.*

To Mr. Nicholson.

Sir,

OBSERVING a passage in a note at p. 12. of your second volume, respecting the harp of Eolus, in which you offer some conjectures respecting its mode of operation, I have thought it might be acceptable to you and your readers to point out some good experiments on that instrument. They are to be found in "An Enquiry into the principal Phenomena of Sound and musical Strings, by Matthew Young, B.D. Trinity College, Dublin." The work is an octavo of 203 pages, and was printed in London in 1784. I transmit you a copy of as much

much as relates to the Eolian harp, which, from the confined circulation of ſuch works as the original, muſt be known to few of your readers.

I am, Sir,
Your obliged,

September 8, 1799. P. P.

Of the Harp of Eolus.

THIS pleaſing inſtrument, which has been related by ſome to be a modern diſcovery, was, in truth, the invention of Kircher, who has treated largely of it in his Phonurgia. It is an inſtrument ſo univerſally known, that it may well be preſumed unneceſſary to give an account either of its conſtruction or the manner of uſing it.

To remove all uncertainty in the order of the notes in the lyre, I took off all the ſtrings but one; and, on placing the inſtrument in a due poſition, was ſurprized to hear a great variety of notes, and frequently ſuch as were not produced by any aliquot part of the ſtring; often too I heard a chord of two or three notes from this ſingle ſtring. From obſerving theſe phenomena, they appeared to me ſo very complex and extraordinary, that I deſpaired of being able to account for them on the principle of aliquot parts. However, on a more minute enquiry, they all appeared to flow from it naturally and with eaſe.

But before we proceed to examine the phenomena, let us conſider what will be the effect of a current of air ruſhing againſt a ſtretched elaſtic fibre. The particles which ſtrike againſt the middle point of the ſtring will move the whole ſtring from its rectilineal poſition; and as no blaſt continues exactly of the ſame ſtrength for any conſiderable time, although it be able to remove the ſtring from its rectilineal poſition, yet, unleſs it be too rapid and violent, it will not be able to keep it bent; the fibre will, therefore, by its elaſticity, return to its former poſition, and by its acquired velocity paſs it on the other ſide, and ſo continue to vibrate and excite pulſes in the air, which will produce the tone of the entire ſtring. But if the current of air be too ſtrong and rapid when the ſtring is bent from the rectilineal poſition, it will not be able to recover it, but will continue bent and bellying like the cordage of a ſhip in a briſk gale. However, though the whole ſtring cannot perform its vibrations, the ſubordinate aliquot parts may, which will be of different lengths in different caſes, according to the rapidity of the blaſt. Thus when the velocity of the current of air increaſes ſo as to prevent the vibration of the whole ſtring, thoſe particles which ſtrike againſt the middle points of the halves of the ſtring agitate thoſe halves, as in the caſe of ſympathetic and ſecondary tones; and as theſe halves vibrate in half the time of the whole ſtring, though the blaſt may be too rapid to admit of the vibration of the whole, yet it can have no more effect in preventing the motion of the halves than it would have on the whole ſtring were its tenſion quadruple; for the times of vibrations in ſtrings in different lengths, and agreeing in other circumſtances, are directly as the lengths; and in ſtrings differing in tenſion and agreeing in other circumſtances, inverſely as the ſquare roots of the tenſions *(ſee Smith*

er Malcolm): and, therefore, their vibrations may become ſtrong enough to excite ſuch pulſes as will affect the drum of the ear: and the like may be ſaid of other aliquot diviſions of the ſtring. In the ſame manner as ſtanding corn is bent by a blaſt of wind, and if the wind be ſufficiently rapid it will have repeated its blaſt before the ſtem of corn can recover its perpendicular poſition, and therefore will keep it bent: but if it decays in rapidity or ſtrength, the ſtem of corn will have time to perform a vibration before it is again impelled; and thus it will appear to wave backwards and forwards by the impulſe of the wind. Thoſe particles which ſtrike againſt ſuch points of the ſtring as are not in the middle of aliquot parts, will interrupt and counteract each other's vibrations, as in the caſe of ſympathetic and ſecondary tones, and therefore will not produce a ſenſible effect. That we may be more fully perſuaded of the truth of theſe principles, I ſhall here ſet down the order of the Eolian notes as accurately as a good ear could diſcover.

Obſervation I. The original note of the ſtring being the grave fifteenth to low F on the violin, the Eolian notes as given in the annexed page * were diſtinctly perceived, and nearly in the ſame order in which they are ſet down.

From the table of proportions in Smith's Harmonics, p. 10, we may ſee that theſe notes were produced by ſuch aliquot parts of the ſtring as are denoted by the fractional indexes, which are written over them agreeable to the theory laid down.

Obſervation II. While ſome of theſe notes were ſounding I applied an obſtacle indifferently to any point, which divided the ſtring into ſuch aliquot parts as would produce theſe notes, and the Eolian note was not interrupted: but if I placed it in any other part, the tone was inſtantly extinguiſhed. This evidently ſhews that the entire ſtring is, in fact, reſolved into ſuch parts, as, from the preceding chain of reaſoning, we ſhould have been induced to preſcribe for it.

Obſervation III. I applied an obſtacle ſlightly againſt the ſtring, ſo as that its diſtance from the extremity ſhould be an aliquot part of the whole; and the Eolian note was that which would be produced by ſuch an aliquot part; thus we may in general predetermine what note the harp ſhall ſound. But this effect will not invariably take place; becauſe, though the

* The engraved table of notes conſiſts of three ſets of lines, with the cliff G on the ſecond line from the bottom, as uſual. In theſe the notes are written, and above each note the fraction which expreſſes the diviſion of the ſtring. As all theſe fractions have 1 for their numerator, I ſhall here give the letters expreſſing the notes, and the denominator or number denoting the ſubdiviſion:—Middle C 6;—lower F 4;—mid. A 5; —upper E, flat, 7, nearly;—mid. C 6;—mid. A 5;—up. D 7, nearly;—up. F 8;—up. D 7, nearly;—mid. C 6;—mid. A 5;—up. D 7, nearly,—up. F 8;—up. F 8, with up. D 7, nearly;—mid. C 6:—up. D 7, nearly;—up. E, flat, 7, nearly;—up. E 7, nearly;—up. F 8;—up. A 10;—up. G 9;—up. F 8;—mid. C 6, with up. E, flat, 7, nearly;—mid. C 6;—low F 4, with mid. A 5;—mid. A 5;—up. E, flat, 7, nearly;—mid. A 5; —up. E, flat, 7, nearly;—mid. C 6;—up. F 8;—up. G 9;—up. F 8;—up. E ſliding to up. E, flat, 7, nearly, and to up. D;—mid. C 6;—up. F 8;—up. E, flat, 7, with mid. C 6;—mid. C 6;—up. E, flat, 7, nearly;—mid. A 5, with mid. C 6, and up. E, flat, 7, nearly;—up. E, flat, 7. with up. F 8;—up. F 8;—up. B 11, nearly;—up. A 10;—up. F 8, with up. G 9;—up. E flat, ſliding to up. D;—up. C 6;—up. D. 7, with up F 8, &c. &c.

obſtacle

obſtacle may determine the ſtring to reſolve itſelf into ſuch aliquot parts rather than any others, yet the blaſt may be too ſtrong, or too weak to admit of ſuch a part vibrating with ſufficient ſtrength to produce a ſound; however, if any note be produced in this caſe, it muſt either be that of this very aliquot part, or of ſome of its own aliquot diviſions; for the obſtacle muſt neceſſarily determine one of the interſections of the equal indentures.

Obſervation IV. When the blaſt riſes or falls, we find the tone alſo gradually riſe or fall: becauſe, as the blaſt riſes, it grows too ſtrong to admit of the vibrations of the longer aliquot parts; the vibrations of the ſhort aliquot parts, therefore, will predominate, and will gradually ſhorten, as the blaſt riſes in ſtrength. But in caſes of ſudden variations in the ſtrength of the blaſt, there will be alſo ſudden tranſitions in the tones.

Obſervation V. We ſometimes hear a chord conſiſting of two or three Eolian notes; becauſe the blaſt, which is of ſuch a degree of ſtrength as to admit of the vibrations of certain aliquot parts, may alſo admit of the vibrations of other parts, if they be not very different in length; for their vibrations will be performed in times not very different. But if the length of theſe parts, and conſequently their times of vibration, be very different, the blaſt that admits of the vibration of the one will prevent that of the other. Accordingly, in looking over the foregoing table, we find that the chords conſiſt of thoſe notes which are produced by ſuch different aliquot parts as are leaſt unequal: thus, one chord conſiſts of C and E, which notes are produced by one ſixth and one ſeventh of the ſtring. Another chord conſiſts of F and A, which are produced by one fourth and one fifth of the ſtring. Another conſiſts of A, C, and E, which notes are produced by one fifth, one ſixth, and one ſeventh parts of the ſtring.

It is alſo worthy of obſervation, that in long ſtrings we never hear the original note and its octave at the ſame time; becauſe, though they are the next aliquot parts, yet their difference is ſo great, that the blaſt which admits of the vibration of one of them, will obſtruct and prevent the other. It is only in the higher diviſions of the ſtring that the chords are heard at all; and the ſlacker the note, the more frequent are the chords, for the reaſons aſſigned above, namely, becauſe the different aliquot parts, in ſuch caſes, approach nearer to equality.

Obſervation VI. Eolian tones are often heard, which are not produced by any exact ſubmultiple of the ſtring; but ſuch notes are very tranſitory, and immediately vary their pitch, gradually falling or riſing to the notes next below or above them, which are produced by exact aliquot parts of the whole ſtring. This ariſes from the tranſition of the diviſions of the ſtring from one number to another; for during this tranſition, the parts of the ſtring, whoſe vibrations produce the note, are gradually lengthening or ſhortening. Thus, ſuppoſe the Eolian tone was produced by one third of a ſtring; and that the breeze ſo varies as to cauſe this tone to fall into the octave of the original note; the points of quieſcence will gradually run along the ſtrings, and by ſo doing will produce a more gradually flattening, until it terminates in the octave to the whole ſtring.

Diſcords are alſo often heard from the uniſon ſtrings of this inſtrument; the cauſe of this is alſo

alſo evident from the manner in which the notes are generated; for the aliquot parts of a ſtring contain in themſelves an infinite variety of diſcords. Kircher in his *Phonurgia*, page 148, has attempted to account for theſe phenomena of the Eolian lyre, by ſuppoſing the current of air to ſtrike on different portions of the ſtring. But this is abſolutely overturned by experience: for, ſuppoſe the Eolian note to be one fifth above the original note of the ſtring, that is, one third of the whole; then, according to Kircher, the remaining part would be at reſt, which is falſe; for an obſtacle applied to any other point than the quieſcent points of diviſion, will deſtroy the eolian tone. Beſides, the chords that would ariſe on this theory are not ſuch as really take place in nature; thus, where the chord conſiſts of the note F and A, the firſt note F is produced, according to Kircher, by the blaſts ſtriking on one fourth of the ſtring: now, in this caſe, the remaining part of the ſtring muſt be at reſt, according to Kircher, but contrary to experience; or if it be agitated as one ſtring, it muſt produce the note of three fourths of the whole ſtring, that is a fourth above the baſs note; whereas, the note really produced is the double octave to the third above the baſs note, as may be ſeen in the table of the Eolian tones.

X.

Analyſis of the Chromate of Iron of the Baſtide de la Carrade. By CIT. TASSAERT.*

THIS metallic combination which has been lately diſcovered at the Baſtide de la Carrade, near Gaſſin, in the department of Var, has the form of an irregular maſs; its colour is a deep brown, nearly reſembling that of brown blende; it poſſeſſes the metallic luſtre; its hardneſs middling; its ſpecific gravity, 4,0326. Before the blow-pipe, it melts with difficulty; and when fuſed with borax, it gives a dirty greeniſh colour.

This ſubſtance was ſent to the Council of Mines, under the denomination of a brown blende; which it ſufficiently reſembles, though it is much heavier. In order to aſcertain the effect of acids on this ſubſtance, two grammes were reduced into impalpable powder, and boiled with weak nitrous acid. This acid not having attacked the powder, the muriatic was tried, which being attended with no greater effect, it was thought proper to attack it by alkalis.

Two other grammes were therefore taken, and ignited with five times their weight of potaſh. After a quarter of an hour's application of the fire, the crucible was ſuffered to cool, and the matter was mixed with water. The fluid exhibited a beautiful lemon colour which led to a ſuſpicion of the chromic acid; and, in fact, when trial was made with the nitates of lead, ſilver, and mercury, there remained no further doubt of its preſence; particularly when

* Annales de Chimie, XXXI. 220. I retain the word Baſtide, which is an old provincial term, denoting a country manſion, or caſtle.

the flight refidue left by the potafh was treated with the nitric acid, it fwelled up like a vegetable extract, which is one of the moft prominent characters of the chromic acid.

A. It being therefore afcertained that the chromic acid is the predominating ingredient in this compound, 500 centigrammes of this metallic falt were taken, reduced into impalpable powder, and expofed to heat in a crucible of platina, with eight times the weight of potafh. The mixture entered into perfect fufion, and was kept red-hot for half an hour, after which the crucible was taken from the fire, and left to cool. The mafs, when cold, had a fine yellow colour, much inclining to green. This was diffolved in water, to which it communicated a very fine lemon-yellow colour, a brown powder fubfiding to the bottom, which was collected on the filter. After well wafhing and drying, its weight amounted to 300 centigrammes.

B. This powder was boiled in muriatic acid. Much oxygenated muriatic acid was difengaged. The liquor affumed a fine emerald colour. It was then diluted with water, and filtered, to feparate a brown powder, which had not been attacked by the acid.

This powder was of the fame nature as the natural metallic falt, and after three fucceffive operations with potafh and the acid alternately, it was totally decompofed. It appears, however, that this alternation is neceffary, becaufe the chromate of iron, with excefs of the oxide, is not decompofable by potafh, however great the quantity may be; fo that it becomes neceffary to remove the excefs of oxide by an acid, before the potafh can take up another portion of the chromic acid. If the nitric acid be ufed, inftead of the muriatic, to remove the excefs of oxide, which is fet at liberty by the alkali, the matter is feen to fwell up like a vegetable extract, a property which immediately manifefts the prefence of chrome.

C. After the whole of the mafs was converted into chromate of potafh and metallic muriate, the latter folution was decompofed by potafh, and afforded a precipitate of a dark brown colour. This laft, after wafhing and drying, weighed 185 centigrammes, and confifted, for the moft part, of oxide of iron. The fluid feparated from this precipitate, had a fine lemon-yellow colour, and was, therefore, added to the chromate of potafh.

D. As it was fufpected that the oxide of iron contained a portion of chromic acid, or perhaps oxide of chrome, it was boiled with nitric acid, and afterwards with cauftic potafh, and afforded a fmall portion of chromate of potafh, which was feparated from the oxide. The latter, after being well calcined, weighed 180 centigrammes; it was then diffolved in muriatic acid; the folution was a fine yellow colour, and afforded a black precipitate by the addition of gallic acid. With the pruffiates it afforded blue, fo that it cannot be doubted but that it was the oxide of iron.

E. All the folutions of chromate of potafh being then put together, they were faturated with nitric acid, taking care to add a flight excefs; after which the whole was diluted with a fufficient quantity of water, and a folution of the nitrate of lead was added. A very abundant precipitate of a fuperb yellow colour fell down. It weighed 883 centigrammes, which, according to the proportions of component parts in the chromate of lead, gives 318 centigrammes.

It

It follows, therefore, from the experiments, D and E, that the chromate of iron is compoſed of

Chromic acid - - - - - - -	318
Oxide of iron - - - - - - -	180
	498
Loſt - - - - -	2
	500

Which gives for 100 parts,

Chromic acid - - - - - - -	63, 6
Oxide of iron - - - - - - -	36, 0
Loſt - - - - - - - -	0, 4
	100, 0

The chromate of iron is likewiſe decompoſed by the ſaturate carbonate of potaſh.

XI.

*Report made to the National Inſtitute of Sciences and Arts (at Paris), on the 29th Prairial, in the ſeventh Year (June 17, 1799), in the Name of the Claſs of Phyſical and Mathematical Sciences, on the Meaſure of the Meridian of France, and the Reſults which have been deduced to determine the new Metrical Syſtem.**

Citizens,

To employ, as the fundamental unity of all meaſures, a type taken from nature itſelf—a type as unchangeable as the globe on which we dwell; to propoſe a metrical ſyſtem, of which all the parts are intimately connected together, and of which the multiples and ſubdiviſions follow a natural progreſſion, which is ſimple, eaſy to comprehend, and in every caſe uniform: this is moſt aſſuredly a beautiful, great, and ſublime idea, worthy of the enlightened age in

* Two ſeparate reports were read to the claſs of phyſical and mathematical ſciences, in the name of the commiſſion of weights and meaſures; one on the 6th Prairial, by Citizen Van Swinden, on the meaſure of the meridian, and the determination of the metre; the other, on the 11th of the ſame month, by Citizen Tralles, on the unity of weight. It was decided by the claſs, that theſe two reports ſhould be united and digeſted into one, to be read at a general ſitting of the Inſtitute; and one of its members was accordingly charged with this office. It was performed by Citizen Van Swinden.—*Note of the reporter.*

I have tranſlated the above from the Journal de Phyſique, Thermidor, an. 7; and to avoid the probability of any error of the preſs in the numerical reſults, I have compared them with the ſame in the bulletin of the Philomathic Society, and the Decade Philoſophique, with which I find they agree.—N.

which

which we live. Accordingly, the Academy of Sciences, which, from its firſt eſtabliſhment, had fixed its attention upon the experiments of Huyghens on the ſimple pendulum, did not fail to direct the meditations of men of ſcience to the uniformity and invariability of meaſures. That learned body was aware of the great importance of this object: the wiſhes of the mathematical world were well known to them in this reſpect, and they beheld one of their number, the celebrated Condamine, employ his talents with the greateſt zeal, in deſtroying the objections which ignorance and prejudice did not ceaſe at that time, any more than at preſent, to oppoſe againſt its eſtabliſhment*. This academy did not fail to ſeize the moment when the people of France began to occupy themſelves in their political and ſocial regeneration to reſume this intereſting ſubject, the execution of which ſeemed to have waited till the period, when the impulſe given to the ſpirits of men, induced them eagerly to ſeize every thing which could tend to the public good; and when the exiſting circumſtances permitted them to attend them without conſtraint, and with the proſpect of ſucceſs. When conſulted by the conſtituent aſſembly, whoſe attention was fixed to this object by the propoſition of Citizen Talleyrand †, and charged to determine the unities of meaſure and of weight, they employed, for good reaſons, which were at that time developed ‡, as the baſe of the whole metrical ſyſtem, the fourth part of the terreſtrial meridian comprehended between the equator and the north pole. They adopted the ten millionth part of this arc for the unity of meaſure, which they denominated metre, and applied it equally to ſuperficial and ſolid meaſures, taking for the unity of the former the ſquare of the decuple, and for that of the latter the cube of the tenth part of the metre. They choſe for the unity of weight the quantity of diſtilled water which the ſame cube contains when reduced to a conſtant ſtate preſented by nature itſelf; and laſtly, they decided, that the multiples and ſub-multiples of each kind of meaſure, whether of weight, capacity, ſurface, or length, ſhould be always taken in the decimal progreſſion, as being the moſt ſimple, the moſt natural, and the moſt eaſy for calculation, according to the ſyſtem of numeration, which all Europe has employed for centuries. Such are the fundamental and eſſential points of the new metrical ſyſtem, which the academy has propoſed; which has been adopted by the conſtituent aſſembly; and which, under names different indeed from thoſe choſen by the academy, have been confirmed by the law of 18th Germinal, in the third year of the republic.

But as the baſis of the new metric ſyſtem depends on the fourth part of the terreſtrial meridian, it is neceſſary that the magnitude of this arc ſhould be known, if not with an extreme preciſion, yet, at leaſt, with a degree of preciſion ſufficient for practice. Various operations had been already made in France, about the end of the laſt century, to determine the magnitude of ſeveral arcs of the meridian, which croſſes this vaſt empire; and though there might remain ſome doubts with regard to the perfect accuracy of theſe operations, notwithſtanding the verifications which have from time to time been made, there were good reaſons to con-

* Memoirs of the Academy for 1748.

† Decree of the 8th of May, 1790.

‡ Memoirs of the Academy of 1789.

clude, after the refearches of the celebrated La Caille, that the mean degree would not differ much from 57027 toifes; and confequently that the fourth part of the meridian would contain 5,123,420, and that the ten millionth part of this arc would be 443,443 lines. In the juft impatience to enjoy the great benefit of exact, uniform, and univerfal meafures, the length of the metre was provifionally fettled at 443,44 under the well-formed perfuafion that the more precife determinations, which were to be expected, would produce but a flight change in this magnitude.

Neverthelefs, the academy, which confidered this fubject in its true point of view, as well in its general as in its particular relation, with regard to the public utility, its intimate connexion with the moft important points of phyfical aftronomy, with the national reputation, to which it was of confequence that the foundations of a new metrical fyftem, propofed to a great people, and prefented to the whole world for their adoption, fhould be determined with the greateft precifion, conceived the great project of obtaining a new meafure of the meridian which croffes France, by extending it beyond the frontiers, as far as Barcelona, and to apply this great arc to determine the fourth part of the earth. The conftituent affembly adopted this vaft project, and entrufted the execution to the academy, which nominated, without delay, feveral of its members to employ themfelves on the different parts which compofed the totality of the metrical fyftem; and laftly, they charged the Citizens Mechain and Delambre, fo worthy, in every refpect, of this honourable, though laborious miffion, with the tafk of meafuring the meridianal arc. The Inftitute afterwards nominated Cit. Lefevre Gineau to make the experiments relative to the determination of the unity of weight; and he has proved, by the beauty and accuracy of his work, how truly deferving he was of being affociated with his illuftrious brethren of the academy.

This great and important operation, projected by the academy of fciences for the eftablifhment of a new metrical fyftem, began by their orders, and happily terminated under the aufpices of the inftitute, after feven years' labour and care, is in many refpects deferving of remark. It is fingular for the extent of the terreftrial arc, which being more than nine degrees and two thirds, furpaffes all thofe which have been meafured;—for the extreme exactnefs with which all the parts have been executed; the terreftrial furvey for determining the length of the arc; the aftronomical obfervations; the operations for fixing the unity of weight; the experiments on the length of the pendulum; all thefe have proceeded together, and each has been treated with the fame precifion; and laftly, it is remarkable, and perhaps without parallel, for the degree of authenticity with which it is fanctioned. In fact, the Inftitute has defired not only that commiffioners chofen from its own body fhould examine every thing that is done, but likewife that learned foreigners fhould join, and make it a common work. The government has feconded this wifh, by inviting the allied or neutral powers to fend deputies for this object. Several have accepted this invitation; and thefe deputies, joined to the French commiffioners, compofed the commiffion of weights and meafures*, which

* The following is an alphabetic lift of the names of the members of the commiffion of weights. Æneæ, from the Batavian republic; Balbo, deputy from the king of Sardinia, afterwards replaced by Cit. Vaffali; Borda,

which has met, for ſome months paſt, in this palace under your auſpices, to fix deciſively the magnitude of the baſis of the new metrical ſyſtem. This commiſſion made the moſt minute enquiries into all the details of every obſervation, and each individual experiment. They weighed all the circumſtances jointly with the obſervers themſelves; deduced from the obſervations thoſe reſults which were to ſerve as grounds for calculation; and they determined the unities of meaſure and weight, which are the definitive reſults of the whole undertaking. Never did an operation of this kind undergo ſo ſtrict an examination; and the commiſſion, as well from duty, as to expreſs the ſatisfaction they have received, have thought fit to acquaint the Inſtitute, that the Citizens Mechain, Delambre, and Lefevre Ginneau, were, in every caſe deſirous of ſubmitting their original documents to their inſpection; that they readily gave every poſſible explanation, as well with regard to the inſtruments as the methods they employed; and in a word, that they anticipated the deſires of the commiſſioners in every point with the attention of brothers and friends, and with that reſpectable openneſs of character which diſtinguiſhes the accurate obſerver, who far from fearing a ſevere examination, is, on the contrary, deſirous that the minuteſt detail, and the moſt ſcrupulous enquiries, ſhould be entered into, in order that the truth may appear in all its luſtre.

In the performance of my charge of rendering this account of the work of theſe excellent obſervers, and of the operations of the commiſſion of weights and meaſures, to fix the unities which ſerve as the baſe of the new metrical ſyſtem, let me be allowed, for the ſake of order among the multiplicity of objects I am to ſubmit to your judgment, to ſpeak firſt of that which relates to the meaſure of the arc of the meridian, and the determination of the metre, or unity of linear meaſures thence reſulting; after which, I ſhall offer to your conſideration, the experiments which it was neceſſary to make to fix the unity of weight; and laſtly, while I preſent to you the ſtandards of theſe two unities, may I offer ſome reflexions on their nature, their uſe, and the method of reſtoring them with the greateſt exactneſs, ſuppoſing even that every ſtandard was deſtroyed, and the name only to remain; an invaluable advantage of theſe new meaſures, which gives them a right to the title of invariable.

Let us begin with what concerns the meaſure of the meridian. Citizens Mechain and Delambre ſhared this immenſe work. The northern part from Dunkirk to Rhodes was performed by the firſt, and Citizen Mechain performed all the reſt from Rhodes to Barcelona. He greatly regretted that circumſtances did not permit him to carry his operations, as far as the iſland Carbera. He had even made all the preparations for that enterprize, had under-

Borda, who died in Ventoſe laſt; Briſſon, Bugge, deputies from the king of Denmark; Ciſcar, deputy from the king of Spain; Coulomb, Darcet, Delambre, Fabbroni, deputies from Tuſcany; la Grange, la Place, Lefevre-Gineau, Legendre, Franchini, deputies from the Roman republic; Maſcheroni, deputy from the Ciſapine republic; Mechain, Multedo, deputies from the Ligurian republic; Pederayes, deputy from the king of Spain; Proni, Tralles, deputies from the Helvetic republic; Van Swinden, deputy from the Batavian Republic; Vaſſali, deputy from the proviſional government of Piedmont.—*Note of the Reporter.*

I have tranſlated the above note as it ſtands in the original; but I ſuppoſe that Briſſon, Coulomb, Darcet, Delambre, la Grange, la Place, Legendre, Mechain, and Proni, are not deputies from foreign powers.—N.

taken the neceſſary excurſions to examine the place, and ſettle the ſtations; and he even laid down on paper the triangles neceſſary to be meaſured; ſo that the whole of this part is ſketched out, and by the activity and care he has beſtowed upon it, it will be eaſy to add that arc to what has already been meaſured, and by that means to prolong the meridian two degrees. Let us hope that more favourable circumſtances may hereafter permit that to be carried into effect which hitherto has been impracticable*.

The obſervers made uſe, for the meaſurement of every kind of angles, of the entire circle of Borda, which is juſtly called the repeating circle, from the valuable advantage which it affords of repeating the angle to be obſerved any number of times, and conſequently diminiſhing the errors in the ſame proportion. This circle, which was conſtructed by the celebrated Lenoir, under the inſpection of Borda himſelf, was fully tried by the obſervations made in the year 1787 by Caſſini, Mechain, and Legendre, in the operations for the junction of the obſervatories of Paris and Greenwich.

More than one ſeries of obſervations was made at each ſtation, and the obſervers formed each ſeries out of ſuch a number of obſervations as they thought neceſſary to produce a conſtant and ſufficiently accurate reſult. They noted in their regiſter the numbers indicated by each obſervation, as well as the particular circumſtances which took place, ſuch as the ſtate of the atmoſphere, the direction of the light, and, in a word, every thing which could ſerve to determine the intrinſic value of an obſervation. The members of the commiſſion nominated for the arrangement of theſe obſervations were, therefore, capable of judging of this value, not only from the facts ſo recorded, but alſo from the information they received from the obſervers themſelves.

From ſuch an attention to the agreement or variations between the different ſeries of obſervations, the commiſſioners were enabled to determine the value of each angle abſtractedly, without paying any attention to the others, nor to the ſum of the three angles of each triangle. They have thought it their duty to take the angles, ſuch as they were, without making the leaſt correction, or proceeding in any other manner than by taking the means of the obſervations, according to the authority which the regiſter appeared to give to each. Theſe careful diſcuſſions for the moſt part related to the determination of tenth parts of ſeconds, and very ſeldom to whole ſeconds. The commiſſioners in this manner formed tables of all the triangles, which have ſerved for the determination of the medium. They have preſented this to the general commiſſion, together with a detail of the method they have employed, and the reaſons of their determinations. The commiſſion has received theſe tables, and paſſed their reſolutions, by which they are depoſited in the Inſtitute as authentic, as including all the principles applicable to the computation of the triangles and the parts of the meridian; and, in fact, the calculations were afterwards made from them.

* On account of the length of this report, I ſhall confine myſelf to ſuch extracts as relate to the immediate operations and reſults, omitting his general remarks. Cit. Delambre has publiſhed an account of his reſearches, under the title of *Méthodes analytiques pour la Determination d'un Arc du Meredien*, in quarto. This work is preceded by a memoir of Legendre on the ſame ſubject.—N.

The

The precision with which the angles were obferved is fuch, that out of ninety triangles, which connect the extremities of the meridian, there are thirty-fix in which the fum of the three angles differs from its proper quantity by lefs than one fecond; that is to fay, in which the error of the three angles taken together is lefs than one fecond: there are 27 triangles in which this error is lefs than two feconds; in 18 others it does not amount to three feconds; and there are four triangles in which it falls between three and four feconds, and three triangles only in which it is more than four, but lefs than five feconds. It may be doubted whether a greater degree of accuracy be obtainable, particularly in the country where the operations were performed; and accordingly it may be fuppofed by thofe who confider thefe tables without being informed of the manner in which they were made, that this appearance of precifion may have been given by management and fubfequent correction; but the original regifters of the obfervers, the refults which they themfelves fent to Paris, long before the bafes were meafured, and at a time when they were ftill bufied with their operations, and the labours of the commiffioners themfelves, prove the contrary in the moft authentic manner. No arbitrary or conjectural correction, however flight, has in any cafe been made; and all the angles have been determined from confiderations derived from the obfervations themfelves.

Two bafes were meafured by Cit. Delambre; one between Melun and Lieufaint, the other near Perpignan, between Vernet and Salces. The care and precaution with which thefe operations were performed, and the means adopted for that purpofe, are detailed at full length by Cit. Delambre, in his Memoir already mentioned. The inftruments were four rods of platina, conftructed with great care by Citizen Lenfire, from the inftructions, and under the infpection of Citizen Borda. Each of thefe rules is covered to within four inches of its anterior extremity with a fimilar plate of brafs, movable in the direction of the length of the rule of platina, and fixed to it by the extremity which is moft remote from the uncovered part. This plate of brafs forms, by the different dilatations which the fame variation of temperature produces in the brafs and the platina, a very fenfible metallic thermometer, the dimenfions of which are engraved upon the anterior extremity, which carries a vernier and a microfcope to afcertain the fubdivifions. Before thefe rules were ufed a number of experiments were made to afcertain their dilatation, the ftate and motion of the metallic thermometers, and their comparifon with the ordinary thermometers. The lengths of the rules, Nos. 2, 3, and 4, were compared with that of No. 1, to which all the meafures were reduced, which for that reafon was called the original (le module); which comparifons were made with fuch accuracy as to have no doubt of the two hundred thoufandth part. Citizen Borda has delivered to the commiffion a memoir containing a detail of all his experiments, which will form an effential and interefting part of the collection to be publifhed refpecting this great operation.

Thefe rules were placed in proper cafes, to fecure them for every external action or flexure without checking their expanfion or contraction, as well as to defend them from the rays of the fun, &c.

Every

Every care was taken to fupport and difpofe them properly in meafuring the bafes themfelves. Their extremities were never brought into contact; but an interval was left, which was meafured by a tongue of Platina, fliding from the end of one of the rules, and carrying a vernier and microfcope. The corrections or allowances for differences of temperature, for obliquities of the line actually meafured, and for the elevation above the level of the fea, were alfo of neceffity to be attended to and allowed for.

As the length of the bafes are expreffed in modules, all the other refults are denoted by the fame unity. But to give a proper notion of the comparative value of this unity, with regard to the ftandards employed in other great operations, it became neceffary to afcertain the length of No. 1, or the module from the toife of the academy called the toife of Peru; which was done before the commencement of the meafuring the bafe. This comparifon was m de with a degree of precifion fufficient to afcertain the hundred thoufandth part of a toife. The details of thefe experiments are given in the Memoir of Cit. Borda, already mentioned. After his return Cit. Delambre did not fail to compare the rules which had been ufed for the meafurement of the bafes, and did not find the flighteft change in their length. Laftly, the commiffion charged feveral of its members to repeat the fame comparifon of the module with that of the toife of Peru, that of the North, and that of Mairan, all three of which have become celebrated or important; the two firft by the great operations to which they have been applied, and the third, becaufe it was in parts of that toife that Mairan has expreffed the refults of his valuable experiments on the length of the pendulum, and becaufe it is the ftandard of the toifes which were ufed to meafure the two terreftrial degrees in the neighbourhood of Rome, by the celebrated Bofcovich and Lemaire. This new comparifon of the module to the toife of Peru again afforded the fame refult, namely, that the fcales had undergone no changes, and proved, moreover, that the module is exactly twice the length of the toife of Peru, and confequently 12 feet in length, when the centigrade thermometer is at $12\frac{1}{2}$ degrees: whence it is deducible, as well by a calculation from the dilatation of the metals as from the direct experiments of Borda, that at the temperature of $16\frac{1}{4}$ degrees, which anfwers to 13 degrees of Reaumur's thermometer, the module is fhorter than the double toife by two hundred parts of a line; that is to fay, about the eighty-fifth thoufandth part of the whole.

The obfervations of Azimuth were made on the fun, and on the pole ftar at Watten, at Bourges, at Carcaffonne, and at Mountjouy; that is to fay, at the two extremities of the meridian, and two intermediate places. The obfervations of latitude, which were made with the circle of Borda. From the great number of the obfervations, and their agreements with each other, it is confidered as a certainty that the error cannot amount to any thing near half a fecond in any of the latitudes obferved.

Thefe obfervations were made at Dunkirk and at Evaux by Citizen Delambre; at Carcaffone and Montjouy by Citizen Mechain; and at Paris by Citizen Mechain, at the national obfervatory, and by Citizen Delambre at his own private obfervatory.

Four

Four commiſſioners were ſpecially charged with the computation of the triangles, which they performed ſeparately, by different methods, in order to leave nothing doubtful as to the certainty of the reſults. They have alſo calculated, and in every caſe by different methods, the four portions of the meridian comprehended between the places at which the latitude was obſerved; namely, the terreſtrial arc comprehended between Dunkirk and the Pantheon at Paris; the Pantheon and Evaux; Evaux and Carcaſſonne; Carcaſſonne and Montjouy. The details of theſe calculations, and the principles on which they are founded, are contained in a memoir depoſited in the archives of the Inſtitute*.

Among other concluſions which preſent themſelves in theſe calculations, there are two, to which the attention of the Inſtitute is directed: the firſt, that the mean degrees concluded for the four intervals, of which mention is made, all decreaſe as they approach the equator; and, conſequently, that this operation alone would prove the oblate figure of the earth, if this article required any proof: the ſecond, which was far from being ſuſpected, and exhibits a very remarkable phenomenon, worthy of the enquiries of the moſt profound mathematicians, that theſe ſame degrees do not follow a gradual diminution, but decreaſe at firſt very ſlowly, between Paris and Evaux, only two modules for a degree of latitude; afterwards very rapidly, namely, ſixteen modules for the degree of latitude between Evaux and Carcaſſonne, and that this rapid diminution becomes leſs between the laſt-mentioned town and Montjouy, being no more than ſeven modules†.

This remarkable fact is intimately connected with another, namely, that there are differences between the azimuths calculated for Bourges, Carcaſſonne, and Montjouy, from that

* The meridian between Dunkirk and Montjouy, which ſubtends a celeſtial arc of 9,6738 degrees, and of which the middle point paſſes through 46° 11′ 5″ of latitude, is equal to 275792.36 modules.

That is to ſay;

			Modules.
1. The diſtance between the parallels of Dunkirk and the Pantheon, the middle point of which lies in lat. 49° 56′ 30″, ſubtends an arc of	2°.18910	and meaſures	62472.59
2. The diſtance between the parallels of the Pantheon and Evaux, the middle point of which lies in lat. 47° 30′ 46″, ſubtends an arc of	2.66868	and meaſures	76145.74
3. The diſtance between the parallels of Evaux and Carcaſſonne, the middle point of which lies in lat. 44° 41′ 48″, ſubtends an arc of	2.96336	and meaſures	84424.55
4. The diſtance between the parallels of Carcaſſonne and Montjouy, the middle point of which lies in lat. 42° 17′ 20″, ſubtends an arc of	1.85266	and meaſures	52749.48
Whole celeſtial arc	9.67380	Meaſure	275792.36

† If from the four intervals before given, we deduce the mean degree, which may be concluded from the ſpherical hypotheſis, which is ſufficient for a curſory view, we ſhall find the mean degree in round members;

	Modules.	Difference.	Difference for one degree.
Between Dunkirk and the Pantheon, mean latitude, 49° 56′ 30″.	28538	5	2
Between the Pantheon and Evaux, mean latitude, 47° 30′ 46″.	28533	44	16
Between Evaux and Carcaſſonne, mean latitude, 44° 41′ 48″.	28489	22	7
Between Carcaſſonne and Montjouy, mean latitude, 42° 17′ 20″.	28472		

of

of Dunkirk taken as a baſe, and the azimuths actually obſerved at theſe three ſtations. Theſe two facts eventually confirm and ſupport each other; and when combined, they indicate, either an irregularity in the terreſtrial meridian, or an elliptic form in the equator and its parallels, or an irregularity in the internal ſtructure of the earth, or an effect of the attraction of mountains, or a powerful action of all theſe cauſes, or a certain number of them united. It isa taſk worthy of the moſt celebrated mathematicians to fix their attention upon theſe facts, and endeavour to develope their elements, in order to obtain a more perfect theory of the earth than we have hitherto poſſeſſed.

The commiſſioners, whoſe object it was to determine the length of the fourth part of the meridian, and thence the unity of meaſure, directed their whole attention towards that object. They employed the whole arc comprized between Dunkirk and Montjouy in their calculations, which were ſtrictly made, according to the elliptical hypotheſis. To make this calculation, it was requiſite to know the difference between the equatorial and polar diameters. This was obtained by comparing the newly-meaſured arc with the largeſt and beſt ſituated of the arcs already meaſured; namely, that in Peru. The computations carefully made, and by different formulæ, gave one three hundred and thirty-fourth part for the flattening of the earth, which is the ſame as reſults from the combination of a great number of experiments at different places on the earth, on the length of the ſimple pendulum, as well as conformable to the theory of the nutation of the earth's axis, and preceſſion of the equinoctial points. It is, moreover, obſerved, that as the middle of the entire arc, terminated by Dunkirk and Montjouy, paſſes near the forty-fifth, or mean degree, of latitude, a ſlight error would have the leſs influence on the final determination.

By various methods of computation, employing the arc between Dunkirk and Montjouy, of two hundred and ſeventy-five thouſand ſeven hundred and ninety-two modules, and thirty-ſix hundredth parts; and the quantity, three hundred and thirty-four for the oblate figure; it was found that the fourth part of the terreſtrial meridian is two million five hundred and ſixty-five thouſand three hundred and ſeventy modules; and conſequently, that its ten millioneth part, or the *metre*, or unity of meaſure, is 256537 millioneth parts of the module.

To reduce this length to the ancient meaſures, it is obſerved that if the module and the toiſe of Peru were ſuppoſed to be each at the temperature of the latter when employed by the academicians, which anſwers to the thirteenth degree of the thermometer of Mercury, divided into 80 parts, or ſixteen and a quarter of the centigrade thermometer, the metre would be equal to 443,291 lines of that toiſe; but by reducing the module to the temperature (as it ought to be) to which it was reduced in the expreſſion of the length of the baſes which was uſed to calculate the triangles, and the length of the portion of the terreſtrial meridian, *the true and definitive metre is four hundred and forty-three lines, and two hundred and ninety-ſix thouſandth parts of a line* of the toiſe of Peru, this laſt being conſtantly ſuppoſed to have the temperature of ſixteen degrees and a quarter. This laſt correction became neceſſary, on account of the difference of expanſion in the two metals.

(To be continued.)

XII.

Detached Facts or Notices on chemical Subjects—Nitric Acid—Oxide of Azote—Indigo—Nitrite of Potash—Residue of Ether—Inflammation of Oils by Nitric Acid—Ammoniac—Putrid Water at Sea. By CITIZEN PROUST, *Professor of Chemistry at Madrid.**

Nitric Acid.

WITH very dry nitre and sulphuric acid I prepared nitric acid, which was pure, and supported the proof of barytes. After it had been deprived of gas, by careful distillation, it continued yellow, and had a specific gravity of 152.—When again distilled, the first product had the specific gravity of 151, and was less yellow; the second, though less coloured, was only of the same density; but I was surprized to find the residue, which was perfectly colourless, weighed only 147. This being also distilled over, the first portion was of 149, and the rest did not exceed 144.

In another experiment the acid obtained from very dry nitre had the density 155. It was first cleared of gas, and afterward distilled. Its first product had the density 162. The second paler, 153; and the colourless residue weighed no more than 149. Hence we may observe that the acid in its concentration is affected very differently from what we might suppose †, and that it is the reverse of other acids. I have remarked on these occasions, that the stronger the acid the easier the distillation, and that it is not necessary to boil it, in order that it may evaporate with more rapidity. A notion of the force which concentrated acid exerts upon its own parts may be formed by pouring the acid of 148 upon pulverized tin. It produces no more effect than upon sand. This, however, is not the case with zinc‡.

Oxide of Azote.

To obtain this gas with certainty, the acid made use of must have the specific gravity of 15° of the areometer of Baumé (1,114) to be applied to zinc. An acid at 18 or 20 afford a mixture of nitrous gas, and no doubt azote.

Indigo.

An acid of 148 dissolves this substance, and turns it yellow by oxidation. It is a resin soluble in alcohol and separable by water. It was with surprize that I found, by means of weak acids, magnesia even abundantly in indigos, besides the extractive matter which they are known to contain.

An acid of 151 or 152 inflames indigo, as Sage has published, and as Woulf likewise shewed to Hillare Rouelle, who shewed it in his lectures.

* Journal de Physique, Messidor VII.

† The same facts have been observed by Chaptal and other French chemists.

‡ The majority of chemists ascribe this difference of effect to the decomposition of water, which, from many facts, appears to be the true solution.

Nitrite of Potash.

This salt is totally deliquescent. It is separated from the fused and ignited nitrate by means of crystallizations, which gives solidity to all the nitrate which has remained unchanged. Weak sulphuric or nitric acid separate nitrous gas in abundance. Distilled vinegar precipitates nothing, though it occasions a slight effervescence.

Expecting to separate the nitrate from the residue of the crystallizations, I added spirit of wine, which threw down a saline precipitate. Losing sight of my nitrate afterwards, I took a fancy to decompose it by an acid in the midst of the spirit of wine. The consequence was, a new fact which produced ideas of some importance, from what at first promised nothing.

Weak sulphuric acid poured into the mixture occasioned an effervescence, and its product instead of being nitrous gas, proved to be nitric ether with disengaged heat. This experiment certainly deserves to be repeated. I set fire to the vapour, and the greenish colour in the flame was similar to that of the ether. The nitrous gas was probably decomposed, its oxygen absorbed by the alcohol, and the azote undoubtedly dissipated with the ethereal gas; but, in order to transform alcohol into nitric ether, is there any thing else wanting but a simple solution of concrete oxygen? The following facts seem to prove this:

I poured 4 oz. of spirit of wine, well dephlegmated, into a pint bottle filled with the oxigenated muriatic gas. The gas was quietly dissolved, and the bottle became clear. A few minutes afterwards the spirit of wine was discoloured, and there was a disengagement of heat without the appearance of any kind of gas. At the opening of the bottle I perceived the existence of nitric ether.

This experiment was repeated with the same spirit of wine successively, on clean bottles of the gas. The same phenomena were re-produced, and the ether appeared to increase; after which I judged, by the diminution of the smell, that it advanced to another state, which is probably the transition from alcohol to the states of vegetable acid, among which the muriatic acid re-appeared with all its inertness.

Why may it not be the same with alcohol as with most of the other vegetable products, which are very capable of being superoxided in their totality without disunion or combustion of any one of their principles? Such are the alterations which we observe in the volatile oils, fat, tallow, colouring resins, &c. all which are capable of receiving new appearances by the addition or spontaneous absorption or a slight dose of oxygen, while the maximum produces their entire decomposition. These observations are not applicable to sulphuric ether.

Residue of Sulphuric Ether.

The residue of this ether, distilled to the utmost, never affords sulphur, notwithstanding the observations of a number of writers. If a small portion be distilled in a large retort, the product is spirit of wine, oil, the common gases, and water.

After

After the intumefcence has fubfided, the pitchy matter, which is merely oil, rendered thick by charcoal, takes this laft ftate by the concentration of the acid, and begins to feparate to fuch a degree the that liquid becomes vifibly clearer.

The diftillation being ftill urged, all this charcoal becomes collected together; fulphureous gas paffes over, and afterwards oil of vitriol, flightly coloured by charcoal. All the acid paffes at laft in this degree of concentration, and the refidue at the bottom of the retort is a plate of pure charcoal; fuch as is obtained by diftillation of fulphuric acid from any charcoal whatever.

When, in order to obtain the olefiant gas of the Dutch chemifts, three parts of the acid are diftilled with one of the fpirit of wine, the pitchy matter becomes entirely charred, and in this manner a dram of dry charcoal is obtained from one ounce of alcohol.

On the Inflammation of Oils by the Nitric Acid.

With ftrong nitric acid of the fpecific gravity 152 it is impoffible to inflame linfeed oil by the ordinary method; but in order to fucceed in fuch a manner as to form a ftriking appearance, it is proper to pour linfeed oil gently to the depth of an inch upon the fame depth of the acid. The wine-glafs (verre à patte) being then placed upon a plate with a fmall quantity of water, the whole is to be covered with a bell-glafs, fufficiently tall. In lefs than a quarter of an hour the ebullition begins, which is fpeedily followed by the inflammation. The experiment fucceeds equally well, though rather more flowly, with acids of 149 and 150.

Oil olive treated in the fame manner, but always with the mixture of fulphuric acid, takes fire with the fame fuccefs. In thefe experiments the operator has the fatisfaction to obferve, at his eafe, the charcoal difengage itfelf from the other principles of the oil before the inflammation.

And in order to obferve with accuracy the changes which the acid undergoes, on its part, from the olive oil, for example, two fingers depth of the latter, are to be poured on the fame quantity of acid, at 149, or thereabouts, placed at the bottom of a cylinder, with a foot, of one inch in diameter; and the line of contact of the two liquids being marked, the following effects are feen :

The height of the acid is rapidly diminifhed; nitrous gas efcapes through the oil; and the phenomena are completed when the remainder of the acid, becoming too aqueous, ceafes to act on the oil, which on the other hand becomes fomewhat thickened.

If, inftead of oil, a fpirit of wine of 35° (0,842. I. p. 39), all the feries of phenomena which belong to the formation of nitric acid are very agreeably obferved. This procefs, in fome refpects, refembles that of Black, which confifts in fkilfully caufing three ftrata of different liquors in a matrafs, placed in ice-cold water. Thefe liquors are the fuming acid, water, and fpirit of wine. Thefe three ftrata may be eafily arranged by means of a ball and fyphon,

ſyphon, with which the liquors are ſuffered to run down on one of the ſides of the neck of the veſſel; which neck for that purpoſe ought not to exceed two inches in length. I have not ſeen this proceſs in any French author (if I am not deceived), though it is deſcribed in one of the firſt Journals of Crell.

On Ammoniac.

It is known, that the flame of a candle placed at the mouth of a cylinder full of ammoniacal gas is enlarged. From this fact, I have uſually in my lectures mixed it with one part of oxygen gas, in which caſe it takes fire, and detonates. For want of time, I have not conſtructed an eudiometer of iron, or of glaſs, with a conductor of iron to operate over the water bath. Mixing theſe two gaſes afterwards, and detonating them, the quantity of azote contained in the volatile alkali, may be very accurately ſeparated in putrid ſea-water.

Sea-water.

Two years ago, the Prince of Parma had the goodneſs to ſend me ſome bottles very well corked of putrid water, taken from a veſſel in its return from Carthagena.

Upon opening one of them, I readily perceived the ſmell of hepatic gas. Water tinged with a ſolution of copper was precipitated of a brown colour, and this precipitate, collected and examined by the blow-pipe, proved to be the blue ſulphuret of copper.

After the water had loſt its ſmell by expoſure to the air, I examined it, and found it exceſſively loaded with plaſter, which led me to conclude that the caſk had certainly not been filled with river-water.

So large a quantity of ſulphate of lime in this water could not but produce ſulphurated hydrogen in its putrefaction; for which reaſon I was not ſurprized at its quality; but in river water which does not contain plaſter, what may be the nature of their putrefaction? As I have never ſeen ſtinking water of this ſecond kind, I ſuppoſe it would be neceſſary to be in ſome ſea-port to obſerve, analyze, and endeavour to render them ſweet.

In the mean time, the following facts with regard to hepatized waters, may be of ſome uſe towards the ſolution of this important problem.

With about half an ounce of magneſia in powder, agitated in the putrid water, I cauſed all its bad ſmell to diſappear in leſs than four minutes.

In order to aſcertain ſtill more clearly the effect of magneſia, it was neceſſary to apply it to water in the higheſt degree ſtinking. For that purpoſe, I ſtrongly agitated an ounce of magneſia in powder, in a pint of artificial hepatic water, the moſt ſtinking I could poſſibly make, and in a few inſtants I ſucceeded very well in ameliorating the ſmell; but it obſtinately retained a ſmell of onions which I could not diſpel: but even in this ſtate, in caſe of neceſſity, I could have drank it without repugnance.

SCIENTIFIC

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

National Inſtitute of France.

THE philoſophical and mathematical communications of this eſtabliſhment during the laſt trimeſtre are as follows:

Guyton related a number of experiments on the mutual affinities of the earths to each other in the humid as well as in the dry way. His experiments of the latter kind were made in crucibles of platina; and on the occaſion of the former, he diſcuſſes the conſideration how far barytes and ſtrontian ought to be ranked with alkalis. At the ſame ſitting, he communicated an account of the experiments on the combuſtion of the diamond. (See page 295.)

It is well known to chemiſts, that Scheele is the firſt who has ſpoken of a brown oxide of lead, and profeſſor Prouſt has lately deſcribed ſome of its properties, and the manner of making it. The experiments of Vauquelin ſhew that this oxide, which is of a dark brown colour, becomes yellow by the ſlighteſt impreſſion of heat from the blow-pipe, and afterwards melts. It affords a very great quantity of pure oxygen gas, when heated in a retort with the pneumatic apparatus. When heated with weak ſulphuric acid, it alſo gives out oxygen gas, preciſely like mangeneſe, many of whoſe properties it appears to poſſeſs. Its moſt remarkable property is, that it takes fire ſpeedily by ſimple trituration with ſulphur. The inflammation is rapid and brilliant, but without detonation or exploſion. This ſuper-oxygenated lead, after this combuſtion of the ſulphur, is converted into galena, of which it has the black blueiſh colour, and all the other characters.

The ſame chemiſt communicated likewiſe ſome obſervations on the decompoſition of the muriate of ſoda by the oxide of lead; a fact, which though well known before, was not ſatiſfactorily explained previous to the experiments and reſearches of Vauquelin.

Huzard read certain obſervations and reflections upon canine madneſs, by the late Flandrin, aſſociate of the Inſtitute. Among other important obſervations, the moſt remarkable is, that herbivorous animals, though ſuſceptible of this diſeaſe, very ſeldom communicate it. The ſame citizen read for Cit. Gilbert, a paper on agriculture, and the perfection of the race of ſheep.

Cit. Teiſſier likewiſe read ſome uſeful agricultural obſervations applicable to France; and Rougier la Bergerie read a memoir on a diſeaſe of barley. Lacepede preſented a new methodical table of mammiferous animals; that is to ſay, viviparous quadrupeds and cetacea. Cit. Desfontaines communicated obſervations which he had made in the deſert of Sahara, on the culture and uſes of the date-bearing palm.

Cits. Ventenat and Vauquelin preſented each a ſeveral work. The firſt, a table of the vegetable kingdom, according to the natural method of Juſſieu. A work in which is contained, not only the abridged hiſtory of the ſcience, with the anatomy of vegetables, but likewiſe the order of their arrangement, the character of the claſſes, the families, and a great number

number of known genera. The ſecond work of Vauquelin conſiſts of a ſeries of experiments, made in order to analyſe the ſap of the elm, the beech, the maple, the birch, and the cheſnut-tree.

The ſame chemiſt has alſo publiſhed a work, entitled, Manuel de l'Eſſayeur, or a treatiſe on the art of aſſaying gold and ſilver by the cupel, and the proceſs of parting; containing every thing which is abſolutely neceſſary to be known in the practiſing that art. Cit. Delambre has publiſhed his method of computing the arc of the meridian from the late meaſurements in France.

The tranſit of Mercury over the ſun's diſc, which happened on the 18th of Floreal, year 7 (29 May, 1798), and laſted ſeven hours twenty-four minutes and fifty ſeconds, was obſerved by Cit. Meſſier, during the whole time of the paſſage. The great number of accurate obſervations which this aſtronomer made, are recorded in a memoir read at one of the ordinary ſittings. Mercury appeared perfectly round, and ſurrounded by a very ſenſible atmoſphere, when the light of the ſun was moderated by ſlight clouds. Cit. Jeaurat obſerved the ſame phenomenon, and read a memoir containing his obſervations.

Cit. Caſſini, aſſociated member, gave a deſcription of a new compaſs for determining the declination of the magnetic needle with the greateſt preciſion. The needle of his compaſſes is ſuſpended by wire or thread according to the method of Coulomb; and Cit. Caſſini, by adapting a teleſcope, and an entire movable circle, obtains an inſtrument with which an angle may be meaſured with the ſame degree of exactneſs, as by the repeating circle of Borda. According to the obſervations made with this compaſs on the platform of the National Obſervatory, it is proved that the bars of iron, which enter into the conſtruction of the edifice, render all the obſervations of the needle defective: and by other experiments made at Montmartre, far from all foreign influence, Cit. Caſſini has ſettled the declination of the magnetic needle at 22° 49′ on the 5th Prairial, in the year 1.

Van Swinden gave an account to the claſs of mathematical and phyſical ſciences, of the operations for determining the length of the meridian, and that of the metre. Cit. Tralles made a report of the operations relative to fixing the unity of weight.

The ſtandards, in platina, of the metre and kilogramme, and the general report of the whole operation by Van Swinden, were preſented to the legiſlative body, and depoſited among the national archives.

Note concerning the Devahk, or Cubit of the Nilometre.

This meaſure, which is of the higheſt antiquity, is valued at 20,544 inches, in the Hiſtory of Modern Aſtronomy, tome II. p. 146 (I ſuppoſe of Lalande), which amounts to 0,5559 metres. Cit. Dillon, examiner of weights and meaſures, having procured a new copy of the cubit of the nilometre, very carefully made in the workſhop of the widow Lennel, after another copy, taken immediately upon the ſpot by a learned Engliſhman, found it correſpond with 0,5555 metres, a reſult falling ſhort of the former determination by 0,0004 only,

only, whence it follows that the cubit of the nilometre is within one thoufandth part of five ninths of the metre, or the eighteen millionth part of the quarter of the meridian.—Soc. Philom. No. 27. An. VII.

Citizen Lalande has communicated to the Inftitute of France the obfervation and computation of the laft oppofition of Mars, and by comparing it with that of 1790, he finds that no more than 58 feconds are required to be taken from the place of the aphelion of Mars, employed in the laft edition of his aftronomy. But he announces the great labours of his nephew, Lefrancais Lalande.

C. Lalande alfo gave computations of folar eclipfes, and fidereal obfervations obferved for feveral years paft, to determine the pofitions of different places. He places Hamburg at 30′ 9″ from Paris; Cobourg, 34′ 30″; Mulheim, 21′ 20″; Halle, 38′ 28″; and Konigfberg, 1h 12′ 35″.

On the Ufe of the Actual Cautery in a Difeafe of the Chefnut-tree, by the Cit. CHAPTAL.*

The chefnut-tree forms the only refource of the cultivator in various parts of France, where it is particularly encouraged on account of its utility. This tree lives long, and often grows to an extraordinary fize; but unfortunately its ligneous part degenerates in feveral circumftances: it foftens, falls into duft, and, in the courfe of time, a cavity is formed in the very heart of the tree, which becomes enlarged by the progrefs of the decompofition; fo that at laft the trunk prefents nothing but a fhell of bark, which being too feeble to fupport the weight of the branches, or to refift the violence of the winds, can no longer fecure or prolong its exiftence. By fuch alterations and decompofitions of the ligneous principle it is, that trees are feen to perifh in a very fhort time, whofe growth have required whole centuries.

Citizen Chaptal, in his travels through different parts of the republic, and chiefly in the Cevennes and in the department de l'Allier, obferved that the internal part of a great number of chefnut-trees was dug out and burned to a coal all over its furface. The inhabitants explained to him, that this was done to ftop the progrefs of the rottennefs, which would otherwife deftroy the whole tree.

When they perceive that this very common and deftructive difeafe begins to make fome progrefs in the chefnut-tree by excavating its trunk, they collect heath, and other combuftible vegetables, and burn them in the very cavity, till the furface is completely converted into a coal: it feldom happens that the tree perifhes by the effect of this operation, and it is always found that this remedy fufpends the progrefs of the decay. It is practifed in the fame manner, and with fimilar fuccefs, on the white oak. When we compare the effects of the actual

tual cautery on the animal ſyſtem, in ſimilar diſeaſes, a new reſemblance is ſeen between the diſeaſes which affect the organic beings of both kingdoms, as well as between the remedies by which they may be oppoſed.—Soc. Philom. No. 27. An. VII.

NEW PUBLICATION.

A Syſtem of Familiar Philoſophy in twelve Lectures; being the courſe uſually read by Mr. A. Walker. Containing, the elements and the practical uſes to be drawn from the chemical property of matter; the principles and application of mechanics; of hydroſtatics; of hydraulics; of pneumatics; of magnetiſm; of electricity; of optics; and of aſtronomy. Including every material modern diſcovery and improvement to the preſent time. London, printed for the author, and ſold by Kearſley in London; price two guineas, 4to. 571 pages, with 47 copper-plates.

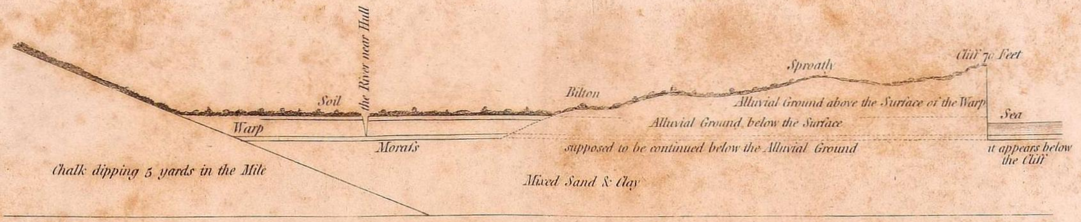
1. Mr. Walker's firſt lecture is introductory, on the nature of light and fire. 2. On the properties of matter. 3. Mechanical powers. 4. Chemiſtry. 5. The atmoſphere. 6. Hydroſtatics and hydraulics. 7. Electricity. 8. The ſame ſubject. 9. Optics. 10. Aſtronomy. 11. Of the moon in particular. 12. Aſtronomy continued.

The public, for a long ſeries of years, has attended with pleaſure and improvement to the clear and perſpicuous lectures of this philoſopher, which makes it the leſs neceſſary to enter, at preſent, into any general character of his performance. Its particular character, or the features of novelty it exhibits, will be found to conſiſt in certain theoretical poſitions, reſpecting the mechanical and chemical effects of light and fire, in the ſyſtem of the univerſe. As I cannot, with ſufficient brevity, give a detail and examination of theſe, upon which the author has employed much time and labour, I muſt leave this part of the ſubject to the ſcientific reader himſelf.

SECTIONS illustrative of the GEOLOGY of the Country near HULL.

Philos. Journal Vol. III. Pl. XIII. facing p. 332.

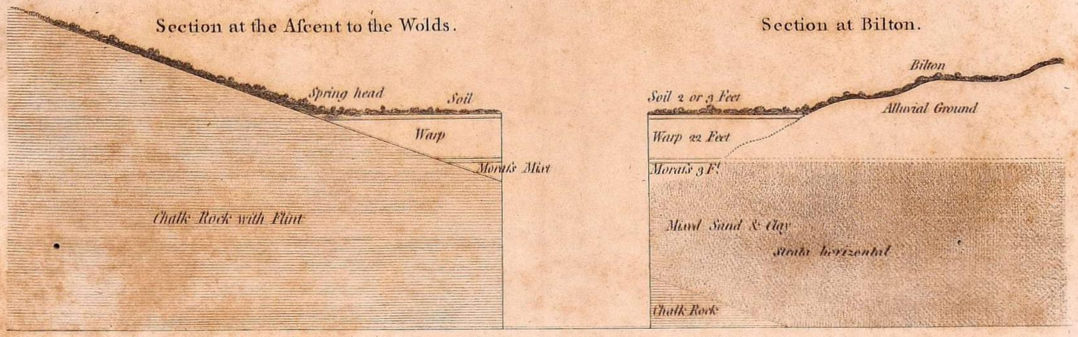
from the Cliff to near Swanland 13 or 14 Miles.



ENLARGED SECTIONS.

Section at the Ascent to the Wolds.

Section at Bilton.



Apparatus for the combustion of the Diamond in oxygen gas.

Fig. 1.

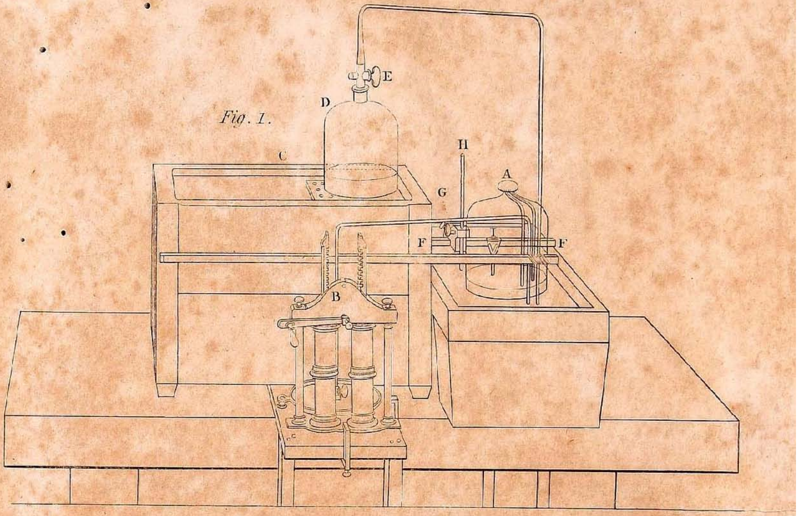
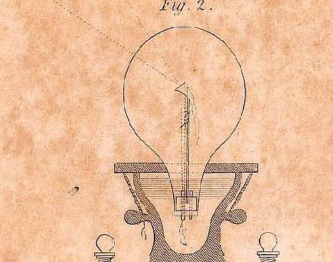


Fig. 3.

Fig. 2.



5 Decimetres.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER 1799.

ARTICLE I.

Chemical Experiments and Obſervations on the Production of Sugar, and an uſeful Syrup from indigenous Plants, by SIGISMUND FREDERIC HERMBSTADT*.

SUGAR has become an indiſpenſable article of conſumption, and, on that account, the monopoly of that commodity from the Eaſt and Weſt Indies, which is favoured by nature itſelf, turns out to be very oppreſſive: and the more ſo, as the demand for ſugar increaſes by the increaſing luxury, even among the poorer claſs of people. For this reaſon, that monopoly becomes an oppreſſive burthen, not only to the individual conſumer, but alſo to the ſtate: for the millions of dollars, which are ſent in caſh to the Eaſt and Weſt Indies without any conſiderable barter, contributes its ſhare to the exhauſting of the public finances. Theſe were, no doubt, the chief motives which, above fifty years ago, induced ſeveral German and foreign chemiſts to inſtitute experiments for diſcovering a native ſubſtance, which might contain, in mixture or combination, a ſufficient quantity of ſugar to be obtained with profit, and thus remove a burthen that daily becomes more oppreſſive. Theſe motives are of themſelves ſufficient to make the home-production of ſugar deſirable; even if it were not combined with another advantage, of great importance in a phyſical as

* Tranſlated from the Neueſte Schriften der Geſellſchaft Naturforſhender Freunde zu Berlin, 4to. vol. II. Berlin, 1799. page 324. 350.

well in a moral view, I mean the abolition of the ſlave-trade, *ſo degrading to the dignity of man*, which may probably in part be effected by ſuch a diſcovery.

The chemical analyſis of vegetable ſubſtances, and the examination of their mixed or combined parts, has ſufficiently proved, that the Eaſt and Weſt Indies are not the only countries which nature has bleſſed with plants containing ſugar. Nature has rather propagated the ſaccharine matter in the products of the vegetable kingdom in ſuch extraordinary abundance, that nothing but perſeverance of reſearch is required, to diſcover thoſe ſingle links in the great chain of natural products, from which the ſaccharine conſtituent part may be experimentally obtained the moſt pure, cheap, and abundant.

Of all the plants hitherto examined with this view, there is certainly none which deſerves to be placed ſo near the *true ſugar-cane*, as the whole genus of the maple-tree ; and of theſe more eſpecially the ſugar-maple *(acer ſaccharinum)*, and the ſilver-maple *(acer daſycarpum. Erhard.)*

Both theſe trees have been made uſe of in North America, more than fifty years ago* ; and, during the laſt eight years†, with uncommonly great profit, for the purpoſe of making a very uſeful ſugar. My own experiments, made rather in the large way with moſt ſpecies of maple, during a period of nearly three years, namely, ſince the winter of 1796, have convinced me, firſt, that all of them may be more or leſs advantageouſly employed for making ſugar ; and, ſecondly, that from the ſugar and ſilver-maples cultivated on German ground, and even of inferior goodneſs, a very good *raw ſugar*, or *muſovado*, in every reſpect like the beſt obtained from the Weſt-Indian ſugar-cane, may be produced ſo cheap, that the pound will coſt no more than eighteen or twenty pennies ‡ ; notwithſtanding the juice was boiled with charcoal, and too great a number of workmen had been employed, which in experimental operations, where nothing but conviction is aimed at, cannot be otherwiſe. But from this it may be preſumed with certainty, that if the proceſs were carried on in the large way, where a ſingle workman, during the tapping, may take care of at leaſt five hundred trees, and where the boiling of the liquor may be performed with pit-coal or turf, the pound of raw ſugar procured from the maple-tree will not amount above *one groſchen*‖.

The

* An account of a ſort of ſugar, made of the juice of the maple, in Canada. Philoſ. Tranſ. No. 171.

Kalin's deſcription how ſugar is made from various ſpecies of trees in North America. Tranſactions of the Royal Academy of Sweden for the year 1751, vol. XIII. Alſo Mémoires ſur le Sucre d'Erable, uſilé dans le Canada, in Nouvel. Oecon. Hiſt. 1757.

† Notice ſur l'Erable à Sucre des Etats Unis, et ſur les Moyens d'en extraire le Sucre, etc. par Mr. Ruſh, in Rozier's Obſervations ſur la Phyſique, etc. tom. XLI. Paris, 1792, page 9, etc.

‡ The Berlin pound is about a half-ounce heavier than the Engliſh avoirdupois ; and 827lb. Berlin = 854 Engliſh. One pound ſterling being reckoned at par equal to ſix dollars fifteen groſch four pennies, Berlin, the above eighteen pennies will give about *two-pence halfpenny* Engliſh money.—Tranſlator.

‖ My experiments for procuring ſugar from maple-trees, made ſeveral years ſince, on the ſuggeſtion of his Excellency the miniſter of State *Struenſee*, are in this place mentioned only by way of digreſſion. I intend to give, perhaps in the next volume of theſe Memoirs, a more circumſtantial deſcription of them, together with

The process of boiling down of the juice is likewise so very simple, that any farmer is able to perform it. The following instance will prove this. *Count Podevills* of Gusow introduced an huntsman to me, for the purpose of seeing and being instructed in the method of reducing the juice of the maple-tree to raw sugar. This man afterwards instructed the manager of Count Podevills's estates, who soon afterwards sent me a considerable quantity of raw sugar from the maple. But as, according to my experiments, only the sugar and silver maple-tree afford these advantages; and as the ash-leaved *(acer negundo)*, the common maple (*acer campestre*), the plane-tree (*acer platanoides*), the sycamore-maple *(acer pseudo-platanus)*, and all the other species, not only yield less juice, but also less rich in saccharine matter, the profits of making of sugar from the maple-tree are not to be expected until the necessary plantations of the sugar and silver-maple have been made, and have sufficiently advanced in growth, which must require twenty or thirty years.

In order, therefore, that Germany, and especially the Prussian territories, may not be deprived for so long a period of the home-manufactory of sugar, it would undoubtedly be highly beneficial to the community, if, among the numerous plants containing sugar, and presented to Germany by nature, some should be discovered, from which a proper substitute for the West India sugar may be extracted, though with less advantage than from maple-trees, and of which a proportional profit may be obtained; at least, until the maple plantations shall have acquired the proper degree of strength*.

From

with their results. I shall only remark, that, according to my experiments, a tree of the sugar and silver-maple from 25 to 27 years age, and twelve or thirteen inches diameter, yields, upon an average, and without particular management, $4\frac{1}{2}$lb. of raw-sugar. The superficial acre being capable of supporting 180 trees, the sugar obtained from an acre will amount to 765lb. In consequence of these experiments, I have made an exact computation. Taking it for granted, that at a medium, one tree should only afford one pound of raw sugar, the charge, including the expences of fuel, wages, and wear of vessels, would be only one grosch $7\frac{1}{2}$ penny (about nine farthings). And one square mile *(German, fifteen in length to a degree of the equator)* would furnish yearly 11,520,000lb. of raw sugar obtained from maple-trees.

* I cannot avoid refuting, in this place, several opinions which have been vulgarly adopted against the maple-tree; namely, that it dies by freezing in hard winters; that it dies after tapping; and, lastly, that it is destroyed by noxious caterpillars. With regard to the first, I refer the reader to the maple-plantations possessed by Count *Veltheim* at *Harbke.* Most of the stocks of sugar and silver-maple are now above thirty years old. They have, therefore, endured the cold winters of the years 1776, 1788, 1793, and will probably not be destroyed this year, since there is not yet any trace of decay observable, even in the branches. Ought not these trees to have been killed long since by the frost, if they are so easily injured by it? With regard to the second case, we may also apply to experience. The trees at Harbke have been strongly drained for two successive years, without any one having died; on the contrary, they have even blossomed and yielded fertile seeds in the subsequent year. This winter, Count *Veltheim* will cause them to be tapped for the third time. In the same manner several trunks of *acer pseudo-platanus*, and *acer platanoides*, in the forests of his royal highness *Prince Henry of Prussia*, at *Rheinsburg*, have been tapped in the last winter of 1798, and not a single one of them has suffered any injury by it. For the purpose of learning how far it may be possible to destroy a maple-tree by draining, I had a couple of them pierced with twenty-four holes. When the liquor had ceased to flow, the borings were left without being stopped, each of these trees was, besides, cut

From thefe motives, I undertook a feries of experiments, two years and a half ago, when I began my examination of the maple-fugar, the object of which was to examine and correct the experiments of former chemifts, as well as to extend our knowledge of the fubject. Of thefe I fhall proceed to give an account.

a. Experiments with India-corn, for the purpofe of making Sugar from it.

The India corn (*zea mays*) is ftated by *Juti**, to contain a very large quantity of true fugar in the knobs of its young ftalks. Mr. *Jacquin* † of Vienna is faid to have repeatedly fucceeded in preparing fugar from the ftalks of India-corn. The fame is afferted by *Marabelli* ‡, in a treatife publifhed on this fubject.

It is alfo reported, that the preparation of fugar from the ftalks of the India-corn, efpecially when grown on a marfhy foil, has been attempted in the large way in Italy; but that the fugar obtained from them was ftill too dear, comparatively with the raw fugar from the Weft Indies.

As my chief end was to clear up this point by actual experiment, I made fome trials with it, of which the following are the refults.

In the fummer of 1796, I cultivated fome India-corn, on a moderately good, but fomewhat fwampy foil. When the young plants were about fix inches high, their leaves, on being chewed, had a tafte refembling that of liquorice, but the ftalks, efpecially near the knots, had a faccharine tafte. The young plants were then cut off above ground, feparated from the leaves and adhering impurities; after which ten pounds of them were cut fmall, then bruifed in a ftone mortar, and the juice preffed out. This liquor weighing five pounds, and ftill poffeffing an herbaceous tafte, was clarified with whites of eggs, by which means it became of a clear yellow colour like wine, and almoft entirely loft its difagreeable tafte. This being reduced by boiling, afforded eight ounces of an agreeably flavoured ufeful fyrup.

b. Examination of the Ears of the India-corn.

As the young ears of the India-corn poffefs a pleafing faccharine tafte, when fcarcely formed, they were likewife fubjected to inveftigation. Ten pounds, freed from all the furrounding leaves, were bruifed in a ftone mortar, and by preffure yielded four pounds of a milky liquor, which could not be perfectly clarified by whites of eggs. By flow evaporation to the confiftence of a fyrup, I obtained nine ounces of a brown pleafantly-tafted fyrup, which, however, was diftinguifhed from the former by containing more mucilaginous matter.

in eighteen places with an axe, and one of them was deprived of all its branches by lopping. Notwithftanding this, both trees continued in a found ftate, and the very next fummer all their wounds healed up of themfelves. It is otherwife with birch-trees, which always wither immediately after a ftrong draining. The third objection is abfurd, and requires no refutation, becaufe contradicted by experience.

* See his Oekonomifche Schriften, part I, page 397, and part II, page 191.

† Crell's Chemifche Annalen, for the year 1784, vol. I. page 96.

‡ Franc. Marabelli de Zea Mays Planta analytica Difquifitio. Pavia 1793.

c. Examination of the Stalks of a more advanced growth of the India-corn.

Twenty pounds of ſtalks of this plant, which had grown larger, were cut ſmall, and then pounded in a ſtone mortar, with the addition of a little water, and their juice preſſed out. This liquor had a conſiderably unpleaſant, herbaceous, and rather ſharp taſte. When purified with white of eggs, and inſpiſſated to the conſiſtence of a ſyrup, it afforded twelve ounces of ſyrup remarkable for its diſagreeable ſaline taſte, reſembling that of a vegetable extract rather than of a ſyrup.

d. Experiments inſtituted with the view of preparing dry Sugar from the India-corn.

In order to find how far it might be practicable to obtain cryſtallizable ſugar from this plant, the ſyrups made from its young ſtalks and ears were rediſſolved, each by itſelf, in lime-water newly prepared, and afterwards gently boiled down; by which management a quantity of impurities was ſeparated. The liquids being ſtrained through a woollen cloth, were again ſeparately evaporated to a thick ſyrup, which laſt was left ſtanding for eight months in a glaſs jar, in a moderately warm place; after which I diſcovered minute grains of true ſugar which had cryſtallized, but could not without difficulty be ſeparated from the large quantity of liquid. In conſequence of this, I inſpiſſated each of theſe ſyrups in gentle heat, to complete dryneſs, and digeſted the dry maſs again in ſix parts of alcohol, with a boiling heat. The fluid, while yet warm, was then haſtily poured through a linen cloth, on which the undiſſolved mucilaginous parts were retained; while, on the other hand, a true ſugar of a yellow colour cryſtallized in the ſpirituous ſolution, in ſmall grains, upon cooling. After this the alcohol was diſtilled off from the remaining fluid; by which treatment an additional portion of true ſugar was obtained from the reſidue by gentle evaporation. On the whole, about two ounces of ſugar were obtained from the ſyrup prepared from the young ſtalks, and $1\frac{1}{2}$ ounce from that of the young ears.

It is therefore ſufficiently proved, that true ſugar can be produced from the young freſh ſtalks, as well as from the young ears of the India-corn. But its ſeparation from the gummy and other parts united with it is ſubject to ſuch great difficulties, and the quantity obtained is ſo ſmall, that the pound of raw ſugar fit for uſe could not be prepared from this material at a leſs price than a dollar (about three ſhillings). It is obvious, from theſe facts, that we are probably never to expect any real advantage, with reſpect to cheapneſs, from this method of obtaining ſugar.

e. Experiments with the Siberian Bear's-breech, for preparing Sugar from it.

The Ruſſian bear's-breech from Kamtſchatka (*ſteracleum ſphondylium Lin. Heracleum ſibiricum*) has been long known among the ſacchariferous plants. This plant is ſaid by Steller* to be, next to the ſugar-cane, one of the moſt abundant in ſugar. By the Ruſſians it

* *Steller*'s Reiſen nach Kamtſchatka, page 84. Alſo in dem Stralſundiſchen Magazin, I. Band. 3tes. Stuck. S. 412.

is

is called *ſweet-herb*, and *katſch* by the people of Kamtſchatka. According to *Gmelin* it does not differ from the common *acanthus*; but according to others it conſtitutes a particular ſpecies (*ſphondylium panaces*). The Kamtſchadales gather its ſtems and the large ſtalks of their leaves in June, and ſeparate them from the foliage, ſcrape off the outer rinds with ſhells of muſcles, and dry them by expoſure to the ſun.

Theſe dried ſtalks are uſually chewed by the Kamtſchadales, to ſuck out the ſaccharine ſubſtance. After the moiſture of the juice has evaporated in the ſun, the external ſurface of the ſtalks becomes covered with a white ſaccharine meal, which is ſeparated by ſhaking them in leathern bags, and is preſerved for the ſame uſes as powder-ſugar. Forty pounds of deſiccated ſtalks are ſaid to yield hardly ſo much as one-fourth of a pound of this mealy ſugar, which conſequently is a ſcarce article. Beſides this uſe, the ſtems and roots of the bear's-breech are likewiſe employed by the natives of Kamtſchatka for making brandy *.

That I might be enabled to make experiments on this ſubject myſelf, the late Profeſſor *Junghans* at Halle ſent me ſome young plants of the *Heracleum ſibiricum*, which the year following were the ſubject of my enquiries.

However, as I did not find the ſtalks of this plant in any reſpect ſo rich in ſugar as thoſe growing in Siberia are ſtated to be, I collected their roots laſt autumn, the quantity being four pounds. Their taſte is ſweetiſh, like that of parſneps. I parted one root from its outer rind, and ſuffered it to dry, but could not perceive any ſaccharine cruſt; for this reaſon, I ordered all the four pounds to be grated, kneaded with water, and the juice to be then preſſed out, which exhibited a ſweetiſh, ſlightly ſharp taſte. It was next boiled with ſome white of eggs, and the clarified juice inſpiſſated to the thickneſs of a ſyrup. A brown and not unpleaſant ſyrup, amounting to ſix ounces, was in this way obtained, which, after three months, afforded brown granular cryſtals of ſugar, contaminated, however, by a foreign flavour. Hence it is proved, that ſugar may be produced from this plant; but that in an economical conſideration it would not be ſerviceable either for ſugar or for a ſyrup, as both would be too expenſive: this plant, when growing in our ſoil, is probably leſs ſacchariferous than that in Kamtſchatka.

f. Experiments for obtaining Sugar from Muſt, or the Juice of Grapes.

Muſt, obtained by preſſure from perfectly ripe grapes, evidently ſhews, by its ſweet taſte, the great quantity of ſaccharine matter it contains, though indeed enveloped in a large proportion of mucilaginous matter. In order to examine the poſſibility of producing true ſugar from it, or leaſt a ſyrup proper for uſe, the following operations were performed:

Eight *Berlin quarts* † of muſt, procured from completely ripe and ſweet wine grapes by *mere draining*, were mixed with whites of eggs, and heated to boiling, and then filtered.

* *Gmelin* Flora Sibirica. tom. I. p. 214.—Allgemeine Reiſen zu Waſſer, etc. XX Theil. page 259.—Stralſun. diſches Magazin. cit.

† The *Berlin quart* holds 58 cubic inches of the former French meaſure; the *Engliſh wine-quart* only $47\frac{3}{4}$ of them.——Tranſlator.

The

The clarified fluid afforded by evaporation three pounds of a ſyrup ſlightly ſour, but not diſagreeable. It was again diſſolved in lime-water, for the purpoſe of depriving it of its diſengaged acid, and lime-water was ſucceſſively added, until the uſual re-agents indicated no ſigns of acidity. The fluid was then again clarified with whites of eggs, and a ſecond time evaporated. By this means I obtained a very pleaſant ſyrup, but could not ſucceed in producing a cryſtallizable ſugar. It muſt alſo be obſerved that it would not be poſſible to prepare this ſyrup in a profitable way, as long as the hogſhead of good wine-muſt can be ſold for fifteen dollars.

g. Experiments made with the Juice of the black and white Birch, with the intention of manufacturing Sugar.

Opinions have hitherto been greatly divided, whether ſugar can be obtained from the juice of birch-trees. Many affirm, but others deny it. This induced me to apply to experiment for a deciſion. According to *Stälhammer*'s * obſervations, eight *kannen* † of the liquor drawn in the ſpring from the common white birch *(betula alba)* yield five and a half ſtop of a ſyrup, which is ſaid to be weaker than that from the maple, but ſtill better than the common brown ſyrup. On the contrary, according to *Kalm*'s experience ‡, the North American black birch *(betula nigra carpinifolia)*, which is alſo called ſugar-birch, yields much ſugar, but not ſo ſweet as that from the maple. By the particular kindneſs of Count Veltheim at Harbke, I received a ſufficient quantity of juice from both ſpecies of birch which ſerved for the following experiments.

Fifty trees of the white birch, from eight to ten inches diameter, being tapped in the month of April, in which this tree flows the beſt, yielded, in four days, 140 Berlin quarts of liquor; which, by evaporation, produced $2\frac{1}{2}$lb of a brown ſyrup, of an unpleaſant taſte, from which no cryſtallized ſugar could be obtained.

On the other hand, ten of the black birch trees being bored, afforded 50 quarts of juice in four days; from which when evaporated a pound and a half of a very good and ſerviceable ſyrup was obtained. Though much inferior to that from the maple, it was nevertheleſs ſuperior to the common; and from this I am inclined to conclude, that *Stälhammer* employed the black birch for his experiments. During the period of four months, a conſiderable portion of ſugar has cryſtallized from the liquor of the black birch; it is therefore proved, that real ſugar may be prepared from the black birch; which, however, would be too expenſive, and with reſpect to quality is far excelled by the maple-ſugar.

* Tranſactions of the Royal Swediſh Academy of Sciences, Vol. XXXV. page 335, ſeqq.

† A *Swediſh kanne* is of 132 Pariſian cubic inches capacity; a *ſtop* is its half, or 66 inches ditto.—— Tranſlator.

‡ Tranſactions of the Royal Swediſh Academy, Vol. XIII. page 151, ſeqq.

(To be concluded in our next.)

Obſervations

II.

Obſervations on the various Kinds of Manure. By JOHN MIDDLETON, *Eſq.**

HAVING made experiments with various kinds of manure, on a farm of which I am the owner and occupier, ſituated at Merton in Surrey, for the purpoſe of aſcertaining the moſt appropriate dreſſing for the ſoil, which is a tenacious loam, on a ſubſtratum approaching towards yellow clay. I am induced, by the regard I feel for the ſucceſs of Britiſh agriculture, to requeſt that you will be ſo obliging as to lay the following obſervations on the ſeveral experiments before the Society for the Encouragement of Arts, Manufactures, and Commerce, for their conſideration. I hope and believe that they will be found not altogether unworthy their attention.

1. *Peat-aſhes* from Newbury, Berks. Of theſe aſhes I have ſpread, in various quantities per acre, 1500 buſhels on wheat tares, ſeeds, and meadow-land, without being able to diſcover any beneficial effect from them.

2. *Coal-aſhes*, ſpread on three or four acres of graſs-land, in March, 1798, produced no viſible effect at *mowing time*; nor have I *ſince* obſerved any.

3. *Wood-aſhes*, the produce of my own fires, when ſpread on the graſs in February, or early in March, I have found to be of ſome, though little, ſervice.

4. *Malt-duſt*, including the duſt from the malt-kilns, I uſed for two or three years to an extent ſufficiently great to aſcertain that the benefits produced by the uſe of it are conſiderable. It may be applied in ſuch a quantity as to enſure *one large crop*; but on meadow-land, even when hay is at five pounds a ton, it only repays the prime coſt.

The quantity which I have uſually laid on has been in proportion of from fifty to ſixty buſhels per acre. The firſt coat of kiln-duſt is ſixpence, and of ſmaller duſt eight pence per buſhel: including the expence of carriage, and ſpreading this dreſſing on the land, it amounts to about two guineas per acre. The extra crop returned me this ſum, but without profit.

5. *Soot.* Of this manure I ſpread eight hundred buſhels over twenty acres of wheat in one year; but I could not, from the ſubſequent appearance of the crop, diſcover whether the increaſe in quantity was equivalent to the additional expence. However, it was evidently of ſome uſe; but to what extent would require more than bare inſpection to aſcertain. By way of compariſon, ſome of the ridges were left without ſoot: they were at harveſt ſcarcely to be diſtinguiſhed from the reſt; but where the ſoot lay in larger quantity than ordinary, as was the caſe in the places at which the loads had been ſhot from the carts, the ſuperior vegetation was very diſtinctly marked. I have, on the whole, formed the ſame opinion with reſpect to this ſpecies of manure as I have already ſtated in regard to malt-duſt, namely, that it returns the coſt price with very little profit.

* Society of Arts, 1799. p. 231, addreſſed to the Secretary.

6. *Soap-*

6. *Soap-makers' waste.* I have tried only one load of this manure on a few roods of ground in four of my meadows. It has not produced the least effect, although it is now three years since it was laid on. Soap-makers' waste, potash, and barilla, are probably held in too much esteem as preparers of the food of plants, by philosophical chemists, of whom it might be wished that a little *practice* were combined with their *theoretical* ideas on the science of agriculture, that they would try their spurious theories by the test of experiment, before they publish them to the world.

I am further induced to consider this kind of dressing for land as of much less utility than is generally imagined, from having been informed by Mr. Russel, junior, that his father, who is a soap-maker of great respectability, at Paris Garden, has used the waste of his own manufactory on his farms in Essex and Kent (in the latter one a clay soil), without discovering that it was of any material benefit to the land; and that he has consequently discontinued the use of it.

The experiments made by major Valley, as reported in the eighth volume of papers published by the Bath Society of Agriculture, seem also to prove that Dr. Hunter's food of plants does not answer *any* of the purposes for which it has been so highly extolled; but, on the contrary, that it is really *hurtful* to corn corps.

7. *Sweepings of London streets.* I have used several hundred loads of this manure on grass-land, and have found it to be of considerable service to the succeeding corps. I have usually laid it in large heaps, and mixed with it a small quantity of horse-dung: and in this state it generates a little heat, though less than might be wished, which helps to decompose or rot the mixture: when thus prepared, it has been spread on the land in proportion of ten or twelve loads per acre.

8. *The soil of privies.* Within the last four or five years this manure has been spread on my land to the expence of about 100l. The proportion from two to four loads per acre. The effect produced by it was *astonishing fertility*; so much so, as to induce me to be of opinion that it exceeds every other kind of manure that can be brought into competition with it, at least for the first year after it is laid on. In the second it is of some service, but in the third its effects very nearly, or entirely cease. From these premises I draw this conclusion, that for land in good condition the application of two loads per acre per annum will continue it in that state for any length of time. And also, that land which has been much exhausted might be restored by laying on four or five loads per acre; after which, a repetition of two loads annually would be found sufficient to keep it in the highest degree of fertility.

9. *Farm-yard dung.* This, when it had been once turned, and become about three-fourths rotten, I have used in the proportion of about *thirteen* or *fourteen loads* per acre; and found it much less effective for *one* year, than *three loads* of night-soil. I believe, that even a load and a half of soil would have been equal to the foregoing quantity of dung. In the *second* year I could not perceive any difference between the dung and the soil.

In the last volume of the Transactions of the Society, page 168, a crop of wheat, amount-

ing to 56 bushels per acre, is said to have been raised by Mr. Henry Harper of Lancashire; which is so much above the general average, that Mr. Harper was at a loss how to account for it. I am inclined to think, that the *night-soil* contained in the mixture with which he dressed the close was the cause of this *wonderful effect.*

He mentions, that the quantity of manure (consisting of *night-soil*, coal-ashes, and sweepings of streets) was eighty tons; and that the close contains eleven acres. The proportion per acre was therefore something more than seven tons. He does not say what part of this proportion was night-soil; but it was probably not less than four tons; a quantity which, as I have before observed, is sufficient of itself to produce one immense crop.

In short, it appears to me, that nature following her general system of reproduction, prepares this matter in the most perfect manner, for the purpose of feeding vegetables, and raising them to the very highest pitch of excellence. And it is certain, that herbage growing under these circumstances is capable of fattening the *largest* cattle in less time than any other.

The importance of this kind of manure being so evident, I am sure the society will feel equally with me the most poignant regret, when they take into their consideration, that ninety-nine parts in every hundred of this valuable article is constantly and most *absurdly* carried by the sewers and drains into the rivers, and thereby totally lost to the purposes of agriculture, for which it is so admirably adapted.

In Britain alone, the quantity of the manure and of urine which is annually thus wasted is astonishingly great; probably not less than five millions of cart-loads: worth to the farmers two millions and a half; and to the community five millions of pounds sterling per annum.

III.

Description of a New Escapement for Clocks. By Mr. Simon Goodrich. *

FIG. 1. Plate XV. represents a front view of the crank escapement.

Fig. 2, a side view of ditto.

The same letters of reference are put to the corresponding parts in each figure.

A A A A, the back plate of a clock. B, the crank, fixed to the end of C, the arbor of the last pinion of the train. D D, two small wire chains, composed of two links only, attached to the crank B, by means of two collets, with plenty of liberty upon it. These chains are fastened to E E, two small springs screwed to F F F, a bar that goes across, and is fastened in the middle to G G, the crutch with which H H, the pendulum, is suspended in the common way. I I, are two screws going through the bar F; the ends of which being

* Society of Arts, 1799, p. 329.

made

made to act against the springs E E, serve to adjust them to a proper distance and degree of strength; and by that means easily to put the pendulum in proper beat. In fig. 1, the pendulum is partly represented by dotted lines, in order to shew the crank, &c.

The advantages of this escapement are, 1st, simplicity of construction; and consequently cheapness. It can easily be adopted by the manufacturers of the common wooden Dutch clocks, as the whole escapement can be formed of a few pieces of wire bent till properly adjusted, without even the necessity of the adjusting screws I I. Therefore the benefits of this invention may extend even to the poorer part of the public.

2dly, Total silence when going: clocks being excluded by many persons to whom and from many places where useful, upon account of their noise.

3dly, Keeping good time, arising from this principle, that the crank always impels the pendulum in the same manner; that is, as it passes through the semicircle under its centre always one way; and as it passes through the semicircle over its centre always the other way; and it recurs in the same situation it set out from, after these two impulses; which being easily made equal by means of the adjusting screws, notwithstanding the small difference in length of the levers acted upon, all the succeeding impulses must also correspond with these, and be equal and regular, as far as regards the escapement itself. In other escapements, if any irregularity exists, or is worn in any one or more of the teeth or pins in the scape-wheel and the pallets, or inclined planes they act against, the impulse given to the pendulum cannot be equal and regular; and the higher the number of the scape-wheel, the greater this chance of imperfection.

4thly, Durability and great certainty of continuance. This escapement does not require oil; which, as it becomes clammy and thick by the dust, is the frequent and well-known cause of the stopping of others: it does not wear itself untrue and out of order. The friction of the collets upon the crank must be very small, as the acting surface is small; and as the intensity of the impulse upon the pendulum is in proportion less in this, as it acts upon a longer lever than in others; also, at the time that the chief force is exerted, as the crank passes over and under its centre, there is very little turning of the collets upon it: so that I judge this escapement, properly made, stands a fair chance of lasting as long as the train itself, without being out of order; which I understand is far from being the state of others.

The ingenious part of the public can no doubt improve upon this application. Of the various methods I have tried, this appears to me to be the best for general use. I can send the Society drawings * of the other methods which I have tried, if by this they should judge them worthy their notice. In the one, the crank, besides its own, can be applied with all the advantages of a dead beat; and in the other, of a detached escapement: but they will not have the advantage of silence.

I was induced to apply this motion to a ready-made clock, in order to try, by the same machine, a point of which I had some doubt; namely, that perhaps, on account of the greater velocity of the acting part, this method might require more power than the common. The

* The drawings here alluded to, as also a model of another escapement by Mr. Goodrich, are reserved in the Society's collection.

 experiment

experiment proved in favour of the crank; though with the common escapement, well adjusted, I kept the pendulum in motion for some days, with as small a power as I could with the crank; yet the same small power with the common escapement was not sufficient to maintain the motion so long as it was with this crank. The weight here used for this eight-day piece, which has kept it going for about six months without once failing, is $4\frac{1}{2}$ lb. It has in it an allowance of more than half for the loss of power from foulness, or other accidents; for three pound was sufficient to work the clock for several weeks when first put together.

IV.

Description of the Furnace of POTT.

To Mr. Nicholson.

Sir,

MUCH has been said on the theory and construction of chemical furnaces, and many ingenious contrivances have been made on that head, especially for philosophical purposes and experiments in the small way; yet, to my knowledge, there is none by which in so simple a manner, and moderately small size, the heat can be increased to such intensity as by Pott's furnace.—I mean *Pott* the famous Prussian chemist, which, though his researches on the more simple earths and stones were chiefly confined to their degree of fusibility, or also of their being fire-proof, may nevertheless be considered as the father and forerunner of those admirable analyses of mineral substances, for which the present period is indebted, after *Margraf* and *Gellert*, to *Gerhard*, *Achard*, *D'Arcet*, and especially to *Klaproth* and *Vauquelin*.

That furnace, it seems, has been long since forgotten; and perhaps it may be of service to the chemical public to be again put in mind of it. On this account I send a drawing, and translation of the description, which *Pott* himself published of his furnace. If you are of the same opinion, and think it may be agreeable to the readers of your valuable and useful Journal, you are requested to insert it.

Sir, your humble obedient servant,

15th October, 1799. N. N.

An Account of Pott's Furnace, given by himself.

When I affirmed in my treatise on the *Lithogeognosia**, that the culinary and common

* John Henry Pott's *Lithogeognosia*, or chemical enquiries into the nature of the common more simple stones and earths, etc. 4to. Berlin, 1784, Continuat. 2d. By the shape of this furnace, it appears that its inside, after being coated, has the form of the cavity of an egg, or nearly elliptical, which was the favourite shape with *Boerhaave*, for furnaces intended to produce a strong heat.

fires

fires are not sufficiently strong for those operations and fusions which I have described in that book, I meant to comprize in that assertion, not only the furnaces of the medical chemists, but even those of the manufacturers of glass and porcelain. For in those furnaces, though the fire be kept up several days, several substances can never be fused, which in my furnace I could bring into the state of fusion by a fire maintained for *two hours* only: such as the *Oriental* and *Bohemian garnets*, and even *hyacinths*.

My furnace is nearly of the same construction as that which Becher has described in his *Tripode Hermetico*, page 32. The body of the furnace, A, fig. 3, plate XV. is made of strong sheet-iron, that it may better resist the action of the fire. Its inside is coated with crude white clay, which is worked up, by means of bullocks' blood, with an equal portion of baked clay.

B is likewise constructed of sheet-iron, and internally coated in the same manner as A, upon which it is placed. Through the small door D the coal is put in. Into the iron tube G, serving as a flue, another, H, of the same material is inserted, which at least should be six feet long. The longer this pipe is, the stronger is the action of the fire; and it should be fastened in the chimney by means of an iron chain, to prevent it from overturning the furnace by its weight.

If it be desired to increase the force of the fire, another pipe, C, must be joined to the door of the ash-pit, E, in such a manner, that the other extremity of C, ending in the form of a funnel, may reach beyond the window, in order that the cold air may be conveyed to the furnace from a considerable distance.

The charcoal, to be employed in this furnace, should be of the size of a hen's or goose's egg: larger or smaller pieces are improper, and may be used for other purposes. The furnace must be filled with the fuel nearly to its top, in order that the crucible be surrounded on all sides by burning coals, and that the fire may thus exert its whole power. Nor must it be forgotten to supply fresh coals at least every eight minutes, or oftener, and to shut the door every time quickly and exactly. In this manner, any substance whose nature is susceptible of fusion, may be fused within the time of two hours.

V.

Process used to separate the Mucilage from Linseed Oil; Methods of dissolving Copal for Varnish, &c. By Mr. TIMOTHY SHELDRAKE.*

I HAD read in some book, the title of which I do not recollect, that linseed-oil might be *purified* by shaking it with water, which would imbibe the impurities, and leave the oil more purified. I tried this experiment by shaking linseed-oil with warm water, and was surprized to find they did not separate, but remained united in the form of emersion. I then boiled them together, and found their tendency to separate diminish. As it is the known property

* Society of Arts, 1799, page 283.

perty of gums, or mucilage, to keep oil and water united in this ftate, I was induced to fufpect the prefervation of mucilage in linfeed-oil.

I had read in Doffie a method of preparing what he calls *fat oil.* It is effected by placing linfeed-oil in a fhallow veffel expofed to the heat of the fun, and ftirring it frequently; in a certain time it lofes its property of drying, thickens, and acquires a degree of tenacity that makes it proper for a fize, or cement, for gilders, &c. A fimilar fubftance is alluded to by Leonardo da Vinci. (Philof. Journal, II. 90.)

Taking the exiftence of mucilage in this oil for granted, I conjectured that the alteration produced in its texture by Doffie's procefs, arofe from the evaporation of fome of its principles, and the more intimate union of the reft, in confequence of that evaporation: to verify this conjecture I tried the following experiment. I filled a half-pint phial full of linfeed-oil, corked and tied it fecurely over with a bladder. This I expofed to the heat of the fun in fummer, during the whole day: after it had remained a few days in this fituation, the upper part of the phial was covered with drops, fimilar to thofe produced by holding a wet bottle to dry before the fire. I then fhook it well, which made the contents look muddy, and fet it to reft again. After a time it became clear, and a portion of tranfparent liquor, like water, lay at the bottom. I then repeated the fhaking and fetting it to reft, till no additional quantity of this fluid was feparated.

By trying this experiment repeatedly upon oils procured from different places, I found that fome oils afforded much more mucilage than others. From fome I feparated a third part of mucilage; from others a pint would not afford more than a table-fpoonful, and fometimes lefs. Whether this difference in the refult was radically in the oils, or from a difference in the proceffes conducted by means fo variable as the heat of the fun, I am not able to afcertain.

Upon trying the fame experiment with nut and poppy oil, I found the fame refult, but in a different degree. The average quantity afforded by nut-oil was, I believe, not more than a third part of the average of linfeed-oil, and the average of the poppy-oil was not fo much as a fixth. In fome cafes, particularly of the poppy-oil, I did not obtain any.

The colour of the oil always diminifhed as the mucilage was abftracted; but the mucilage was always as colourlefs as water. It is a queftion I will not pretend to decide, whether the colour of thefe oils depends upon the prefence of the mucilage, or upon any other principle which is deftroyed by the action of light. I have, in fome inftances, had the oil as colourlefs as water.

This decompofition of the oils, if it may be fo called, is curious, as proving the exiftence of mucilage in them; but as it is very troublefome, may it not be advifeable to prefer thofe which have naturally the leaft mucilage in their compofition?

After pouring the oils from the mucilage, I put feveral quantities of the latter together, and found them mix without any difficulty. I mixed this mucilage with water, and found it unite with it in any proportion, without becoming turbid. I laid it upon plates of metal, expofing

expoſing them to dry in the ſun and before the fire, and when dry, waſhed them with a ſponge and water; but it ſhewed no tendency to diſſolve; though while in a liquid ſtate it ſeemed to poſſeſs all the properties of a gum.

As the proceſſes by which I diſſolved the amber and copal to make the oil varniſhes are to be found in many books, and as it will be better for artiſts to purchaſe than attempt to make thoſe varniſhes, it can ſcarcely be thought neceſſary to detail thoſe proceſſes here; but as I believe the methods by which I diſſolved the copal in ſpirit of turpentine and ſpirit of wine are not known, I ſhall now ſubjoin them.

To diſſolve Copal in Spirit of Turpentine.

N.B. Whatever quantity is to be diſſolved ſhould be put into a glaſs veſſel, capable of containing at leaſt four times as much, and it ſhould be high in proportion to its breadth.

Reduce two ounces of copal to ſmall pieces, and put them into a proper veſſel. Mix a pint of ſpirit of turpentine with one-eighth of ſpirit of ſal ammoniac; ſhake them well together; put them to the copal, cork the glaſs, and tie it over with a ſtring or wire, making a ſmall hole through the cork. Set the glaſs in a ſand-heat, ſo regulated as to make the contents boil as quickly as poſſible, but ſo gently that the bubbles may be counted as they riſe from the bottom. The ſame heat muſt be kept up exactly, till the ſolution is complete.

It requires the moſt accurate attention to ſucceed in this operation. After the ſpirits are mixed, they ſhould be put to the copal, and the neceſſary degree of heat be given as ſoon as poſſible. It ſhould likewiſe be kept up with the utmoſt regularity. If the heat abates, or if the ſpirits boil quicker than is directed, the ſolution will immediately ſtop, and it will afterwards be in vain to proceed with the ſame materials; but if properly managed, the ſpirit of ſal armoniac will be ſeen gradually to deſcend from the mixture, and attack the copal, which ſwells and diſſolves, except a very ſmall quantity which remains undiſſolved.

It is of much conſequence that the veſſel ſhould not be opened, till ſome time after it has been perfectly cold. It has twice happened to me on uncorking the veſſel, when it was not warm enough to affect the hand, that the whole of the contents were blown with violence againſt the cieling. It is likewiſe important that the ſpirit of turpentine ſhould be of the beſt quality. I have never ſucceeded with that which is ſold at the colour-ſhops; but whenever I procured my ſpirits at Apothecaries-hall, I have diſſolved the copal by the proceſs I have deſcribed without difficulty.

This varniſh is of a rich deep colour when viewed in the bottle, but ſeems to give no colour to the pictures it is laid on; if left in the damp, it remains ſacky, as it is called, a long time; but if kept in a warm room, or placed in the ſun, it dries as well as any other turpentine-varniſh; and when dry, it appears to be as durable as any other ſolution of copal.

To diſſolve Copal in Alcohol.

Diſſolve half an ounce of camphor in a pint of alcohol; put it in a circulating glaſs, and add four ounces of copal in ſmall pieces; ſet it in a ſand-heat, ſo regulated that the bubbles may

may be counted as they rise from the bottom; and continue the same heat till the solution is completed.

Camphor acts more powerfully upon copal than any substance that I have tried. If copal is finely powdered, and a small quantity of dry camphor rubbed with it in the mortar, the whole becomes, in a few minutes, a tough coherent mass. The process above described will dissolve more copal than the menstruum will retain when cold. The most economical method will, therefore, be to set the vessel which contains the solution by for a few days; and when it is perfectly settled, pour off the clear varnish, and leave the residuum for a future operation.

This is the brightest solution of copal that I have seen; it is an excellent varnish for pictures, and may, perhaps, be found to be an improvement in fine Japan works; as the stoves used in drying those articles may drive off the camphor entirely, and leave the copal pure and colourless on the work.

N.B. Copal will dissolve in spirit of turpentine by the addition of camphor with the same facility, but not in the same quantity, as in alcohol.

At the time I determined to lay the preceding papers before the society, I conceived that the quick and certain manner in which the vehicle dried was one of its advantages: but as that circumstance has been objected to, and in some cases really is a disadvantage, I have since endeavoured to remove that objection by the following process.

Put a pint of nut or poppy oil into a large earthen vessel; make it boil gently upon a slow fire; put in, by degrees, two ounces of ceruse, and stir it continually till the whole is dissolved.

Have ready a pint of the copal-oil-varnish heated in a separate vessel; pour this by degrees into the hot oil, and stir them together till all the spirit of turpentine is dissipated; let it then be set by till cold, when it will be for use.

It is obvious that as this is a compound of the copal-varnish with the least exceptionable of the drying oils, it will partake of the properties of each of its component parts. It gives less brightness and durability to colours than the varnish will, but more than oil: but as it certainly may be used in painting in the same manner as any other drying oil, and gives more durability and brightness to colours than they can derive from any other oil, it is not unreasonable to suppose that it will prove an advantageous vehicle.

I have mentioned specific quantities of the ingredients; but it is easy to see that the relative proportions may be varied according as it is required to dry faster or slower. It must be remarked too, that wherever the mixture is to be made, both the ingredients should be hot; because if either of them is cold, the mixture becomes turbid, and a part, often the whole of the copal is precipitated: but this inconvenience is avoided by mixing and boiling them together as I have directed. It must likewise be observed, that after some time a spontaneous alteration takes place, which diminishes, and at last destroys, the drying quality of this mixture; and it will, therefore, be adviseable to use it fresh; or at least not to use it after it has been made more than a month, or six weeks.

Analysis

VI.

Analyſis of the Spinel. By Profeſſor KLAPROTH. *

THE ſpinel appears to me to be among thoſe precious ſtones which ought to be placed in the claſs of hyacinths of the ancients. For they do not deſcribe its colour as being a red inclining to yellow, like the hyacinth of the moderns, but as being of a clear violet, and alſo of a roſe colour. We read, for inſtance, in Pliny †, *Multum ab amethiſto diſtat hyacinthus, tamen è vicino deſcendens. Differentia hæc quod ille emicans in amethiſto fulgor violaceus dilutus eſt in hyacintho.* Epiphaius reckons five kinds of hyacinths, of which he terms the third *νάτιβος* ‡, and Salmaſius deſcribes its colour in theſe words, *inter roſeum eſt et dilutiorem.* The modern hyacinth has been confounded with that of the ancients, no doubt, from the following paſſage of Pliny §, *Hyacinthos Æthiopia mittit et chryſolithos aureo colore tranſlucentes:* but this, by a well-founded criticiſm, may be read thus, *Marceſcens celerius nominis ſui flore hyacinthus. Æthiopia mittit et chryſolithos, &c.*; in which caſe, the *aureus color* of the latter, which is known to be the modern topaz, will have no application to the hyacinth, and we ſhall have at the ſame time one contradiction leſs in Pliny.

The ſpinel has hitherto been claſſed among the rubies as the ſecond ſpecies of that genus, the true ruby being the firſt. But ſince Romé de l'Iſle ‖ has ſhewn the different cryſtallization of theſe two ſtones, and has ſhewn that the figure of the ruby, of which Pegu is the country, is an elongated double hexahedral pyramid, modern mineralogiſts have claſſed it among the ſapphires as a red variety, and have conſidered the ſpinel as a particular genus; a diſtinction which the differences of hardneſs and ſpecific gravity appear alſo to point out.

In doubtful caſes, the fracture may determine the ſpecies. The ſpinel has a foliated fracture, the plates being diſpoſed in three different directions; but the ſapphire has a conchoidal fracture on all ſides **.

Chemical analyſis is always the moſt deciſive. But the ſcarcity of the ruby in its natural ſtate, cryſtallized in the hexahedral pyramid, or the red ſapphire, leaves us little reaſon to hope for ſucceſs in this reſpect.

The primitive figure of the ſpinel is an octahedron, or double tetrahedral pyramid. This cryſtallization is rather frequently found in a perfect ſtate, but much oftener with ſome con-

* Beitrage fur chemiſchen keutniſs, vol. II.

† Lib 37, c. 9.

‡ From the Arabian word, ſignifying the colour of red chalk. See Joan de Laet de Gemmis et Lapid. Lug. Bat. 1647.

§ Loc. citato.

‖ Cryſtallographie, I. 213.

** Mineralogy of Eſtner, II. ſect. 1. p. 96, 97.

derable variations. All these different crystallizations are very carefully described by the Abbé Estner *.

The colour of this stone is also much varied, and passes through all the gradations of red. This variety of colour has caused lapidaries to distinguish it by different names, such as the ruby, almandin, spinel, balais, rubicel.

The red colour of this precious-stone is not only very fixed in the fire, but it even becomes deeper by careful ignition of the pale varieties. According to the testimony of Julius Scaliger †, the inhabitants of Zealand are very well acquainted with the method of profiting by this, which is, perhaps, the only reason why we so seldom observe in the rough ruby the fine and beautiful colour of those which have been cut.

With regard to their colour, those spinels are the most rare which are beautifully transparent and colourless, such as the perfect octahedron which Mr. Macie of London preserves in his collection; in the next place are those of a sapphire-blue, such as that of Mr. F. Greville; and next the green, as that possessed by Mr. Hawkins, &c. This stone affords a new proof that the colour of precious stones is a very inferior character for determining their classification.

I found the specific gravity of the spinel in select crystals to be from 3,570 to 3,590.

Though I have published an analysis of the spinel several years ago‡, there are nevertheless several matters which were not, at that time, well known, and appeared to me to require a new course of experiments. The event has shewn me that, in my former work, I had overlooked a substance not suspected to exist in this stone, namely, magnesia. Among other analyses made to determine the quantity of this new principle, I shall here give that which appeared to me to deserve the most confidence.

A. One hundred grains of rough spinel of Zealand, in select crystals, were pounded in a steel-mortar, and then reduced to an impalpable powder by grinding them in a mortar of silex with a little water. The powder being then dried and slightly ignited was found to have gained nine grains by erosion from the mortar.

B. On this powder I poured 960 grains of muriatic acid, and set them to digest. After the acid was evaporated nearly to dryness, I diluted the mass with water, and poured the whole on a filtre. The acid liquor was yellow. I supersaturated it with ammoniac, which threw down brown flocks of iron, that weighed one quarter of a grain after ignition.

C. The liquor separated from this precipitate was evaporated, saturated with muriatic acid, and mixed with oxalate of potash. A precipitate of oxalate of lime fell down, which was carefully collected, placed in a hole in charcoal, and ignited by the blow-pipe. It afforded three quarters of a grain of pure calcareous earth. When this was dissolved in the nitric acid, and mixed with the sulphuric, it afforded sulphate of lime.

* Mineralogy of Estner, p. 73, et. seq. † Exercit. CXVIII.

‡ Beobact und entdek, &c. Berlin, 1789, vol. III. page 336.

D. The

D. The powder thus extracted by muriatic acid, was put into the silver crucible, with ten times its weight of a solution of caustic potash, of which the alkali constituted half the weight. The mixture was evaporated to dryness on a sand bath, and afterwards ignited for an hour. The mass diffused in boiling water, left on the filtre a residue of a fawn colour, which, dried in the air, weighed 54 grains.

E. These 54 grains again treated with caustic potash, and the mass diffused in water, left a residue in very fine powder, which dried in the air weighed 43 grains.

F. The alkaline solutions D and E, which were of a yellow colour, were mixed together, saturated with sulphuric acid, and the precipitate which was formed redissolved with a slight excess of acid. The carbonate of potash occasioned, in this solution, a very voluminous precipitate, which, after having been well washed, was redissolved in the sulphuric acid. The solution resembled a mucilage: by exposure to heat, it was perfectly liquified, and deposited a white powder, which collected, washed, and dried in the air, weighed 95 grains. The acid which had been separated was set aside.

G. I slightly ignited these 95 grains, with three times their weight of caustic potash. The whole being afterwards diluted with water and filtered, a very small portion of residue was left, which, after washing, was dissolved in the sulphuric acid, except a few flocks.

H. The part dissolved by the potash G, was precipitated by sulphuric acid, and redissolved in excess of acid, and afterwards precipitated hot by carbonate of potash. The precipitate being well washed, was again dissolved in sulphuric acid.

I. The sulphuric solutions F, G, H, were evaporated. The liquor having assumed the gelatinous form, indicated that silex had been separated. It was therefore diluted with much water, then concentrated, and the silex collected on the filtre.

K. I evaporated the acid solution of the sulphate. After having added the acetite of potash to obtain crystals*, a perfectly pure alum was formed; but the solution having assumed a greenish colour towards the end, I added prussiate of potash. A slight blue precipitate was afforded, in which the oxide of iron might be estimated at one fourth of a grain. The solution, cleared of iron, was decomposed by the carbonate of potash. The precipitate being again dissolved in sulphuric acid, afforded to the end perfectly pure crystals of alum, which were added to the former.

L. I then undertook the decomposition of the 43 grains, E, and which remained insoluble in the potash. Having put them into diluted nitrous acid, they were dissolved, and left only a small portion of silex. The solution, after having been separated from this last, was mixed with a small portion of the acetite of potash, and left to spontaneous evaporation Some more small crystals of alum were formed, after which the solution afforded only crystals of sulphate of magnesia.

* In order to add to the sulphate of alumine the quantity of potash necessary for its crystallization, I at present employ the acetite of potash. By this means I avoid decomposing part of the alum (which might be already formed) by adding more potash than necessary.

M. To ſeparate the ſulphate of magneſia from the ſulphate of alumine, I ſtrongly ignited the whole in a porcelain crucible for half an hour; after which I diffuſed the ſaline maſs in water, and filtered it. The alumine thus ſeparated, was diſſolved in the ſulphuric acid, and cryſtallized in alum.

N. The ſolution of pure ſulphate of magneſia was precipitated hot by the carbonate of potaſh. The carbonate of magneſia waſhed and dried, weighed $20\frac{1}{2}$ grains; but after ſtrong ignition, there remained only $8\frac{1}{4}$ grains of pure magneſia.

O. The waters uſed in the waſhings (of which that afforded by the decompoſition of the ſulphate of alumine F, had preſerved its yellow colour), were all evaporated to dryneſs. The ſaline maſs, diffuſed in water, again let fall a little earth, which I added to the ſmall quantity of flocks remaining after the experiment G. This being urged by a red heat with cauſtic potaſh, and treated with the ſulphuric acid, was decompoſed into ſilex and alumine.

P. All the alum obtained in the experiments K, L, M, and O, which weighed 665 grains, was diſſolved in water, and the boiling ſolution decompoſed by carbonate of potaſh. The alumine waſhed with boiling water, and dried by a gentle heat, weighed 221 grains. But after being purified by digeſtion in acetous acid, and ſaturating this laſt with ammoniac, it was again well waſhed, and ſtrongly ignited for a quarter of an hour. Its weight was then only $74\frac{1}{2}$ grains.

Q. The ſilex of the experiments I, L, and O, collected and ignited for half an hour, weighed $24\frac{1}{2}$ grains. Deducting the nine grains of increaſe afforded by the mortar A, there will remain $15\frac{1}{2}$ grains for the part afforded by the ſpinel.

This analyſis, therefore, gives for one hundred parts of the ſpinel:

Alumine (P)	- - - - - - -	74,50
Silex (Q)	- - - - -	15,50
Magneſia (F)	- - - - -	8,25
Oxide of iron { (B) 1,25 / (K) 0,25 }	- - - - -	1,50
Calcareous earth (C)	- - - - -	75
		100,50

The increaſe of half a grain, which is here found contrary to the rule, ſince there muſt always neceſſarily be ſome loſs, can only ariſe from the dryneſs of the ſtone in its natural ſtate, which cannot be given to its ſeparate parts even by ſtrong ignition.

I muſt here mention a phenomenon which occurred in another analyſis of the ſpinel, which guided me to the preceding, and of which the deſcription may not be totally uſeleſs. After having treated the ſpinel with potaſh and the muriatic acid, the earth precipitated from the muriate by ammoniac was thrown into a ſolution of cauſtic potaſh. The reſidue was diſſolved in the muriatic acid, and after having ſeparated a ſmall portion of ſilex, the ſolution was concentrated by evaporation.

At

At the end of feveral days fmall groups of cryftals were feparated, compofed of fmall flat tetrahedral prifms, moft of them joined two and two, in the form of St. Andrew's crofs. Others were joined, to the number of three or more, in the form of a crofs.

They were very foluble in water. When decompofed by ammoniac, the precipitate appeared of a light brown, becaufe contaminated by a fmall portion of oxide of iron. This being diffolved in the fulphuric acid, and the new combination concentrated, the cryftals of alum were firft feparated; after which the liquor afforded only cryftals of magnefia, in tetrahedral prifms.

This cryftallization of the muriate of magnefia containing alumine appeared to me to be very remarkable.

VII.

Verbal Procefs of the Converfion of Soft Iron into Caft-fteel, by Means of the Diamond. By CITIZEN GUYTON.*

THE clafs may recollect the account † I gave of the great experiment of the combuftion of the diamond in oxygen gas, in the focus of the lens of Tfirnhaufen, and the new truths I have deduced refpecting the true nature of the diamond; of plumbago, which is its oxide in the firft degree; of charcoal, which is its oxide in the fecond degree; and of carbonic acid, which is the product of its complete oxigenation. Thefe experiments gave our brother the citizen Clouet the idea of feeking a new kind of additional proof, by attempting to caufe foft iron to pafs to the ftate of fteel, by cementation with the diamond.

It has hitherto been confidered as a decided fact, that iron does not melt but in its tranfition to the ftate of fteel, or caft-iron; but in what ftate does the carbone enter into this combination? It may be conjectured that it is in the ftate of plumbago, or oxide of the firft degree; becaufe that which is feparated by acids is found to poffefs the brilliant black colour, and incombuftibility, which form its principal character. Hence it appeared proper to conclude that the carbone entered into this union in the ftate of oxidule; and confequently that the charcoal employed in the cementation of fteel began by parting with a certain portion of its oxigen. Of this there was alfo an indication of confiderable ftrength in the charcoal which had been ufed for this operation: as in fact it has a more brilliant afpect, and refifts incineration nearly in the fame manner as charcoal in the mafs after ftrong ignition in clofe veffels.

* Read at the fitting of the National Inftitute of France, 26 Thermidor, in the year VII. and inferted in the Annals de Chimie, XXXI. 328. whence the prefent tranflation is made.

† Philof. Journal, III. 298.

But

But if the charcoal be really unburned in the cementation of iron, there muſt be a diſengagement of oxigen gas. I have endeavoured to reſolve this queſtion by experiment.

I cemented ſmall pieces of iron in a porcelain retort, which had received by a previous operation a vitreous coating, and conſequently was no longer permeable by the air. Theſe pieces of iron were ſurrounded on all ſides by very dry powdered charcoal of beech.

The retort was placed in the reverberatory furnace with an adjutage, bearing a ſyphon, plunged beneath the ſurface of mercury, under an inverted glaſs veſſel. A large quantity of elaſtic fluid was diſengaged, conſiſting of carbonated hydrogenous gas, and carbonic acid; the latter of which was at the commencement 0,11 of the volume; towards the middle 0,13; and towards the end 0,15.

The converſion of iron into ſteel being but little advanced, after the fire had been continued for three hours and a half, the ſame iron and the ſame charcoal were again put into the retort, and ſubjected to the heat of a large furnace, urged by three bellows-pipes. There was now no more than a very ſmall quantity of gas; but it was carbonated hydrogen, mixed with carbonic acid, with a progreſſive ſlow increaſe; ſo that the latter, which at firſt formed only the 0,07 of the bulk, amounted in the laſt portions to 0,12. The iron was now converted into ſteel, and the pieces were even joined together by a commencement of fuſion.

It is very probable that part of the carbonic acid, collected in this operation, was formed at the expence of the remaining charcoal, with oxigen which was diſengaged; but the conſtant preſence of hydrogen leaves nothing certain in the reſult, but the difficulty of entirely depriving the charcoal of the laſt portions of water it contains.

I muſt here obſerve, that this experiment does not appear eaſy to be reconciled with the opinion of ſome chemiſts, that hydrogen has a greater attraction for oxygen than charcoal has; an opinion founded on the conſideration that coal is precipitated in the eudiometer of Volta, when a mixture of oxygen and carbonated hydrogen gaſes are detonated, and the quantity of oxygen is not ſufficient for ſaturating the two baſes. I ſay that this elective attraction does not take place in my experiment, in which it cannot be doubted that the temperature is ſufficiently elevated to produce water by the union of oxygen and hydrogen, and there is nothing in this caſe to give a preference of the oxygen for the carbone.

This conſideration appeared to me to give a new intereſt to the experiment propoſed by Citizen Clouet. I did not heſitate to uſe one of the diamonds of the polytechnic ſchool for this purpoſe, purſuant to the authority I had received from the council; being perſuaded that if it ſhould diſappear in this operation, by the mere expoſure to an elevated temperature, in contact with iron, without the preſence of air, nor any other oxigenating matter, the fact would well repay the ſacrifice. Beſides which, its form, colour, and irregular cryſtallization, rendered it of little value, even as an object of inſtruction.

Citizen Clouet had himſelf prepared a ſmall crucible of ſoft iron, expreſsly forged out of choſen heads of nails. Its form was that of an eight-ſided ſolid. See fig. 1. Plate XVI. and it was cloſed by a ſtopper of the ſame metal, well fitted, fig. 2.

This crucible was intended to be placed in a Heſſian crucible, furniſhed with a cover,

well

well luted on. Such was the whole apparatus of the experiment; and I cannot better defcribe the refult than by giving the verbal procefs which was drawn up by Citizen Clouet, Welter, and Hatchette.

Verbal Procefs of the Experiment, made at the Polytechnic School, the 25th Thermidor, in the Year VII. *on the Converfion of Iron into Steel, by the Diamond.*

The diamond employed weighed 907 milligrammes (14 grains). As it did not fill the entire capacity of the iron crucible, the remaining fpace was filled with filings of the fame iron out of which it was formed. The crucible was clofed with its iron ftopper, which was driven in, to diminifh as much as poffible the fpace occupied by air.

	grammes.
The crucible and its ftopper weighed together - -	55.8
The filings which covered the diamond - - -	2.
Total weight of iron enveloping the diamond - -	57.8

After having taken off the projecting part of the ftopper*, the crucible was placed alone, and without the addition of any furrounding matter, in a Heffian crucible, and this in a fecond crucible of the fame earth, but the interval between the two crucibles was filled with a filiceous fand, exempt from any mixture of iron. Laftly, the largeft crucible was luted with earth, made of pounded crucibles and crude clay, and the whole was expofed about an hour to the heat of the forge furnace urged by three bellows-pipes.

When all was cold, the crucible of iron was found in the interior crucible converted into a button of caft-fteel. See fig. 3. Plate XVI. It formed, together with the ftopper and the filings, one fimple mafs, rounded, and well terminated, except a few globules which were detached, and of which the weight was only 884 milligrammes.

	grammes.
The button of caft-fteel weighed - -	55.500
Detached globules - - -	0.884
Total weight of fteel obtained	56.384, or grains.

The iron and the diamond weighed before the operation 58.707 grammes, whence it follows, that there was a lofs of iron of about 2.423 grammes. This iron had given the Heffian crucible the colour of plumbago.

(Signed) *Clouet*, *Welter*, and *Hatchette*.

* This portion, as well as the remainder of the bar of which the crucible had been formed, was exhibited to the clafs, to fhew the nature of the iron made ufe of.

The

The fusion of the iron being perfect to such a degree as to exhibit the rudiments of the most beautiful crystallization on its surface, it cannot possibly be imagined that any part of the diamond could remain unchanged in the mass, without entering into a state of intimate combination; because the difference of specific is repugnant to such a notion.

The diamond, therefore, disappeared by the attractive force exerted upon it, by virtue of the elevated temperature to which both were exposed, in the same manner as one metal disappears in its mixture or alloy with another.

Consequently, the diamond did in this experiment afford the same principle as the charcoal, because it produced the same compound with iron.

The conversion into steel is out of doubt, for the button having been laid bare by the mill of the lapidary, a drop of weak acid immediately produced a spot of an obscure grey colour, absolutely similar to that produced on the English cast-steel, and that fused by the process of Cit. Clouet. They who have often touched steels by this kind of proof, long since pointed out by Rinman, have had occasion to remark that the spot on cast-steel*, though very perceptible, is not so black as on steel made by cementation; a circumstance which probably depends on the different state of oxidation in which they may have taken up the carbon.

Explanation of the Figures, Plate XVI.

A, fig. 1, is the plan of the iron crucible.

A′——is its section.

a, fig. 2, plan of the stopper.

a′, —— section of the stopper.

B C, fig. 3, button of cast-steel seen in perspective. At *e* is represented the spot formed by the nitric acid on the part laid bare and polished.

VIII.

Notice of the Experiments made by Order of the Minister of the Interior of France, relative to the Fineness of Tin or Pewter.†

THE approaching renewal of the measures of pewter‡, which are used for wine, and several other liquors, demanded a knowledge, 1. of the degree of purity which the tin ought

* From very frequent trials of steel for working uses, I have been led to adopt the contrary rule, particularly with plate-steel. There is a kind of plate-steel sold in London, at seven-pence per pound, which remains white under the same diluted acid as turns cast-steel as black as ink. The accurate reporter has, no doubt, made his trials with different products from those in our market.—N.

† Communicated to the Philomathic Society, by Cit. Coquebert, and inserted in their Bulletin, No. 30. Fructidor, an. VII.

‡ *Etain*, which word is indiscriminately used by most writers for pewter, or alloyed tin, as well as that which is pure.

to have to secure the health of the consumers; and 2. of the simple and easy method of ascertaining, at all times, the fineness of tin, without injuring the vessels composed of that material.

The members of the bureau of weights and measures (Legendre of the National Institute, Gattey, and Ch. Coquebert) proposed, in consequence, a few months ago, to the minister of the interior, a plan of experiments to be made jointly with those of the Council of Mines (Gillet, Lefevre, and Lelievre) to obtain a solution of these two questions.

These united commissions invited the assistance of Citizens Fourcroy, Vauquelin, and Dillon, and after a great number of delicate experiments made with extreme care, the result of their united labours was a number of new and interesting facts, which served to fix the opinion of the supreme administration of the republic on those points which constituted the objects of enquiry.

It is more especially a point of justice to present a summary of the facts to the Philomathic Society, as seven of the commission are among its members.

Chemical experiments have proved, 1. that tin is more easily dissolved than lead, and before it, in point of time, by the action of wine, and of vinegar; and, 2. that lead is not perceptibly oxided in these fluids, except at the line of contact of the air and the liquor; and consequently at an extremely small surface: 3. that the newest and most acid wine in the neighbourhood of Paris dissolved no more than an inappreciable quantity of lead, after having remained eight or ten days in vessels of pewter containing 18 per cent of that metal.

4. That the effect was nearly the same with vinegar, no perceptible precipitation having been obtained, when the vessels in which wine or vinegar were suffered to remain contained full 18 per cent of lead. In proportion as the vinegar saturates itself with tin, it lets fall a small quantity of tartarite of lead; but the quantity of this precipitate is extremely small, even when the operation is performed with vessels of a large diameter and extended surface.

5. When red wine remains in tin vessels, it loses its colour, which effect is owing to the settling of the colouring matter after its combination with the oxide of tin. This deposition does not appear to contain lead. The taste of bad wine made use of in this experiment was nevertheless improved; but there is reason to think that it was rather by the precipitation of the colour, and the saturation of part of the acid of the wine, than by the presence of lead.

From these several experiments, the commissioners have concluded that the alloy of lead with tin might be admitted in vessels intended to contain wine and vinegar, in the proportion of from 15 to 18 per cent, and that there is no reason to apprehend any injury to the health of those who use such vessels.

The second part of the enquiry was directed to ascertain a process for easily determining the fineness of tin. The hydrostatic balance has long appeared to afford a most certain method, though this instrument has not been employed for such a purpose in any country. Conjectural methods were substituted which depend on the aspect or flexibility of the metal,

or other circumſtances equally uncertain. Chemical analyſis is, no doubt, capable of accuracy; but the proceſſes are long and minute; beſides which, they require that a portion of the veſſel itſelf ſhould be taken off. The examination of ſpecific gravity has none of theſe inconveniences; but to make it the foundation of a legal proof it is neceſſary to know by experiments in what manner tin and lead, united together in different proportions, are affected in this reſpect. It has been ſuſpected that the alloys of theſe two different metals do not exactly poſſeſs the ſpecific gravity which would reſult from taking the ſpecific gravities of each ſeparately. But is the ſpecific gravity greater or leſs than the reſult of computation? Do the two metals penetrate each other, or do their parts on the contrary leave a greater vacancy than before their union? Experiment alone can ſhew this, as the opinion of learned men differ on the ſubject. Kaeſtner, Hauſen, Hahn, and even Lavoiſier in his report on the art of the pewterer by Salmon, were of opinion, that penetration takes place. The common ſpecific gravity reſulting from the mixture, according to the latter, greatly exceeds what would be deduced by calculation from the volumes of the maſſes. Kraft alone adopted the opinion of the dilatation, in the 15th vol. of the Memoirs of Peterſburg; but he ſupported his reaſoning on one ſolitary fact. Thoſe who have elſewhere treated this ſubject, particularly in the Memoirs of the Academy of Stockholm, have confined themſelves to compute without making experiments; and conſequently it became neceſſary to refer the whole to obſervation.

The pureſt tin and lead which could be procured were taken and mixed in different proportions, with the greateſt care to mix the alloy well together, and leave no cavity within. Three ſeries of theſe pieces were made, which were ſubjected to the hydroſtatic balance, and afforded the following reſults.

The alloys of tin and lead have really a leſs ſpecific gravity than would be indicated by calculation. Conſequently, the two metals are ſo far from penetrating each other, that they increaſe their volume when mixed together.

The following is the law of this augmentation as deduced from experiment.

When the alloy of lead was in the proportion of $\frac{9}{10}$, and, conſequently, the tin was $\frac{1}{10}$, the volume of the mixture was augmented, or, in other terms, the ſpecific gravity was diminiſhed 26 thouſandth parts.

8 parts of lead and 2 of tin, augmentation of volume				40 thouſandths
7 ditto of lead and 3 of tin,	-	-	-	48
6 ditto of lead and 4 of tin,	-	-	-	47
5 ditto of lead and 5 of tin,	-	-	-	46
4 ditto of lead and 6 of tin,	-	-	-	45
3 ditto of lead and 7 of tin,	-	-	-	43
$2\frac{1}{2}$ ditto of lead and $7\frac{1}{2}$ of tin,	-	-	-	39
2 ditto of lead and 8 of tin,	-	-	-	33
$1\frac{1}{2}$ ditto of lead and $8\frac{1}{2}$ of tin,	-	-	-	30

1 ditto

1 ditto of lead and 9 of tin,	- - -	23
$\frac{1}{2}$ ditto of lead and $9\frac{1}{2}$ of tin,	- - -	14

The experiments were more multiplied for thoſe mixtures in which the proportion of tin exceeds that of lead; and more eſpecially when the tin contained from 5 per cent of lead to 25 or 30, becauſe theſe caſes offer themſelves moſt commonly in actual buſineſs.

On theſe data a table is formed, by means of which it is very eaſy to determine, by weighing a veſſel in air and in water, what proportion of lead the tin may contain.

This method undoubtedly cannot ſhew the other metals, ſuch as copper, zinc, biſmuth, and antimony, with which the tin may be alloyed; but every one knows that theſe metals are mixed with the pewter in the market in very ſmall proportions; ſome becauſe they alter the colour and quality; and others, becauſe the price being at leaſt equal to that of tin, does not render it the intereſt of the tradeſman to add them*.

CH. COQUEBERT.

IX.

A Chemical Examination of the Bath Waters. By G. S. GIBBES, B.M. F.R.S.†

TWO ounces of the King's-bath water experienced no change of colour when a few drops of the ſpirituous tincture of galls were added to it: on the addition of one drop of a ſolution of ſulphate of iron, it became of a purple colour.

The water which ſerved for this experiment had been carefully preſerved in a well-cloſed bottle.

The ſame quantity of this water warm and freſh from the ſpring experienced a conſiderable change of colour on the addition of the tincture of galls, evidently ſhewing the preſence of iron.

Under preciſely the ſame circumſtances, the water of the two other ſprings which I examined, viz. the Hot and the Croſs-bath waters, produced the ſame appearances.

* As it cannot be expected that this abridged account ſhould contain the various reaſonings and precautions of the gentlemen of the commiſſion, I may, perhaps, have been anticipated on the ſubject of the following remarks. 1. The handle, and ſometimes the foot, of pewter veſſels are made hollow; which conſtruction, if not provided againſt by the examiner, will defeat the purpoſe of this proof. 2. The artiſt may leave cavities for the fraudulent purpoſe of rendering his goods lighter, and conſequently of a finer apparent title than it really poſſeſſes. 3. The denſity of caſt-metal varies according to the heat of the metal, and the coldneſs of the mould. 4. Moſt articles of pewter are hammered, which will increaſe the denſity, though not ſo much as the laſtmentioned cauſe. 5. The ſmall additions which are made to improve the hardneſs and colour may alſo change the denſity. It is affirmed that arſenic, in particular, is very effectual in this reſpect.—N.

† Communicated by the author.

From

From the experiments which I have made, and which I have varied in every way I could devise, as will hereafter appear, it seems that the celebrated philosopher Mayow made his experiments on the waters after they had been suffered to cool. He says, "Quod ad vitriolum, denique spectat, balneum vulgo dictum balneum crucis, item alterum præfervidum nominatum, vitriolum plane nullum continere videntur; etenim si gallæ contusæ aquis thermarum dictarum infusæ fuerint; aquæ istæ colorem purpureum aut nigrum nequaquam habituræ sunt; quod tamen omnino contingeret, si thermæ istæ vitriolo imbutæ essent. Quod ad balneum regis, (sic dictum) istoc vitrioli tantillo imprægnari videtur; quippe si gallæ contusæ, ejus aquæ injiciantur, eadem colore atro-purpureo leviter tingetur." I have found upon repeated trials, that the King's-bath water will assume a purple tinge on the addition of tincture of galls, when warm, and even when it has been recently cooled, but not the least colour after it has remained cold for any length of time. It appears from the experiments which I have made, that a small portion of iron is dissolved by the carbonic acid, and that this acid by evaporating leaves the iron unsusceptible of any change of colour by the addition of galls. Mayow's observations on this circumstance confirms my opinion, and are perfectly consistent with the facts I have observed. He says, "Annotandum est autem, quod minera quædam indolis metallicæ una cum Thermarum prædictarum Scaturiginibus e Terrâ prorumpat; quæ facile in vitriolum converti potest: etenim si sabulo (quod una cum aquis Thermarum e Terrâ erumpens, in fundo balneorum reperitur) liquor quivis acidus superfundatur, idem non sine effervescentiâ satis insigni a menstruo acido corrosum, ex parte aliquâ in vitriolum convertetur; haud secus ac limaturæ ferri a liquore acido corrosæ, contingit: quippe si sabulum istoc balneorum liquore acido impregnatum, infusioni gallarum injiciatur, liquor mox colorem atro-purpureum acquiret: cum tamen, si infusio Gallarum sabulo isti recens a balneis exempto, non vero a liquore acido jam corroso, affundatur, ea nequaquam colorem purpureum obtinebit, indicio utique manifesto, sabulum balneorum metallicum, non nisi a menstruo acido corrosum, indolem vitriolicam induere.

Advertendum est autem, quod sabulum istud thermarum aliquandiu servatum, aerique expositum fponte suâ in vitriolum commigrabit: quippe si istius modi sabulum gallarum infusioni injiciatur, aqua mox colorem atro-purpureum habitura est. Quinimo si idem lingua imponatur, sapor vitriolicus satis manifeste se prodet.

Nimirum spiritus nitro-aereus cum minerâ metallicâ, sive marchasitâ salino-sulphureâ (e quali vitriolum confici solet), sabulo dicto admistâ tractu temporis congreditur, et effervescit; eamque tandem modo alibi ostenso, in vitriolum convertit."

Here we cannot help admiring the extent of Mayow's mind, who, without the aid of modern science, could so accurately describe the causes which produce the vitriol he speaks of. Allowing his spiritus nitro-aereus to be oxygen, his theory is perfectly consistant with all the modern discoveries; the iron, by acquiring the spiritus nitro-aereus, or oxygen, becomes ochre or rust, and is then soluble in acids; or if it (as he observes) be united with sulphur, the sulphur acquires his spirititus nitro-aereus, is converted into vitriolic acid, which combining with the iron, forms vitriol of iron, and is then affected by an infusion of galls.

That

That all this is perfectly confiftent with fact, I hope to be able to fhew by experiments I fhall hereafter relate.

To two ounces of the King's-bath water I added a few drops of a folution of pruffiate of potafh, and no change of colour took place, on the addition of one drop of a folution of fulphate of iron, an evident blue colour appeared. This experiment was made on the Crofs and Hot-bath water with the fame refults.

The hepatic water, which is compofed of 16 grains of fulphuret of lime, and 10 grains of the acidulous tartarite of potafh, is an excellent teft for difcovering metals which are held in folution. Iron precipitated by it is rediffolved by the fulphuric acid. With this teft, I could not perceive the leaft quantity of iron in any of the three waters.

As iron may be held in a ftate of combination in which it may refift the action of the foregoing tefts, I have ufed the following means for afcertaining its prefence.

In Profeffor Bergmann's table of attractions, I find that the fulphuric acid has, after the acids of fugar and tartar, the ftrongeft attraction for iron; I have, therefore, firft added a very fmall quantity of the fulphuric acid, and after a due time for combination, a few drops of the hepatic teft: ftill I could not perceive the leaft trace of iron. This experiment was repeated with other tefts with the fame refults. Had any iron been held in folution by means of the fulphuric acid, the hepatic teft would moft probably have fhewn it. The fmalleft quantity of that acid was added, that it might not fuperabound with carbonate of potafh. I diminifhed even this quantity: but carbonate of potafh will produce a brown precipitate from the teft, if fulphuric acid has been prefent; but that brown precipitate will not be rediffolved by the acid.

The Bath waters do not produce any change of colour in vegetable tefts, confequently they do not contain any acid or alkali in an uncombined ftate. I added one drop of diluted fulphuric acid to two ounces of each of the Bath waters, which, with that fmall quantity of acid, changed the colour of turnfole. Had there been any difengaged alkali, or earth, it would have moft probably deftroyed the effects therein produced.

Diluted fulphuric acid poured into two ounces of the King's-bath water, produced a feparation of minute air-bubbles, which I apprehend are carbonic acid, as lime-water added to another quantity of this water caufed a precipitation. The fame appearances were obferved in the other waters, but their quantities vary confiderably.

Thefe experiments would lead us to conclude that thefe waters contain carbonic acid gas, either in a difengaged ftate, or united to fome alkaline bafis. From fome of the foregoing experiments, however, it appears to be in a difengaged ftate, as the tefts did not fhew the prefence of any alkali, and the carbonic gas was too fmall for them to detect it. We fhall hereafter fhow that very large quantities of elaftic gafes arife with the waters; their heat however prevents any large quantity from uniting with them. The carbonic acid has the greateft attraction for water, and therefore the other gafes are not combined with it.

When to the waters of the three baths I poured lime-water, an evident precipitation took place; which precipitation I, of courfe, imagined was owing to the carbonic acid gas being

united

united to the pure lime. When I added a ſmall quantity of diluted vitriolic acid, the precipitation was rediſſolved, and I obſerved a ſeparation of ſome air-bubbles. Although the Bath waters contain carbonic acid gas, they yet attract more air in cooling. This I aſcertained by expoſing a quantity of the water to ſome air in a large cloſe veſſel, the water having a communicating tube on the outſide. The water in the outſide tube was at firſt on a level with the water within, but after a little time the water ſunk in the outſide tube; proving that the air in the receiver had been abſorbed.

The carbonate of potaſh produced a very copious precipitate in each of the waters of the three baths.

A ſolution of pure potaſh produces only a ſlight precipitation in each. A ſolution of pure ammonia produces alſo in all the waters a weak precipitate. Pure ammonia does not ſeparate calcareous earth from its ſolutions. Solution of carbonate of ammonia produces a much greater precipitate in the Hot-bath water than in the other two. In Profeſſor Bergmann's table of affinities we find that lime has a much greater attraction for the ſulphuric and marine acids than the volatile alkali. Upon a ſolution of muriate of lime in diſtilled water I poured a ſolution of pure ammonia, and no precipitation enſued; but on adding a ſolution of carbonate of ammonia in diſtilled water, a precipitation was plainly perceptible. This laſt experiment ſhews that a double decompoſition took place, in which the carbonic acid left the ammonia to unite with the lime, and the muriatic acid left the lime to unite with the ammonia.

Pure ammonia, however, produces a precipitation in the Bath waters, which ſhews very clearly that other earths beſides lime are contained in them. Now the marine and ſulphuric acids have a leſs attraction for ammonia than they have for magneſia; it is therefore not magneſia which is ſeparated by the pure ammonia. As clay has a much leſs affinity for the marine and ſulphuric acids than ammonia, it appears that clay is ſeparated in the foregoing experiments. I poured ſome pure ammonia on a ſolution of ſulphate of alumine, or common alum, and a copious precipitation enſued. In a former experiment I mentioned that a ſolution of pure potaſh did not produce ſo large a precipitation as the ſolution of the carbonate of potaſh. The reaſon is obvious; for although the pure potaſh would ſeparate the ſulpuric and marine acids from lime, yet that lime would be for the moſt part rediſſolved by the water, and a very ſparing precipitation would enſue. This I proved more clearly by adding to the mixture of pure potaſh and the Bath waters more diſtilled water, which leſſened the quantity of precipitate, though I could not by thoſe means make it entirely diſappear. This ſhews that other earths beſides lime are contained in theſe waters. The precipitates entirely diſappear on the addition of diluted ſulphuric acid.

The oxalic acid produces a copious precipitation when added to the King's-bath and Croſs-bath waters. The Hot-bath waters produce a much greater quantity of precipitate with the teſts for lime than the other two. We have now proved that theſe waters contain a large proportion of lime, and that this lime is combined with ſome acid.

Having ſeparated by means of a filtre the oxalate of lime which was formed in the foregoing

going experiment, I added to the clear fluid which came through the filtre a ſolution of pure ammonia, which cauſed a precipitation. Now from Profeſſor Bergmann's table, and from direct experiments before related, I learn that pure ammonia has for the ſulphuric and marine acids a leſs attraction than magneſia; but pure ammonia, as before mentioned, has a greater attraction for thoſe acids than clay; conſequently it is evident that this precipitation is clay. There appeared this ſeparation in the three waters of the baths, but there is a great difference in the quantity of precipitate. The Hot-bath waters contain more than the King's-bath, and the King's-bath more than the Croſs-bath waters.

When the waters of the ſeveral baths are warm, and freſh from the ſprings, there is an evident diſcolouration on the addition of infuſion of galls, plainly indicating the preſence of iron.

Two ounces of the waters freſh from the bath were mixed with a ſolution of ſugar of lead; each produced a conſiderable milkineſs, and the colour remained perfectly white. On the addition to each of a quarter of a grain of ſulphuret of potaſh, an evident diſcolouration took place, indicating the preſence of ſulphur. From this, as from many other experiments, I am convinced that there is no ſulphurated hydrogenous gas produced by the Bath waters.

When ſilver-leaf was placed in a veſſel containing theſe waters freſh from the pumps, it remained of the ſame brightneſs and white colour, but on the addition of ſulphuret of potaſh it aſſumed an almoſt black colour.

(To be continued.)

X.

Conſtruction of a Lamp for Burning Tallow. By MR. WILLIAM CLOSE.

To MR. NICHOLSON.

SIR,

THE conſtruction of a lamp for burning tallow ſome time ago engaged my attention, and my endeavours were attended with ſome degree of ſucceſs. As the contrivance is very ſimple, and likely to ſave conſiderable expence in the article of illumination, I have ſent you a deſcription of it, which I hope may not be unacceptable to ſome of your readers.

It was the peruſal of the obſervations formerly made by yourſelf upon the ſame ſubject * that firſt induced me to make the attempt.

I am, ſir, your humble ſervant,

Dalton, Sep. 30, 1799. WILLIAM CLOSE.

* Philoſophical Journal, I. 67.

A, repre-

A, reprefents a cup made in the form of a cone: it contains the tallow, and is fupported, with the point downwards, by the thumb-fcrew *i*, upon the piece of iron D, which is firmly fixed into the circular wood bottom E. The wideft diameter of this cup is about two inches and a half, and the diameter of a fmall aperture in the point is rather lefs than one-eighth part of an inch. This cup muft be made of iron, brafs, or copper, and the joint on the fide clofed with hard-folder.

A circular plate of iron is made to fit the wideft diameter, and firmly fixed therein by the fides of the cup being turned over it a little: near one fide of this circular plate, a circular hole lefs than one inch in diameter is made, and into it is fixed the ring *d*, which forms the mouth of the cup, and may be clofed with a cork, &c.

a b reprefents a piece of wire which paffes through a hole made in the circular piece, and through the aperture in the point of the cup. This wire is rather more than one-eighth part of an inch in diameter; it converges near the point, and exactly clofes the fmall aperture when thruft down: it ferves to regulate the defcent of tallow into the cup B, according to the quantity confumed by the flame; and therefore when it is required not quite to clofe the aperture, it is drawn up a little, and a fmall fpring of brafs, in the infide of the cup at C, preffes againft it, and holds it in the place. The fpring paffes through the circular plate, and is fixed on the outfide by a fmall fcrew.

B reprefents a fmall cup, in which the tallow is burned: it is about one inch in diameter, and about half an inch in depth. Into the bottom of this cup is foldered the tube *f*, which is about two inches and a half long, and flides into the tube *g*, which is foldered into the bottom of the cup *c*.

e reprefents a piece of bended wire, which fupports the wick of the lamp. The ends of this piece of wire are thruft into a piece of foft wood, fitted into the tube *f*. Another tube, reprefented by *h*, is foldered to one fide of the cup B, above the brim: the ufe of this tube is to contain a quantity of clean wick, and to ferve for a handle to lift the tube *f* out of the focket *g* when the lamp is to be lighted.

C reprefents a cup to receive any tallow that may chance to run over the fides of the cup B. It is rather more than one inch deep, and two inches in diameter.

Laftly, by the help of the thumb fcrews *i i*, the height of the cups B, C, and A, and the diftance between B and A, may be regulated at pleafure.

A wick of cotton being put into the tube *h*, and brought through the ring *e*; a quantity of tallow put into the conical cup; and the fmall cup filled with melted tallow; the lamp may be lighted: if the point of the cup A be raifed two or three inches above the brim of the cup B, and the air in the room at reft, the tallow in the cup A will be fufed in a few minutes, and if the wire *a b* be properly adjufted, a conftant fupply of tallow will *drop* to the flame.

Small particles of duft and other impurities in the tallow fometimes impede the drops, but the light continues undiminifhed, until all the tallow in the little cup is confumed, and the

the cup is then eaſily filled again by holding up the wire, after which the drops muſt be regulated again. To leſſen this inconvenience as much as poſſible, nothing but clean tallow, or hogs' lard, muſt be burned.

Tallow and hogs' lard will burn with a very clear bright flame, of the ſame intenſity, for a long time.

The ſmall cup, when detached from the reſt of the apparatus, will ſupply the different uſes of a candle: it may be carried about by the tube *b*, or, occaſionally, placed with the tube *f* in a ſmall wooden ſtand. The tallow is not very liable to be ſpilled.

Every time before the lamp is lighted, a new portion of wick muſt be drawn through the ring. This is not eaſily effected when the tallow is cold; therefore, to avoid this trouble, it is the beſt method always to draw up the wick immediately before the flame is extinguiſhed; for which purpoſe, ſmall forceps, made of one piece of bended iron, will be moſt convenient.

Laſtly, it may be neceſſary to remark, that after the lamp is lighted, when the tallow in both cups is cold, the tallow in the ſmall cup muſt be broken and ſtirred up, that a ſufficient quantity may be fuſed immediately to ſupply the flame; and the ſooner to fuſe the tallow in the conical cup, the flame ſhould be raiſed near to its point, and when there is a ſtrong current of air in the room, in that ſituation it ought to remain *.

XI.

Report made to the Inſtitute of Sciences and Arts (at Paris), on the 29 *Pr arial, in the Seventh Year (June* 17, 1799*), in the Name of the Claſs of Phyſical and Mathematical Sciences, on the Meaſure of the Meridian of France, and the Reſults which have been deduced to determine the new Metrical Syſtem* *.

(Concluded from p. 324.)

MEASURES of ſurface are eaſily deduced from the determination of the length of the metre, which is the baſis of the whole ſyſtem. But this is not the caſe with meaſures of

* Count Rumford aſſured me ſome time ago, that tallow will burn very well in the common fountain lamp, provided the baſon, or receptacle into which the tallow flows, is ſufficiently capacious to hold enough of the material to ſupport the combuſtion till the tallow in the reſervoir is fixed. The principal inconvenience I found on trying this, was the interception of a large quantity of light by the baſon itſelf, which, nevertheleſs, I think may be almoſt entirely ſucceeded, though perhaps not quite as perfectly as in a candle. Tallow ſold retail at the price of candles coſts ten pence a pound, which in the middling-ſix will burn above 40 hours. Such an illumination will therefore coſt leſs than one farthing an hour. I do not know the price, conſumption, and quality of oil, but eſtimate it at about half that of tallow. Some better information reſpecting both ſhall be given in our next number.—N.

† Abridged from the Journal de Phyſique. Fructidor, An 7.

 weight

weight. The delicate and laborious taſk of determining the unity of weight was entruſted by the National Inſtitute to Lefevre Gineau, together with Fabroni of Florence; and a ſpecial commiſſion was afterwards employed in examining all the regiſters of obſervation and experiment, and verifying all the computations. The unity of weight muſt neceſſarily conſiſt of ſome ſolid, the magnitude of which ſhall be determined from the lineal meaſures of the ſyſtem, and the material of which ſhall be ſuch as can at all times be procured of one and the ſame uniform denſity. The magnitude of the ſolid adopted for this purpoſe was the cube of the decimeter, and the ſubſtance itſelf diſtilled water. Hence the experiment was reduced to determining the relation between the new unity of weight and thoſe of the ancient ſyſtem, by an actual experiment with ſuch a ſolid of water. Experimental philoſophers are aware that there are two methods of doing this; that is to ſay, by actually weighing the water contained in a veſſel of known dimenſions, or otherwiſe by finding the weight of the quantity of water diſplaced by a ſolid, of which the dimenſions muſt alſo be known. The latter method was preferred, and with juſtice, on the preſent occaſion.

A hollow cylinder of braſs, internally ſupported by edge-bar-work, was conſtructed by C. Fortin, together with a guage capable of aſcertaining longitudinal meaſures, to the preciſion of the four thouſandth part of a line of the ancient meaſure. On the baſes of this cylinder were drawn twelve diameters, interſecting each other at equal angles, and each diameter lying in the ſame plane with a correſpondent diameter upon the oppoſite baſe. Three concentric circles were drawn on each baſe, alſo correſponding by pairs with each other. The points of interſection, including the centre itſelf, were conſequently 37; and by meaſuring all the ſeveral diſtances between each of theſe points, and its correſpondent point on the other baſe, the true figure of theſe boundaries, and mean length of the cylinder, were then deducible. Eight circles were alſo deſcribed on the convex ſurface, and twelve right lines were drawn joining the extreme points of the diameter that had been drawn on the baſes. Theſe lines and circles afforded ninety-ſix interſections, to every pair of which the guage being duly applied, gave the meaſure of forty-eight diameters. From all theſe meaſures, ſuitably reduced, it was found that the volume of the cylinder at the temperature of 17,6 of the centigrade thermometer was 0,0112900054 of the cubic metre, or more than eleven times the magnitude of the intended unity.

The balances uſed for weighing were extremely accurate. One of theſe, charged with rather more than two pounds poids de marc, in each baſon, ſhews the millionth part of the weight; that is to ſay, one fiftieth of a grain, and it turns with one tenth of a grain when each arm is loaded with 23 pounds.

The weights were arbitrary, though nearly the intended ſtandard, it being of more conſequence that they ſhould be adjuſted to the moſt preciſe equality, than that they ſhould agree with any definite weight. The ſub-diviſions were in the decimal order.

The cylinder was made hollow, in order that it might load the balance as little as poſſible, under a given magnitude, but it was neceſſary that it ſhould be heavy enough to ſink in water.

There

There was a communication between the external air and the inſide of the cylinder, by means of a tube, which ſerved to ſuſpend it; and the weights being of the ſame material, the reſult in air was the ſame as it would have been in vacuo.

In order to avoid all dependence upon the relative lengths of the arms of the beam, the weighing was always performed in one and the ſame ſcale, the thing itſelf being firſt counterpoiſed, and afterwards taken out, in order to admit the equivalent weight in the ſame ſcale. By 53 experiments, the weight of the cylinder proved to be at a mean 11,4660055 of the arbitrary unities, and the extreme difference among theſe experiments did not amount to 45 millionth parts of the unity. The more difficult operation of weighing the cylinder in water was repeated thirty-ſix times, and the mean apparent reſidual weight proved 0,2094190 unities with no greater abſolute difference between the extremes than before.

The reporter enumerates the various corrections to which this apparent weight muſt be ſubjected. In the firſt place he obſerves, that the air ſupports the counterpoiſe; and does not ſupport the body plunged in the water. Secondly, the apparent weight expreſſes not only that of the cylinder, but likewiſe of the air contained in its cavity. Thirdly, regard muſt be had to the denſity of the water, as governed by its temperature. The experiments were made by ſurrounding the veſſel that contained the water with pounded ice, which kept the temperature of the water itſelf at three tenths of a degree of the centigrade thermometer above the freezing point; but the reſults were reduced to the maximum of denſity of water, which by another courſe of experiments was found to be at the fourth centigrade degree, conformably to the experiments of Deluc. Laſtly, it was requiſite to allow for the expanſion or contraction of the braſs cylinder, by the difference of temperature at the time of admeaſurement, and the ſubſequent experiment.

After all reductions, it was found that 11,2796203 cubic decimetres of water at its maximum of denſity weighed 11,27, and that one ſingle cubic decimetre of water at its maximum of denſity weighs 0,9992072 of the unity, which is the true kilogramme of the new metrical ſyſtem.

It remained then to determine the relation between the arbitrary unity made uſe of, and the ancient French weights. For this purpoſe the ancient pile weighing fifty marcs, called the pile of Charlemagne, was examined. The whole pile repeatedly weighed was found to be equal to 12,2279475: whence it follows, that each unity is equal to 18842,9088 grains poids de marc, and that the true kilogramme, or weight, of one cubic decimetre of diſtilled water, taken at its maximum of denſity and weight in vacuo, that is to ſay, the unity of weight, is 18827,15 grains. The reporter adds in a note, that according to theſe experiments, the foot cubic (French) of diſtilled water, taken at its maximum of denſity, is 70 pounds 223 grains; and at the temperature of three tenths of a degree, it weighs 70 pounds 141 grains; and at the temperature of melting ice it weighs 70 pounds 136 grains.

The pile of Charlemaine, though very accurately made for the workmanſhip of the fourteenth century, at which time it is pretended that it was made, or renewed, is not accurately the ſame in all its parts. The mark taken as the fiftieth part of the whole pile proved to be

be 0,2445589 of the unity. The hollow mark was 0,2445127; and the ſolid mark, 0,2444675. Whence the differences are: between the mark deduced from the whole pile and a hollow mark 0,87 grains; between the ſame and the ſolid mark, 1,87 grains; and between the ſolid mark, and the hollow mark, 0,85 grains. The mark which the celebrated Tillet uſed in his experiments on the weights of different countries, made in 1767, and inſerted in the Memoirs of the Paris Academy for that year *, was different from this pile, though the reporter does not ſay what was the difference.

The ſtandards preſented to the Inſtitute by this commiſſion, were the metre in platina, equal at the temperature of melting ice to 443,296 lines of the toiſe of Peru, this toiſe being ſuppoſed to have the temperature 16 and a quarter, as has been already obſerved.

Beſides this extraordinary ſtandard, other ſtandards made of iron were alſo preſented for uſe on common occaſions. It is recommended that the operations of adjuſting meaſures of different metals to theſe ſtandards ſhould be performed at or near the 15th degree of the centigrade thermometer, becauſe the ſubſequent variations, either to freezing or to a conſiderable heat, would then produce a leſs difference between the different kinds of metal.

The ſtandards of weight were a kilogramme of platina intended for the legiſlative body, and to be preſerved with the moſt ſcrupulous attention for very important occaſions, and ſeveral other kilogrammes of braſs, made with the ſame exactneſs, and intended for civil uſe. Theſe two kilogrammes of platina and of braſs being truly adjuſted, are not equal in air, but only in vacuo. The difference in air is, that the braſs weight is about one grain and two thirds lighter, on account of buoyancy.

Such are the ſtandards which have been produced with great labour, from a courſe of obſervations, experiments, and deductions, capable indeed of being repeated, though not likely to be again performed by a leſs power than that of a public government. What might be the limit of difference between theſe reſults and others which might be had by ſuch a repetition, can therefore be only eſtimated from a ſedulous examination of the particulars of this arduous enterprize. It will not, however, be neceſſary, for the preſervation of the reſults of Cit. Mechain and Delambre, to recur to the actual ſtandards which have been here preſented; for, as the reporter points out, it will be ſufficient to render the operation of renewal more eaſy if the length of the ſimple pendulum at a known place be expreſſed in parts of the metre. Cit. Borda, Mechain, and Caſſini, have determined this for Paris at the national obſervatory, with an apparatus which will be deſcribed in a memoir of Borda hereafter to be printed. The length of the ſimple pendulum which beats ſeconds at Paris was found to be 0,2549919 of the module ſuppoſed to be at the temperature of melting ice: whence it is eaſy to conclude that this length is 0,993827 of the metre.

* See alſo the Firſt Principles of Chemiſtry; and my Chemical Dictionary, art. Balance.

On

XII.

On the Web of the Garden-Spider. By a Correfpondent.

TO MR. NICHOLSON.

SIR,

YOU have, probably, often remarked the peculiar conftruction of the web of what, I think, is called the garden-fpider. This web is compofed of threads radiating from a centre, and nearly equidiftant; thefe are interfected by others, which bear fome refemblance to concentric circles. I remarked, feveral years fince, a very ftriking difference between the circular and radiating thread; if the former be touched with the finger, they will adhere to it fo tenacioufly, as fometimes to break upon withdrawing it, although their elafticity is fuch that they may be extended to more than double their ufual length; which, however, they inftantly refume, upon quitting the body to which they adhered. On the contrary, the radiating threads appear totally deftitute of the adhefive property, and never indicated the leaft difpofition to attach themfelves to any fubftance I prefented to them. Having remarked that the infect often doubles, and fometrimes triples the radiating threads; that thofe which ferve as boundaries to the web, to which the extremities of the former are faftened, are frequently compofed of feven or eight fingle threads united by the fpider, fo as to form but one (which, as well as the former, is not poffeffed of the adhefive property); and that thofe of the circular kind are always fingle; I was at firft induced to attribute the abfence of the adhefive property in the two former to their exerting this property upon each other. To fee how far this idea would agree with facts, I united feveral of the circular threads; but upon trial, no diminution of their tendency to adhefion was perceptible. In the almoft innumerable experiments I have made fince this phenomenon (if it can be termed fo) was firft obferved, the refults have been uniformly the fame, not only when the finger has been employed, but with every fubftance with which the experiment has been tried.

It appears that this remarkable difference is not occafioned, nor in any perceptible degree influenced, by the ftate of the atmofphere, nor by the fize, nor confequently by the age of the fpiders*.

I am, Sir, with great refpect,

Your humble fervant,

J. L.

* This fact is fomewhere mentioned, but I cannot recollect the place. It is affirmed that the fpider walks only on the radial threads.

In anfwer to the queftion in the poftfcript, requefting me to indicate an elementary chemical work containing directions for conducting the various chemical proceffes, defcriptions of apparatus, and a full explanation of the new chemical nomenclature, I muft obferve, that we have no modern book which defcribes the proceffes with the minutenefs and fidelity of Boerhaave, or Shaw, in their lectures; but that the elements of Chaptal, and of Lavoifier, are found difcurfive treatifes, written according to the modern fyftem. Tables of the chemical nomenclature have been publifhed in Englifh, by Dr. Pearfon and others.—N.

Pla

XIII.

Plan of Experiments which were made in the Garden of Plants upon Sheep, and other Domestic Animals. By DAUBENTON *.

If all the experiments of which I prefent the plan were but a project, I fhould be cautious of fpeaking of it to the Inftitute, becaufe a fimple project would not be worthy of their attention: but it is now a confiderable time fince I have been employed in thefe experiments. I have already made a great number, many of which have afforded refults fufficiently interefting to be publifhed. I purpofe to communicate thefe to the Inftitute; and have therefore thought it neceffary to defcribe the methods I have followed, in order that they may be enabled to judge what degree of confidence they deferve.

In the meeting of the 24th Thermidor, in the year iii. of the French republic, the profeffors and adminiftrators of the National Mufeum of natural hiftory gave to my difpofal, for the purpofe of making experiments on fheep, a piece of ground and building, which were purchafed of Cit. Leger, and forms a court-yard. I have altered and appropriated it to fheep, and other domeftic animals, upon which I had before made fome trials, which afforded experimental facts always to be depended on, and often ufeful to the advancement of our knowledge. My age and infirmities having prevented me, for many years, from going to Montbard, I had fufpended this work; but the facilities with which my brethren have procured me at the mufeum have engaged me to refume and continue it.

During the fpace of eight-and-twenty years, which I have employed in the improvement of fheep, I have always feen with regret that cultivators have never employed thofe remedies which have been fuggefted for the treatment of this animal during illnefs, becaufe they have been thought too expenfive. In fact, they cannot be expected to expend as much in curing a difeafed fheep as it would be worth if in health: their treatment muft, therefore, neceffarily coft much lefs; for it is not poffible to cure every diforder. I have, however, difcovered a method of curing the moft common difeafes of fheep, at a much lefs expence. I have been obliged to fufpend thefe refearches; but the advantages which I have fince found in the mufeum, has enabled me once more to refume this work, which is now nearly terminated.

It has been affirmed that the goat willingly cohabits with the ewe; and the ram with the fhe-goat: but a fhe-goat has been in my fheep-fold, near Montbard, during a number of years, with a flock of rams, without ever having yet produced young. If the goat and the ram were of the fame fpecies—if they were to couple and prove fruitful, what varieties we fhould fee in the productions of this fpecies! fome would be found with horns refembling both to the goat and the ram, or they would be covered with goats'-hair and wool. But the diftinctive characters of thefe two animals have never been confidered as equivocal by any one; on the contrary, fo many perfons have affirmed that they breed together, and this opinion is now become fo general, that it is of fome intereft to natural hif-

* Memoirs of the National Inftitute of France, I. 377.

tory to ascertain the facts of this supposed cohabitation, and to learn, in case it takes place, what is the produce. For this purpose, I put a ram with a she-goat, and a ewe with a goat.

I gave an account to the Academy of Sciences, in 1779, of the experiments which I had already made, to ascertain what foods might be eaten by sheep without injury, and also that which would prove noxious. I am enabled at the Museum to extend these experiments to a great number of plants which I could not find in the district of the province of the Côte d'Or, where I have operated, during a long course of time, on the improvements of sheep.

There are convincing proofs, that the time the sheep remain folded in corn-fields, and in the meadows, greatly augments their produce; yet the practice of folding sheep on such grounds is not practised in the greater part of the provinces of the French republic. If this were done on all the fields, we should probably be able to raise a sufficient crop for the consumption of the republic; or, at least, we should not be obliged to import so great a quantity from foreign countries. I have entertained the opinion, that we might extend the custom of folding or penning sheep, if a small flock were to be so managed in the Garden of Plants; people from all the provinces, who visit that place, would then see the manner in which a park or fold is constituted, how they change the place, the cottage where the shepherd sleeps who guards it, the dog-house, &c.

They would also observe the difference between the produce of the fields, which had been thus treated, and those which had not; for to extend the use of a practice so useful, so important, and so necessary as the parking of sheep in corn-fields, and in meadows, it is necessary to employ every means which can contribute to it. I have caused very circumstantial experiments to be made in my sheep-farm, near Montbard, in the province of the Côte d'Or, on the produce of lands cultivated in this manner: I have compared the crops of fields and meadows which had been parked, with the crops of neighbouring lands on which sheep have not been fed, and with those of other lands which had neither been parked or manured, and also with lands which had only been manured without folding the sheep upon them. Incontestable proofs must necessarily be given of the advantages of parking, to induce those people to employ it who may not be convinced of the great benefits they would reap from it. I cannot too often observe, that if all the sheep which are in the republican territory were to be thus treated, it would greatly augment the quantity of our first and greatest subsistence. This consideration surely merits the particular attention of government.

It has been affirmed, that the flesh of the hogs of Siam* is more delicate and white than that of the common hogs. This animal has been variously spoken of. In some of the republican provinces where there were formerly some, there are now none; and though they are reared in other provinces, yet these animals form no part of the commerce of the dealers in swine of Paris, who can give no reason why they are not. I shall make trial of a hog of Siam, to ascertain whether they are worth being reared for common consumption.

* I suppose this to be known in England by the name of the Chinese breed.—N.

The

The greater part of the wild rabbits are deftroyed, which is certainly productive of much benefit to agriculture; but it is difficult, at prefent, to find rabbits whofe flefh has fo good a tafte, or fo high a flavour. The flefh of the rabbit is neverthelefs wholefome; it agrees with all conftitutions; it is eafy of digeftion, and, confequently, good for convalefcents, whofe appetite it revives when the flavour is good. I fhall make refearches, for the fake of convalefcents, and of thofe whofe ftomachs are weak, into the means of rendering the flefh of domeftic rabbits as agreeable, if poffible, as that of the wild rabbits; or, at leaft, to take away much of the expence and time required before the domeftic rabbits are fit for the table, and the bad qualities which arife from the kind of food, which is more effectual to promote their growth than to give their flefh an agreeable tafte. It is well known that fheep fed with lucern have their fat of a yellowifh colour, and a difagreeable tafte; whilft thofe which are fed with trefoil, have their fat alfo of a yellowifh colour, but of a good tafte. It is neceffary to try other plants to know their effects with regard to the qualities of the flefh and fat of animals. I fhall make a great number of thefe trials upon rabbits; as they will be more eafy and fpeedy than that of fheep or kine. They will at leaft indicate thofe foods which will communicate the moft defirable qualities to the flefh and fat of cattle. Rabbits increafe and grow in lefs time; they are fed with lefs expence; from all which qualities they are preferable to other animals for trials of this nature. Rabbits are expofed without regret to the moft dangerous experiments, becaufe they coft fo little. We do not fear to make trials with the moft noxious plants on thefe animals, and even to repeat and modify them, in order that we may not be obliged to make more than one experiment on animals of greater value, fuch as the fheep, the hog, &c.

Columella has made mention of hens with five toes. This race of the fpecies of the cock and hen exifts at prefent. It appears that the fifth toe is a monftrofity; but there is reafon rather to be furprized that this monftrofity fhould have been perpetuated from generation to generation, during fo great a number of years, and that they fhould have been a conftant race, well known to be of value before the time of Columella; if they have not degenerated, they furely deferve to be multiplied. I propofe putting them to various trials. We know not whether the race of fowls without rumps, and confequently without tails, are not as ancient as thofe with five toes; but they really exift: and this alfo is a monftrofity. There is a race of fowls, which they call negroes, becaufe they have the comb, the wattles, the fkin, and the periofteum, if a black colour. Are there not means of making this colour difappear, or at leaft of weakening it? Experiments made on thefe fowls would be equally interefting with thofe with five toes, to natural hiftory, anatomy, and phyfiology; particularly by the mixture of thefe three races with each other, and with the common race of fowls. Would this mixture vary the number of toes? Would the rump and the tail be completely, or only partly, formed? I have been informed that there have been fowls with only half a rump and tail.

There are to be feen in the Garden of Plants a fmall number of fheep of the Spanifh race, of a fuperior fleece, who have been from the moment of their birth continually expofed,

posed, without shelter, to the open air, and which were produced by a ram and the ewe, who have also been from generation to generation, during a space of twenty years, exposed to the air. There are also to be seen lambs produced in the open air, who prosper better than when housed, whatever may be the rigour of the season. This example may, perhaps, induce the proprietors of sheep to suppress the use of stables. The expence of maintaining these buildings, instead of being useful to the sheep, is in a great degree hurtful to them. When there happens to be no stables in a farm, and the proprietors are not disposed to construct one, they avoid (in France) putting any sheep on that farm, not being aware that they are better in the open air than in stables, which the small flock in the Garden of Plants evidently proves. This would be of benefit, not only to individuals, but to the republic in general, since sheep form one of the principal sources of wealth.

In the experiments which I have made in my sheep-farm, near Montbard, on sheep, to ascertain what herbs they might eat without injury, and those which would prove noxious, I put two sheep into a small fold. These animals are so accustomed to be together, that a sheep who finds himself alone, becomes restless, and employs himself in searching for others, rather than in eating. I gave to these two sheep enclosed in the fold only one species of plants in the rack for their subsistence for eight days. In the Garden of Plants, I am able to try a considerably greater number of plants than in the province of the Côte d'Or, where my sheep-farm is situated; but the greater part of these plants are not sufficiently abundant to try them on two sheep for many days. I have, therefore, been obliged to divide the little folds into two, and to put only one. The sheep in each being separated only by a hurdle, they imagine themselves together, and they eat without any uneasiness with regard to company.

It appears that Linnæus was the first who thought of making trials to ascertain what plants animals eat, and those which they abstain from. He has made these experiments himself, and recommended them to his disciples. They have been published in the work entitled *Pan suecicus*; where is also to be found the method which he has followed in making these trials. I have thought it my duty to avoid his plan, as I found it to be attended with many inconveniences.

Herbs were placed before these animals, and it was too quickly concluded that they will, for the future, be agreeable to them, even though they had been compelled, after a long abstinence, to eat them. I think that these trials ought to be extended to a great number of days, provided there be a sufficient quantity of herbs of the same species to supply them with. For I have seen sheep obstinately refuse oats the first time they were presented to them, and eat them the second, with the utmost avidity, when they had an appetite.

Linnæus does not prescribe that trials should be made with herbs for feeding sheep when they are going out fasting; but when they return from pasturage, and are nearly satisfied with food. I believe this proceeding would answer, if we knew what were the herbs which sheep love best; but that is not the object of my researches: I wish to know with what plants

plants sheep may be fed and supported, though they eat those plants only when they are in want of such as they are accustomed to.

The manner in which I made the trials of plants upon sheep, were distinguished as follows:

1. Those plants which they eat with a good appetite.
2. Those which they eat against their inclination to appease their hunger.
3. Those which they absolutely refused to eat.
4. Those which caused them to drink more than ordinary, which is a very bad symptom in sheep.
5. Those which produced much urine.
6. Those which occasioned the colic in the stomach.
7. Those which purged.
8. Those which caused an evacuation of blood by the urinary passage.
9. Those which proved mortal.

XIV.

On the Native Iron of Peru, and on Mercury contained in Sea-Salt. By Professor PROUST, *of Madrid.**

THE history of the discovery of native iron, by Don Rubin de Celis, is consigned in the Annales de Chimie. Accident procured me some small pieces, amounting to about half an ounce in all, which I was curious in employing to resolve the problem of native iron.

Before I exposed it to the agency of solvents, I examined its external characters. The most remarkable was, that it does not rust as easily as forged iron. My pieces, according to every appearance, were taken from the external surface of the great mass; but at all the places where the chissel has passed to cut them off, they preserve a whiteness and clearness which is astonishing, particularly when it is considered that they have crossed the sea, and have been kept several years in paper, which is the worst covering we know of for rusting iron.

These pieces are very ductile, forge wonderfully well, are very soft under the file, and cannot be hardened by ignition and cold water. When placed alongside of a piece of iron filed and softened to the same degree, they are whiter, and resemble steel annealed and filed clean.

I have dissolved one hundred grains in the aqueous sulphuric acid, and they produced me only 170 inches of hydrogen; whereas, at the same temperature and pressure of the atmosphere, soft iron constantly afforded 200 inches.

The solution was made with facility. Black particles are separated, but disappear towards the end of the solution; so that I could not decide that they were plumbago.

* Journal de Physique, VI.

In

In order to examine this solution, I began by hepatic water, with the intention of seeing whether any one of the metals would be separated which yield oxygen to sulphurated hydrogen, but no turbidness was produced.

It remained, therefore, to be proved, whether it contained any one of the four metals which are not precipitated by hepatic water; and beginning with manganese, I prepared my solution for the purpose of ascertaining its presence.

I first heated it with a little nitric acid, in order to carry the oxide of iron at the maximum of its oxidation. I then precipitated it gradually by the carbonate of potash, till the liquor had lost its yellow colour. It was then filtered, and the precipitation completed. A deposition of a light green colour was then made, which I soon perceived to be the carbonate of nickel. It was perfectly pure, and tinged borax of an hyacinth colour. This precipitate, redissolved in sulphuric acid, easily gave rhomboidal cubic crystals of considerable size. In this manner I obtained about 50 grains, which indicates that this native Peruvian iron contains a notable proportion of nickel.

If the carbonate of nickel of this alloy, or of any other ore whatever, be perfectly pure, as I have succeeded in obtaining it, loses the carbonic acid, becomes black, then parts with its oxygen, and at length remains on the coal well reduced to the metallic state. It is unfusible, at least with respect to my means, and very much resembles, in its colour and spongy appearance, platina obtained by the blow-pipe from the ammoniacal muriate of platina.

Nickel is perfectly attracted by the magnet, and that quite independent of iron, as I have proved by experiments on the nickel of Arragon, which I shall hereafter publish.

We may, I think, conclude, that iron alloyed with nickel in a certain proportion communicates a degree of whiteness, diminishes its disposition to rust, does not in any respect injure its ductility, if even it do not add to it; which deserves to be proved by direct experiments; and lastly, that it would be premature to determine whether this precious compound be the work of art or of nature.

On Mercury contained in Sea-Salt.

The strongest marine acid which is commonly found in the markets of France and Spain is usually made with common salt and oil of vitriol. This acid contains mercury in the state of corrosive sublimate: it arises from the mercury which is naturally mixed in sea-salt. This fact, which, at first, very much astonished the world, when Hilaire Rouelle announced it in the *Journal de Medicine*, is not mentioned in the Elements of Modern Chemistry, because no general inference could be drawn from a solitary fact. Yet mercury is also found in the salt which is consumed in Spain. I perceived it the first time by the spots of amalgam, which was left in certain silver basons in which I had purified a considerable quantity; and in this manner also it was that Rouelle was led to observe it.

The marine acid which is made by Charlar at Paris contains mercury. To this last

ſubſtance it is that I attribute the mercury I found in all the tin in the market, when I operated at that city. Being provided with marine acid from the ſame works, at the firſt erection of the laboratory of Segovia, I again found this amalgam at the end of the ſolutions of the tin of England, Mexico, and Monterey in Spain.

I remained a long time perplexed with the explanation of theſe facts, till chance, a ſhort time ago, diſcovered to me the ſource of this aſtoniſhing mercury.

There is in the hiſtory of copper a peculiar kind of oxidation, which appears to me to have been hitherto unobſerved. It takes place when plates of copper are kept in a bottle of marine acid full and cloſed. The copper, after having taken up about 17 per cent of oxygen, whereas it can receive 25 in other acids, this copper is transformed into a white muriate cryſtallized in tetrahedrons; which becomes violet-coloured by light, is inſoluble in water, ſoluble in ammoniac without colouring it, and has other curious properties, of which I ſhall ſpeak more fully hereafter. Being deſirous of repeating this experiment with the marine acid of Paris, I found, two days afterwards, that my plates of copper were rendered white. I took them out, and examined them, and diſcovered without difficulty that they were changed by mercury.

To detect the exiſtence of mercury in marine acid in a more direct way, I mixed it with hepatic water. It became immediately turbid, and afterwards depoſited a black ethiops. This alſo is the habitude of the ſolutions of ſublimate mercury with hepatic water.

If marine acid of the fabric of Cadahaſſo in La Mancha be mixed with the muriatic ſolution of tin which has been kept upon tin, the mixture ſoon becomes turbid, of a light grey colour, and will depoſit mercury on a piece of gold placed at the bottom of the veſſel. I have found as much as two grains in the pound of this acid.

However little may be the utility of quoting the ancient chemiſts, at this day, I ſhall nevertheleſs offer ſome paſſages which appear to ſhew that it was known that mercury is contained in marine ſalt.

Boyle found a ſmall quantity of mercury contained in a mixture of lead and marine acid, left for a time in his laboratory. "This was ſo much the leſs ſurpriſing to me," ſays he, "as the principal ingredient was marine acid." On the Production of Principles. De Product. Princip. p. 55.

Does not this paſſage prove that the exiſtence of mercury in common ſalt was not a new fact to Boyle?

Athanaſius Kircher affirms, p. 316 of one of his treatiſes, of which I forgot the title when I wrote this note, that mercury is obtained from ſea-ſalt. In his time, the ſyſtem of mercurification, and the mercuries of metals, were much in vogue.

Beccher, Phyſ. ſubt. p. 205, obtained mercury from a mixture of ſea-ſalt and clay. He affirms, page 456, that he was aſſured that mercury might be augmented by means of ſea-ſalt, and aſks, on this occaſion, how much ſea-ſalt may be contained in a pound of mercury? The reverſe of the queſtion would have been better founded.

Senac, in his Chemiſtry, ſpeaks alſo of mercury found in ſea-ſalt. And, laſtly, we find other traces

traces of mercury in ſea-ſalt, in the writings of Tachenius, Clavens, Beguin, in the treatiſe de Tribus Principiis of Glauber, &c.

If any traveller after reading this ſhould take the trouble to obſerve whether the ſheeting of a veſſel newly ſailing out of port ſhould become ſilvered on certain places, particularly when the ſheeting is new; or if he ſhould take the trouble to ſuſpend a plate of gold in the water, and obſerve its changes; he might perhaps flatter himſelf with affording at his return a new article to natural hiſtory, reſpecting the marine acid. Who can affirm whether the deſtruction of (copper) ſheeting, which is ſometimes ſo rapid, and ſtill unknown as to its cauſe, may not depend upon mercury in greater abundance in ſome ſeas than in others?

XV.

Note concerning the Earthquake in Peru, 1797. *By M. CAVANILLES.**

THERE are more than ſixteen volcanos, of which the internal parts are in a ſtate of inceſſant agitation, and from which denſe vapours, and often flames, are emitted, either by the crater or by lateral openings. In the moſt profound calms, noiſes and dreadful roarings are often heard, announcing the approach of earthquakes, to which this country is particularly ſubject. Since the year 1791, this noiſe was frequently heard in the neighbourhood of the mountain, called Tunguraqua. Antonia Pineda, and Nee, naturaliſts in the expedition round the world, examined the ſides of this volcano, the lava of which was hardened by the internal heat of the mountain, as well as by the rays of the ſun, and were much alarmed at the horrid noiſes and extreme heat of the place. Pineda, that excellent philoſopher, whoſe premature death is ſtill a ſubject of bitter regret to the ſcientific world, predicted that a terrible eruption was preparing in the mountain of Tunguraqua, and the event confirmed his ſuſpicions. On the fourth of February, 1797, at three quarters after ſeven in the morning, the ſummit of the mountain being uncommonly free from vapours, the bowels of the volcano were agitated by frequent ſhocks, and the adjacent chains, or ridges, were dilacerated or burſt in ſuch a manner, that in the ſpace of four minutes an immenſe country was overthrown by an undulatory motion. Hiſtory has never related the effects of an earthquake ſo extraordinary; nor did ever a phenomenon of this kind produce greater miſchiefs, or deſtroy a greater number of the human race! A number of towns and villages were deſtroyed in an inſtant. Some, among which were Riobamba, Quero, Pelileo, Patate, and Pillaro, were buried under the ruins of the neighbouring mountains: others, in the juriſdiction of Harnbata, Latacunga, Guaranda, Riobamba, and Alauſi, were totally deſtroyed. Others ſuffered prodigiouſly by the gulphs which were formed, and the reflux of rivers intercepted in

* J. de Phyſ. VI. 231.

their

their course by dams of earth. Others again, after the agitation of frequent ſhocks, remained in a ſtate ſo ruinous as to threaten their immediate deſtruction. Sixteen thouſand men are reckoned to have periſhed in the firſt and ſubſequent ſhocks. At ten in the morning, and at four in the evening of the ſame day (Feb. 4), after a dreadful noiſe the earth ſhook with additional force, and continued to tremble, though with leſs effect, during the whole of the months of February and March. On the fifth of April, at three quarters paſt two in the morning, the villages already in part deſtroyed were again ſo ſtrongly agitated as to have completed this overthrow. Theſe ſhocks were felt through country extending 140 leagues from eaſt to weſt; that is to ſay, from the ſea coaſt to the River of Napo, and doubtleſs ſtill farther; for we are little acquainted with thoſe parts which are inhabited by the natives, and from N.E. to S.W. from Popajan to Piura, which is reckoned 170 leagues. (The volcano of Tunguragua had before occaſioned an earthquake in 1557.) But in the centre of this diſtrict lies the part which is totally deſtroyed, to the extent of 40 leagues north and ſouth from Guarandam to Machache, and 20 leagues from eaſt to weſt.

But as if this earthquake had not been ſufficient to ruin a country of ſuch fertility, riches, and population, there was another misfortune prepared for the ſufferers which had never before been known. The earth opened and formed immenſe gulphs; the ſummits of the mountains rolled down into the vallies, and from their burſten flanks there iſſued a quantity of fetid water, ſo immenſe that in a ſhort time it filled vallies one thouſand feet wide, and ſix hundred feet deep. It covered the villages, the edifices, and the inhabitants; cloſing the mouths of the ſprings of fine waters, and condenſing by drying in a few days into a very hard earthy paſte, which intercepted the courſe of rivers, cauſing them to return or deviate for 87 days, and converted what was formerly dry land into lakes.

During theſe earthquakes many of the moſt extraordinary events happened, which will no doubt be recorded in the page of hiſtory. I ſhall here relate only two. At the very inſtant of the earthquake, the lake Quirrotoa, near the village Inſiloc, in the juriſdiction of Lacatunga, took fire, and its vapours ſuffocated the herds which fed in the neighbouring lands. Near the town of Pallileo ſtood a large mountain, named Moya, which being overthrown in an inſtant, vomited a river of the thick and fetid matter, which covered and completed the deſtruction of that wretched town. The naturaliſt will hereafter find in theſe deſolated tracts objects worthy of his reſearches. Specimens of the minerals and earths of Tunguragua have been ſent to Spain; but theſe will not ſuffice to indicate the cauſes of theſe aſtoniſhing phenomena. For this purpoſe it will be neceſſary to viſit the ground itſelf, where this conflict of the elements took place, and has left the ruinous marks of its energy.

ACCOUNTS

ACCOUNTS OF BOOKS.

ELEMENS d'une Typographie, &c. Elements of a Typography, which reduces that at present in use to one third; and a writing which gains near three fourths upon the common (French) writing: both applicable to all languages, preserving all the grammatical principles, and the riches of those which are printed in the Roman character, and written by the letters used in France; established on simple principles easy to be comprehended; demonstrated by clear and precise rules, of which the perfect theory may be learned in less than a day, and of which the exercise will enable a skilful hand to follow the speech of an orator. Paris, printed for the Author, A. Pront, No. 249, Rue de la Harpe; and sold by F s and Desenne. One volume octavo, about 200 pages, with 47 plates of examples, price 18 franks. Copies on vellum paper, 36 franks: and there are a few copies in large vellum paper, quarto, price 73 franks, in which the examples, words, and detached signs, are made with the pen.

The learned A. M. Millin, author of the Magasin Encyclopédique *, gives a long account of this work, and speaks highly in its favour, but, as he justly remarks, a work of this kind may be read and studied, but cannot be abridged. This is so far the case in his account, as well as the extracts he gives from the report made to the National Institute of France, that I must confess that though my curiosity is excited by the importance of the subject, and the general character of this performance, yet I am far from being satisfied with the obscure notion, conveyed, of the means by which the desirable purposes mentioned in the title are said to have been accomplished. The Reporter to the Institute, whose name is not mentioned, informs us, that "the consonants and the vowel are so identified in the processes of this invention, that two, three, four, and even five characters form only one, without any confusion in their values. Whence it constantly happens that a single figure, which demands no more time for its description than a single letter of common writing, and occupies no more space, but frequently less, is employed to represents two characters, often three, and sometimes four and five:" to which Cit. Millin adds, that this condensation proceeds even as far as eight.

Citizen Pront makes use of 118 characters: 25 to represent the capitals of the alphabet; 25 for the smaller letters; 32 for the compound vowels, &c.*

The written characters do not essentially differ from those used for printing. The chief difference consists in the ligatures, or methods of joining in the former.

As the condensation is stated to be as three to one, the fatigue of the eye must be only one third part as much as with the ordinary characters.

But the most remarkable peculiarities of the new theory of this author is (says C. Millin);

* Vol. III. 200.

† This &c. is in C. Millin's account, otherwise I should have mentioned the disposal of the rest.

Millin), that though this method is extremely concife, eafy to be acquired, and rapid in the execution, he has fucceeded in retaining every fyllable, vowel, point, mark, and even accent.

The gain of two-thirds of the time employed in writing is certainly very extraordinary. Profeffional men in the law have more occafion to employ copyifts than thofe of any other bufinefs. Their work is reckoned by the fheet confifting of feventy-two words. Common copyifts can write eighteen or nineteen fheets in an hour, with the words at full length; and it is reckoned fpeedy writing to complete twenty-four in the fame time. The moft fpeedy writer I ever employed as an amanuenfis could write thirty fheets from dictation in the hour: but I never employed him fo regularly as to know whether he could continue for days at that rate. I myfelf write at the rate of twenty-feven, and I have had feveral affiftants who have written at about the fame fpeed. Out of three fhort-hand writers I have employed, the fwifteft did not write more than four or five and forty fheets in the hour, and that not very correct; which may be reckoned barely twice as fwift as the common writers. I fuppofe the profeffed fhort-hand writers in our courts do much more; but they undoubtedly have many fingle characters, and abridgments of whole fentences, and common forms which muft add to their difpatch. Deliberate reading fo as to pronounce every fyllable with extreme precifion, or clear oratorial matter, is meafured by about 120 fheets in an hour:—diftinct colloquial reading by about 160 fheets. From the performance of the fhort-hand writers compared to the extent to which they abridge their words, I have no doubt but that we form our common letters in lefs time than they do their fimple characters, and that this difference arifes from our greater practice. Hence it fhould feem as if, for common purpofes at leaft, it would be better to abridge in the ufual characters than in thofe of fhort-hand.

I have fent for this work, which may probably deferve further notice when I receive it.

Hiftoire des Mathematiques, &c. Hiftory of the Mathematics, &c. a new Edition, confiderably augmented, and continued to the prefent Time. By J. F. Montucla, of the National Inftitute of France, 2 vols. 4to. 1465 pages, type Cicero.

Cit. Lalande has given a fhort account of this well-known and efteemed work in the Mag. Encyclop. III. 257. The firft edition appeared in 1758, and the author has employed himfelf, during the long feries of years elapfed fince that time, in increafing and perfecting it: but his employment in the public buildings did not leave him time enough to attend to the fecond edition. In 1792, the author being difgufted with the circumftances of the times, was tempted to renounce it: but Lalande, and the bookfeller Panckoucke, urged him to proceed; and in 1794, the work went to prefs. Thefe two firft volumes bring the fubject, as in the firft edition, to the end of the laft century; but the two others will carry it to the end of the prefent. Half the third volume is already printed, and there is reafon to hope that both will appear, notwithftanding the great age of the author, who was born on the 5th September, 1725. This edition is confiderably augmented; the number of letters in the firft edition, being to prove in the prefent as 232 to 336.

Mr. Simon Goodrich's Application of a Crank to answer the purposes of an Escapement in Clocks.

Fig. 1.

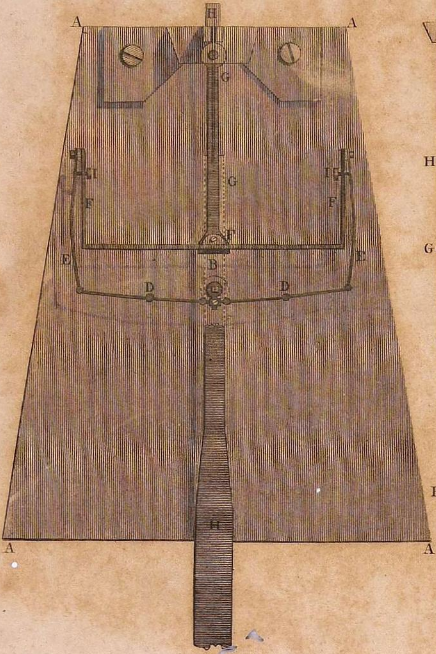


Fig. 2.

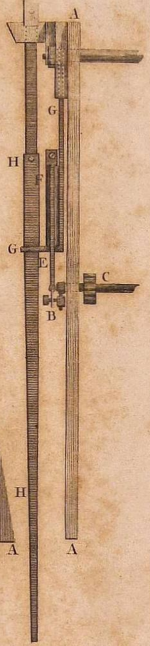


Fig. 3.

Barlow sculp.

Steel made with the Diamond.

Fig. 1.

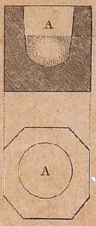
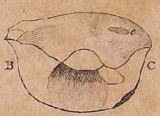


Fig. 2.

Fig. 3.



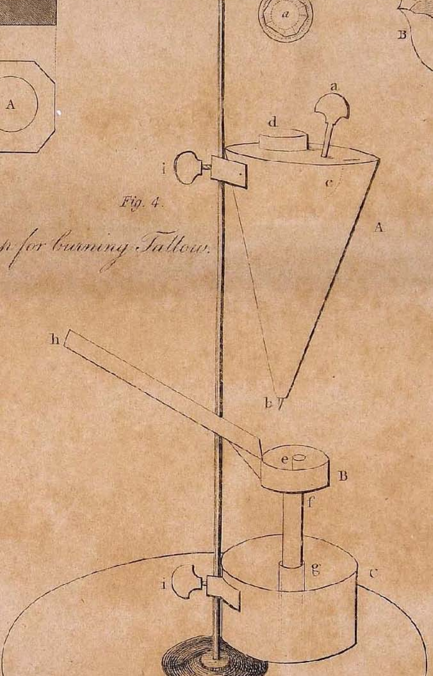


Fig. 4.

Lamp for burning Tallow.

Barlow sculp.

A

JOURNAL

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AND

THE ARTS.

DECEMBER 1799.

ARTICLE I.

An Inquiry concerning the Weight ascribed to Heat. By BENJAMIN COUNT OF RUMFORD, F. R. S. M. R. I. A. &c.*

THE various experiments which have hitherto been made with a view to determine the queſtion ſo long agitated, relative to the weight which has been ſuppoſed to be gained, or to be loſt, by bodies upon their being heated, are of a nature ſo very delicate, and are liable to ſo many errors, not only on account of the imperfections of the inſtruments made uſe of, but alſo, of thoſe, much more difficult to appreciate, ariſing from the vertical currents in the atmoſphere, cauſed by the hot or the cold body which is placed in the balance, that it is not at all ſurpriſing that opinions have been ſo much divided, relative to a fact ſo very difficult to aſcertain.

It is a conſiderable time ſince I firſt began to meditate on this ſubject, and I have made many experiments with a view to its inveſtigation; and in theſe experiments, I have taken all thoſe precautions to avoid errors, which a knowledge of the various ſources of them, and an earneſt deſire to determine a fact which I conceived to be of importance to be known, could inſpire; but, though all my reſearches tended to con-

* Philoſ. Tranſ. 1799. page 179.

vince me more and more, that *a body acquires no additional weight upon being heated* or rather, that heat has no effect whatever upon the weights of bodies, I have been so sensible of the delicacy of the inquiry, that I was for a long time afraid to form a decided opinion upon the subject.

Being much struck with the experiments recorded in the Transactions of the Royal Society, Vol. LXXV. made by Dr. *Fordyce*, upon the weight said to be acquired by water upon being frozen; and being possessed of an excellent balance, belonging to *His most Serene Highness the Elector Palatine Duke of Bavaria*; early in the beginning of the winter of the year 1787,—as soon as the cold was sufficiently intense for my purpose,—I set about to repeat those experiments, in order to convince myself whether the very extraordinary fact related, might be depended on; and, with a view to removing, as far as was in my power, every source of error and deception, I proceeded in the following manner.

Having provided a number of glass bottles, of the form and size of what in England is called a Florence flask,—blown as thin as possible,—and of the same shape and dimensions, I chose out from amongst them two, which, after using every method I could imagine of comparing them together, appeared to be so much alike as hardly to be distinguished.

Into one of these bottles, which I shall call A, I put 4107,86 grains Troy of pure distilled water, which filled it about half full; and into the other, B, I put an equal weight of weak spirit of wine; and, sealing both the bottles hermetically, and washing them, and wiping them perfectly clean and dry on the outside, I suspended them to the arms of the balance, and placed the balance in a large room, which for some weeks had been regularly heated every day by a German stove, and in which the air was kept up to the temperature of 61° of *Fahrenheit's* thermometer, with very little variation. Having suffered the bottles, with their contents, to remain in this situation till I conceived they must have acquired the temperature of the circumambient air, I wiped them afresh, with a very clean dry cambric handkerchief, and brought them into the most exact equilibrium possible, by attaching a small piece of very fine silver wire to the arm of the balance to which the bottle which was the lightest was suspended.

Having suffered the apparatus to remain in this situation about twelve hours longer, and finding no alteration in the relative weights of the bottles,—they continuing all this time to be in the most perfect equilibrium,—I now removed them into a large uninhabited room, fronting the north, in which the air, which was very quiet, was at the temperature of 29°, F; the air without doors being at the same time at 27°; and, going out of the room, and locking the door after me, I suffered the bottles to remain forty-eight hours, undisturbed, in this cold situation, attached to the arms of the balance as before.

At the expiration of that time, I entered the room,—using the utmoſt caution not to diſturb the balance,—when, to my great ſurpriſe, I found that the bottle A very ſenſibly preponderated.

The water which this bottle contained was completely frozen into one ſolid body of ice; but the ſpirit of wine, in the bottle B, ſhowed no ſigns of freezing.

I now very cautiouſly reſtored the equilibrium, by adding ſmall pieces of the very fine wire of which gold lace is made, to the arm of the balance to which the bottle B was ſuſpended, when I found that the bottle A had augmented its weight by $\frac{1}{35904}$ part of its whole weight at the beginning of the experiment; the weight of the bottle with its contents having been 4811,23 grains Troy, (the bottle weighing 703,37 grains, and the water 4107,86 grains,) and it requiring now $\frac{134}{1000}$ parts of a grain, added to the oppoſite arms of the balance, to counterbalance it.

Having had occaſion juſt at this time to write to my friend, Sir *Charles Blagden*, upon another ſubject, I added a poſtſcript to my letter, giving him a ſhort account of this experiment, and telling him how "*very contrary to my expectation*" the reſult of it had turned out; but I ſoon after found that I had been too haſty in my communication. Sir *Charles*, in his anſwer to my letter, expreſſed doubts reſpecting the fact; but, before his letter had reached me, I had learned from my own experience, how very dangerous it is, in philoſophical inveſtigations, to draw concluſions from ſingle experiments.

Having removed the balance, with the two bottles attached to it, from the cold into the warm room, (which ſtill remained at the temperature of 61°), the ice in the bottle A gradually thawed; and, being at length totally reduced to water, and this water having acquired the temperature of the ſurrounding air, the two bottles, after being wiped perfectly clean and dry, were found to weigh as at the beginning of the experiment, before the water was frozen.

This experiment being repeated, gave nearly the ſame reſult, the water appearing, when frozen, to be heavier than in its fluid ſtate; but, ſome irregularity in the manner in which the water loſt the additional weight which it had appeared to acquire upon being frozen, when it was afterwards thawed, as alſo a ſenſible difference in the quantities of weight apparently acquired in the different experiments, led me to ſuſpect, that the experiment could not be depended on for deciding the fact in queſtion; I therefore ſet about to repeat it, with ſome variations and improvements; but, before I give an account of my further inveſtigations relative to this ſubject, it may not be amiſs to mention the method I purſued for diſcovering whether the appearances mentioned in the foregoing experiments might not ariſe from the imperfections of my balance; and it may likewiſe be proper to give an account, in this place, of an intermediate experiment

 which

which I made, with a view to difcover, by a fhorter route, and in a manner lefs exceptionable than that above mentioned, whether bodies actually lofe, or acquire, any weight, upon acquiring an additional quantity of latent heat.

My fufpicions refpecting the accuracy of the balance arofe from a knowledge,—which I acquired from the maker of it,—of the manner in which it was conftructed.

The three principal points of the balance having been determined, as nearly as poffible, by meafurement, the axes of motion were firmly fixed in their places, in a right line, and the beam being afterwards finifhed, and its two arms brought to be in equilibrio, the balance was proved by fufpending weights, which before were known to be exactly equal, to the ends of its arms.

If with thefe weights the balance remained in equilibrio, it was confidered as a proof that the beam was juft; but, if one arm was found to preponderate, the other was gradually lengthened, by beating it upon an anvil, until the difference of the lengths of the arms was reduced to nothing, or until equal weights, fufpended to the two arms, remained in equilibrio; care being taken, before each trial, to bring the two ends of the beam to be in equilibrio, by reducing, with the file, the arm which had been lengthened.

Though, in this method of conftructing balances, the moft perfect equality in the lengths of the arms may be obtained, and confequently the greateft poffible accuracy, when ufed at a time when the temperature of the air is the fame as when the balance was made, yet, as it may happen, that in order to bring the arms of the balance to be of the fame length, one of them may be much more hammered than the other, I fufpected it might be poffible that the texture of the metal forming the two arms might be rendered fo far different, by this operation, as to occafion a difference in their expanfions with heat; and that this difference might occafion a fenfible error in the balance, when, being charged with a great weight, it fhould be expofed to a confiderable change of temperature.

To determine whether the apparent augmentation of weight, in the experiments above related, arofe in any degree from this caufe, I had only to repeat the experiment, caufing the two bottles A and B to change places upon the arms of the balance; but, as I had already found a fenfible difference in the refults of different repetitions of the fame experiment, made as nearly as poffible under the fame circumftances, and as it was above all things of importance to afcertain the accuracy of my balance, I preferred making a particular experiment for that purpofe.

My firft idea was, to fufpend to the arms of the balance, by very fine wires, two equal globes of glafs, filled with mercury, and, fuffering them to remain in my room till they fhould have acquired the known temperature of the air in it, to have removed

them

them afterward into the cold, and to have seen if they still remained in equilibrio, under such difference of temperature; but, considering the obstinacy with which moisture adheres to the surface of glass, and being afraid that, somehow or other, notwithstanding all my precautions, one of the globes might acquire or retain more of it than the other, and that by that means its apparent weight might be increased; and having found by a former experiment, of which I have already had the honour of communicating an account to the Royal Society, that the gilt surfaces of metals do not attract moisture; instead of the glass globes filled with mercury, I made use of two equal solid globes of brass, well gilt and burnished, which I suspended to the arms of the balance, by fine gold wires.

These globes, which weighed 4975 grains each, being wiped perfectly clean, and having acquired the temperature of (61°) of my room, in which they were exposed more than twenty-four hours, were brought into the most scrupulous equilibrium, and were then removed, attached to the arms of the balance, into a room in which the air was at the temperature of 26°, where they were left all night.

The result of this trial furnished the most satisfactory proof of the accuracy of the balance; for, upon entering the room, I found the equilibrium as perfect as at the beginning of the experiment.

Having thus removed my doubts respecting the accuracy of my balance, I now resumed my investigations relative to the augmentation of weight which fluids have been said to acquire upon being congealed.

In the experiments which I had made, I had, as I then imagined, guarded as much as possible against every source of error and deception. The bottles being of the same size, neither any occasional alteration in the pressure of the atmosphere during the experiment, nor the necessary and unavoidable difference in the densities of the air in the hot and in the cold rooms in which they were weighed, could affect their apparent weights; and their shapes and their quantities of surface being the same, and as they remained for such a considerable length of time in the heat and cold to which they were exposed, I flattered myself that the quantities of moisture remaining attached to their surfaces, could not be different as sensibly to effect the results of the experiments. —But, in regard to this last circumstance, I afterwards found reason to conclude that my opinion was erroneous.

Admitting the fact stated by Dr. *Fordyce*,—(and which my experiments had hitherto rather tended to corroborate than to contradict,)—I could not conceive any other cause for the augmentation of the apparent weight of water, upon its being frozen, than the loss of so great a proportion of its latent heat as that fluid is known to evolve when it congeals; and I concluded, that if the loss of latent heat added to the weight of

of one body, it muſt of neceſſity produce the ſame effect on another, and conſequently that the augmentation of the quantity of latent heat muſt,—in all bodies,—and in all caſes,—diminiſh their apparent weights.

To determine whether this is actually the caſe or not, I made the following experiment.

Having provided two bottles, as nearly alike as poſſible, and in all reſpects ſimilar to thoſe made uſe of in the experiments above mentioned, into one of them I put 4012,46 grains of water, and into the other an equal weight of mercury; and, ſealing them hermetically, and ſuſpending them to the arms of the balance, I ſuffered them to acquire the temperature of my room, 61°; then, bringing them into a perfect equilibrium with each other, I removed them into a room in which the air was at a temperature of 34°, where they remained twenty-four hours.—But there was not the leaſt appearance of either of them acquring, or loſing any weight.

Here it is very certain, that the quantity of heat loſt by the water, muſt have been very conſiderably greater than that loſt by the mercury; the ſpecific quantities of latent heat in water and in mercury, having been determined to be to each other as 1000, to 33; but this difference in the quantities of heat loſt, produced no ſenſible difference on the weights of the fluids in queſtion.

Had any difference of weight really exiſted, had it been no more than *one millionth* part of the weight of either of the fluids, I ſhould certainly have diſcovered it; and, had it amounted to ſo much as $\frac{1}{700000}$ part of that weight, I ſhould have been able to have meaſured it; ſo ſenſible, and ſo very accurate, is the balance which I uſed in theſe experiments.

I was now much confirmed in my ſuſpicions, that the apparent augmentation of the weight of the water upon its being frozen, in the experiments before related, aroſe from ſome accidental cauſe; but I was not able to conceive what that cauſe could poſſibly be, unleſs it were, either a greater quantity of moiſture attached to the external ſurface of the bottle which contained the water, than to the ſurface of that containing the ſpirits of wine,—or ſome vertical current or currents of air, cauſed by the bottles or one of them not being exactly of the temperature of the ſurrounding atmoſphere.

Though I had foreſeen, and, as I thought, guarded ſufficiently againſt, theſe accidents,—by making uſe of bottles of the ſame ſize and form,—and which were blown of the ſame kind of glaſs,—and at the ſame time,—and by ſuffering the bottles, in the experiments, to remain for ſo conſiderable a length of time expoſed to the different degrees of heat and of cold, which alternately they were made to acquire; yet, as I did not know the relative conducting powers of ice and of ſpirit of wine with reſpect to heat; or, in other words, the degrees of facility or difficulty with which they acquire the

temperature

temperature of the medium in which they are exposed;—or the time taken up in that operation; and, consequently, was not *absolutely certain* as to the equality of the temperatures of the contents of the bottles at the time when their weights were compared, I determined now to repeat the experiments, with such variations as should put the matter in question out of all doubt.

I was the more anxious to assure myself of the real temperatures of the bottles and of their contents, as any difference in their temperatures might vitiate the experiment, not only by causing unequal currents in the air, but also, by causing, at the same time, a greater or less quantity of moisture to remain attached to the glass.

To remedy these evils, and also to render the experiment more striking and satisfactory in other respects, I proceeded in the following manner.

Having provided three bottles, A, B, and C, as nearly alike as possible, and resembling in all respects those already described; into the first, A, I put 4214,28 grains of water, and a small thermometer, made on purpose for the experiment, and suspended in the bottle in such a manner that its bulb remained in the middle of the mass of water; into the second bottle, B, I put a like weight of spirit of wine, with a like thermometer; and, into the bottle, C, I put an equal weight of mercury.

These bottles, being all hermetically sealed, were placed in a large room, in a corner far removed from the doors and windows, and where the air appeared to be perfectly quiet; and, being suffered to remain in this situation more than twenty-four hours, the heat of the room (61°) being kept up all that time with as little variation as possible, and the contents of the bottles A and B appearing, by their inclosed thermometers, to be exactly at the same temperature, the bottles were all wiped with a very clean, dry cambric handkerchief; and, being afterwards suffered to remain exposed to the free air of the room a couple of hours longer, in order that any inequalities in the quantities of heat,—or of the moisture attached to their surfaces,—which might have been occasioned by the wiping, might be corrected by the operation of the atmosphere by which they were surrounded, they were all weighed, and were brought into the most exact equilibrium with each other, by means of small pieces of very fine silver wire, attached to necks of those of the bottles which were the lightest,

This being done, the bottles were all removed into a room in which the air was at 30°; where they were suffered to remain, perfectly at rest and undisturbed, forty-eight hours; the bottles A and B being suspended to the arms of the balance, and the bottle C suspended, at an equal height, to the arm of a stand constructed for that purpose, and placed as near the balance as possible, and a very sensible thermometer suspended by the side of it.

At the end of forty-eight hours,—during which time the apparatus was left in this situation,—I entered the room, opening the door very gently, for fear of disturbing the balance;

lance; when I had the pleafure to find the three thermometers,---*viz.* that in the bottle A, (which was now inclofed in a folid cake of ice,) that in the bottle B, and that fufpended in the open air of the room, all ftanding at the fame point, 29° F, and the bottles A and B *remaining in the moft perfect equilibrium.*

To affure myfelf that the play of the balance was free, I now approached it very gently, and caufed it to vibrate; and I had the fatisfaction to find, not only that it moved with the utmoft freedom, but alfo, when its vibration ceafed, that it refted precifely at the point from which it had fet out.

I now removed the bottle B from the balance, and put the bottle C in its place; and I found that *that* likewife remained of the fame apparent weight as at the beginning of the experiment, being in the fame perfect equilibrium with the bottle A as at firft.

I afterwards removed the whole apparatus into a warm room, and, caufing the ice in the bottle A to thaw, and fuffering the three bottles to remain till they and their contents had acquired the exact temperature of the furrounding air, I wiped them very clean, and, comparing them together, I found their weights remained unaltered.

This experiment I afterwards repeated feveral times, and always with precifely the fame refult; the water, *in no inftance*, appearing to gain, or to lofe, the leaft weight, upon being frozen, or upon being thawed; neither were the relative weights of the fluids in either of the other bottles in the leaft changed, by the various degrees of heat, and of cold, to which they were expofed.

If the bottles were weighed at a time when their contents were not *precifely of the fame temperature*, they would frequently appear to have gained, or to have loft, fomething of their weights: but this doubtlefs arofe from the vertical currents which they caufed in the atmofphere, upon being heated or cooled in it: or to unequal quantities of moifture attached to the furfaces of the bottles;—or to both thefe caufes operating together.

As I knew that the conducting power of mercury, with refpect to heat, was confiderably greater than either that of water, or that of fpirit of wine, while its capacity for receiving heat is much lefs than that of either of them, I did not think it neceffary to inclofe a thermometer in the bottle C, which contained the mercury; for it was evident, that when the contents of the other two bottles fhould appear, by their thermometers, to have arrived at the temperature of the medium in which they were expofed, the contents of the bottle C could not fail to have acquired it alfo, and even to have arrived at it before them; for, the time taken up in the heating or in the cooling of any body, is, *cæteris paribus*, as the capacity of the body to receive and retain heat, *directly*, and as its conducting power, *inverfely*.

The bottles were fufpended to the balance by filver wires, about two inches long, with hooks at the ends of them; and, in removing and changing the bottles, I took care not

to

to touch the glass. I likewise avoided, upon all occasions, and particularly in the cold room, coming near the balance with my breath, or touching it, or any part of the apparatus, with my naked hands.

Having determined that water does not acquire or lose any weight, upon being changed from a state of *fludity* to that of *ice*, and *vice versâ*, I shall now take my final leave of a subject which has long occupied me, and which has cost me much pains and trouble; being fully convinced, (from the results of the above-mentioned experiments,) that if heat be in fact a *substance*, or matter,---a fluid *sui generis*, as has been supposed, which, passing from one body to another, and being accumulated, is the immediate cause of the phænomena we observe in heated bodies, (of which, however, I cannot help entertaining doubts,) it must be something so infinitely rare, even in its most condensed state, as to baffle all our attempts to discover its gravity. And, if the opinion which has been adopted by many of our ablest philosophers, that heat is nothing more than an intestine vibratory motion of the constituent parts of heated bodies, should be well founded, it is clear that the weights of bodies can in no wise be affected by such motion.

It is, no doubt, upon the supposition that heat is a substance distinct from the heated body, and which is accumulated in it, that all the experiments which have been undertaken, with a view to determine the weight which bodies have been supposed to gain, or lose, upon being heated or cooled, have been made; and, upon this supposition (but without, however, adopting it entirely, as I do not conceive it to be sufficiently proved,) all my researches have been directed.

The experiments with *water*, and with *ice*, were made in a manner which I take to be perfectly unexceptionable; in which no foreign cause whatever could affect the results of them; and the quantity of heat which water is known to part with, upon being frozen, is so considerable, that if this loss has no effect upon its apparent weight, it may be presumed that we shall never be able to contrive an experiment by which we can render the weight of heat sensible.

Water, upon being frozen, has been found to lose a quantity of heat amounting to 140 degrees of *Fahrenheit's* thermometer; or, which is the same thing, the heat which a given quantity of water, previously cooled to the temperature of freezing, actually loses, upon being changed to ice, if it were to be imbibed and retained by an equal quantity of water, at the given temperature, (that of freezing,) would heat it 140 degrees, or would raise it to the temperature of $(32° + 140)$ $172°$ of *Fahrenheit's* thermometer, which is only $40°$ short of that of boiling water; consequently, any given quantity of water, at the temperature of freezing, upon being actually frozen, loses almost as much heat as, added to it, would be sufficient to make it boil.

It is clear, therefore, that the difference in the quantities of heat contained by the

 water

water in its fluid state, and heated to the temperature of 61° F, and by the ice, in the experiments before mentioned, was, *at least*, nearly equal to that between water in a state of boiling, and the same at the temperature of freezing.

But this quantity of heat will appear much more considerable, when we consider the great capacity of water to contain heat, and the great apparent effect which the heat that water loses upon being frozen would produce, were it to be imbibed by, or communicated to, any body whose power of receiving and retaining heat is much less.

The capacity of water to receive and retain heat,—or what has been called its specific quantity of latent heat,—has been found to be to that of gold as 1000 to 50,—or as 20 to 1; consequently, the heat which any given quantity of water loses upon being frozen, were it to be communicated to an equal weight of gold, at the temperature of freezing, the gold, instead of being heated 172 degrees, would be heated $140 \times 20 = 2800$ degrees, or, would be raised to a *bright red heat*.

It appears therefore to be clearly proved, by my experiments, that a quantity of heat equal to that which 4214 grains (or about $9\frac{3}{4}$ oz.) of gold would require to heat it from the temperature of freezing water to be *red hot*, has no sensible effect upon a balance capable of indicating so small a variation of weight as that of $\frac{1}{1000000}$ part of the body in question; and, if the weight of gold is neither augmented nor lessened by *one millionth part*, upon being heated from the point of *freezing water* to that of a *bright red heat*, I think we may very safely conclude, that ALL ATTEMPTS TO DISCOVER ANY EFFECT OF HEAT UPON THE APPARENT WEIGHTS OF BODIES, WILL BE FRUITLESS.

II.

On the Plants used by the ancient People of Europe, to poison their Arrows. By C. CH. COQUEBERT.*

ALL nations that subsist by hunting have sought for quick poisons among vegetables, in which they could dip their arrows, in order to kill with more certainty the animals which constituted their food.

Historians in general have neglected to acquaint us with the plants which our ancestors, the half-savage inhabitants of Europe, used for that purpose, in remote ages. I have met accidentally with passages in two Spanish works, which throw much light on this interesting subject.

The first of these works is entitled *Sinopsis stirpium indigenarum Arragoniæ,* printed 1779, and whose author is only designed by the initial letters C. A. R. native of Sarragosa.

* Soc. Philomath. Feb. 1798. No. II.

goſa. This author quotes a manuſcript of *Cienfuegos*, his countryman, who in 1618 wrote on the botany of Arragon, and in which he relates that in his time the Spaniſh huntſmen ſtill uſed to poiſon their arrows; that the poiſon in which they dipped them was ſo ſpeedy in its effect, that the leaſt wound inflicted on the animal was ſufficient to ſecure the huntſman his game. The vegetable from which they prepared it was the *Veratrum album*, (white hellebore) a plant very common on the fields of the Alpine mountains. There was, however, ſome ſkill required in preparing the confection of the veratrum for that uſe; for *Cienfuegos* adds, that the King of Spain had at that time a huntſman wonderfully ſkilled in that art.

The ſecond work, from which I have received ſome information, is the hiſtory of the war of Grenada, under *Philip* the ſecond, by *Mendoza*. This author, whom the Spaniards highly eſteem for the purity of his diction, the impartiality which diſtinguiſhes his writings, and the extent of his knowledge, ſays that the poiſon uſed by the huntſmen, even in his time (at the beginning of the ſeventeenth century), was prepared on the mountains of Bejar and Guadarrama, with the black hellebore, called in that part of Spain *el zumo de vedegambre*. The extract made from it was of a brown red. Another poiſonous native plant was uſed for the ſame purpoſe on the lofty mountains of the kingdom of Grenada, which is called Yerva by the inhabitants, a word which denotes an excellent or eminently uſeful plant. It is the aconitum lycoctonum, which grows like the veratrum, on the high mountains.

The animals wounded by theſe poiſoned arrows are affected in the ſame manner as thoſe which have been wounded by the hellebore, or aconite. The ſymptoms are a ſudden extreme weakneſs, ſhivering cold, numbneſs and blindneſs, with foam at the mouth, and convulſions of the diaphragm. *Mendoza* affirms, that two plants, which he denotes only by the Spaniſh names of Membrillo and Retama, of whoſe ſignification I am ignorant, are ſucceſsfully uſed as counter-poiſons.

After having read thoſe two paſſages, I was deſirous to ſee what *Haller* ſays of the plants in his Hiſtoria ſtirpium indigenarum Helvetiæ; or rather, in the French tranſlation, he has given of that part of Vicat which relates to the properties of plants.

If, ſays he, the poiſon of the veratrum accidentally penetrates to the blood-veſſels, without loſs of its ſtrength, death is the immediate conſequence, even though by a very ſlight wound. This was obſerved at the time when the ancient Portugueſe uſed to poiſon their arrows with the juice of that plant. *Mathiolus* has confirmed this obſervation by his experiments. When death is thus produced, the progreſs of putrefaction is ſo rapid that the fleſh of the animal becomes ſoft as ſoon as it has ceaſed to breathe. *Guiluadinus* alſo mentions the poiſon the Spaniards prepared with this plant.

Two drams of the root of the veratrum, in decoction, injected into the veins of an animal, have excited immediate convulſions and vomiting, which were followed by death, and ſoon afterwards by a ſtate of flaccidity.

The ſpirituous infuſion, according to *Haller*, has more effect than the aqueous infuſion, and the latter more the decoction and extract. It is to be ſuppoſed then that the virtue of the plant reſides in the volatile parts which boiling evaporates.

At the article of the black hellebore (helleborus viridis, L.) *Haller* alſo affirms, that that plant is uſed to poiſon arrows. He quotes *Monardus*, who mentions of a hen which died, after a fibre of black hellebore had been paſſed through her comb. It is difficult, however, to aſcribe ſo deleterious an action to that hellebore; for ever ſince the time of *Columella* the root has been uſed to make ſetons for ſheep, which they uſually paſs through the ſkin of the neck. It produces ſuppuration.

With regard to the aconites, I find in *Haller*'s works, relative to that kind which *Linné* calls aconitum cammarum, that its juice having caſually, and in a very ſmall quantity, entered a wound, the accident was followed by cardialgia, fainting, ſwelling, and at length a gangrene.

It appears from theſe facts, that the three plants I have mentioned, particularly the veratrum, were uſed by the ancient inhabitants of Europe to poiſon their arrows, and that the uſe of fire-arms alone has aboliſhed that of the poiſon, uſed by the Spaniards as late as the laſt century.

III.

Experiments to determine the quantity of tanning principle and Gallic acid, contained in the Barks of various Trees. By GEORGE BIGGIN, *Eſq.**

THE bark of trees contains the aſtringent principle called gallic acid, and alſo that principle which has a peculiar affinity to the matter of ſkin, and which, from the uſe to which it is applied, is called the tanning principle. But in the preſent mode of tanning, bark is applied in *maſs* to the ſkins; conſequently *both* principles are applied. It remains for examination whether both principles are uſeful in the proceſs of tanning; for if they are not both uſeful, probably *one* is detrimental.

To a Nobleman whoſe zeal on every occaſion by which the ſciences or arts may receive illuſtration or improvement, is eminently conſpicuous, and to whoſe public energy, as well as private friendſhip, I feel myſelf much indebted, to his Grace the Duke of *Bedford* I owe the means of proſecuting ſome experiments on this ſubject. His Grace, by collecting a variety of barks at Woburn, gave me an opportunity of making

* Philoſ. Tranſ. 1799. p. 299.

ſome experiments to aſcertain the quantity of tanning principle and gallic acid, each bark contained. For that purpoſe I made uſe of the following methods, according to the principles laid down by Mr. *Seguin*.

By diſſolving an ounce of common glue in two pounds of boiling water, I procured a mucilaginous liquor, which as it contains the matter of ſkin in ſolution, is a teſt for the tanning principle. By a ſaturated ſolution of ſulphate of iron, I obtained a teſt for the Gallic acid.

I then took one pound of the bark I meant to try, ground as for the uſe of tanners, and divided it into five parts, each part being put into an earthen veſſel. To one part of this bark I added two pounds of water, and infuſed them for one hour. Thus I procured an infuſion of bark which I poured on the ſecond part of the bark, and this ſtrengthened infuſion again on the third part, and ſo on to the fifth. But as a certain portion of the infuſion will remain attached to the *wood* of the bark, after the infuſion is poured or drawn off, I added a third pound of water to the firſt part, and then followed up the infuſion on the ſeveral parts till the three pounds of water, or ſo much of them as could be ſeparated from the bark, were united in the fifth veſſel, from which I generally obtained about one pint of ſtrong infuſion of bark.*

To a certain quantity of this infuſion, I added a given meaſure of the ſolution of glue; which formed an immediate precipitate, that may be ſeparated from the infuſion, by filtering paper. When dried it is a ſubſtance, formed by the chemical union of the matter of ſkin with the tanning principle, and is in fact a powder of leather. By ſaturating the infuſion with the ſolution of glue, the whole of the tanning principle may be ſeparated by precipitation.

For the Gallic Acid.

To the pound of bark left in the earthen veſſel and already deprived of its tanning principle by theſe *quick* infuſions, I added a *given* quantity of water to procure a ſtrong infuſion of the gallic acid which requires a longer time (ſay 48 hours). This infuſion when obtained pure† affords little ſigns of the preſence of the tanning principle when tried by the teſt of the ſolution of glue; but with the ſolution of ſulphate of iron it gives a ſtrong black colour (the common black dye), which differs in denſity according to the quality of the bark; this may be further proved by boiling a ſkain of worſted in the dye, by which the gradations of colour will be very perceptibly demonſtrated.

* The ſpecific gravity of this infuſion was aſcertained by an hydrometer, whoſe gradations are inverſe to thoſe of a ſpirit hydrometer.

† It is hardly poſſible from the intimate connection of the two principles, to ſeparate them entirely by infuſion; in the infuſion of tanning principle there will always exiſt a little gallic acid; and in an infuſion of gallic acid a little tanning principle will commonly be preſent, unleſs the infuſion of gallic acid is very weak, and procured by a third or fourth watering.

Having

Having thus obtained a point of comparifon by making a fimilar infufion under fimilar circumftances of any bark or vegetable fubftance, and paying ftrict attention to the fpecific gravity of the infufion, the quantity of precipitate of leather, and the denfity of colour produced by given quantities of one or the other teft, the refult will be a comparative ftatement of the refpective powers of any bark or vegetable fubftance. This comparative ftatement I conceive to be fufficient for all commercial purpofes.

As oak bark is the ufual fubftance employed in the trade of tanning; if a quantity of tanning principle is found to be contained in any other bark or vegetable, the commercial utility of that bark or vegetable may be determined by comparing its quantity of tanning principle and price with thofe of oak bark.

For an accurate chemical analyfis I have tried a variety of acids and fimple and compound affinities; and have purfued the above experiments at the fame time that I was employed on fome in dying, I found the muriate of tin (the method of ufing which is defcribed by Mr. *Prouft*, in the *Annales de Chimie*) very convenient. A folution of it being added to the infufion of bark, forms a precipitate with the tanning principle, leaving the gallic acid fufpended; the precipitate is of a fawn colour; and is compofed of tanning principles and oxidated tin.

By thefe means, I have been enabled to form a comparative fcale of barks; which, however, I do not produce as accurate. Oak bark in its prefent ftate, as procured for commercial purpofes, differs very much in quality from accidental circumftances; the feafon of the year in which it is collected occafions a ftill more important difference; confequently the fcale now produced muft be very imperfect; but I am of opinion that by the purfuits of fcientific men, who may be inclined to inveftigate this fubject more fully, a very accurate fcale may hereafter be found.

In the following fcale I have taken fumach as the moft powerful in the comparative ftatement; leaving, however, a few degrees for a *fuppofed maximum of tanning principle*, which I reckon 20.

Scale of Barks.

Bark of	Gallic acid by colour.	Tanning principle by hydrometer.	Tanning principle (in grains) from half a pint of infufion and an ounce of folution of glue.
Elm* - - -	7	2,1	28
Oak cut in winter - -	8	2,1	30
Horfe chefnut - -	6	2,2	30
Beech - - -	7	2,4	31

* The infufion of elm was fo loaded with mucilage that it was with difficulty I could feparate the tanning principle, or try the fpecific gravity.

Willow

Willow (boughs) - -	8	2,4	31
Elder - -	4	3,0	41
Plum tree - -	8	4,0	58
Willow (trunk) -	9	4,0	52
Sycamore - - -	6	4,1	53
Birch - -	4	4,1	54
Cherry tree - - -	8	4,2	59
Sallow - - -	8	4,6	59
Mountain aſh - -	8	4,7	60
Poplar - -	8	6,0	76
Hazel - - -	9	6,3	79
Aſh - - -	10	6,6	82
Spaniſh cheſnut - -	10	9,0	98
Smooth oak - -	10	9,2	104
Oak cut in Spring - -	10	9,6	108
Huntingdon or Leiceſter willow	10	10,1	109
Sumach - -	14	16,2	158

It is to be obſerved, that the barks do not keep any reſpective proportion in the quality of gallic acid and tanning principle contained in each; which is an evidence of the diſtinctneſs of principle, and may, perhaps, open a new field of ſaving oak bark in dying, as the willows, ſallow, aſh, and others, produce a very fine black. It is alſo worthy of obſervation, that the quantities of gallic acid and tanning principle do not differ in *equal* proportions between the winter and ſpring felled oaks. This fact may lead to the diſcrimination of the proper time for cutting; which is, probably, when the ſap has completely filled and dilated that part of the vegetable intended for uſe. This will make a difference in the ſeaſon of cutting oak, elm, and other trees, ſhrubs, &c. Leaves ſhould be taken when arrived at their full ſize, and then dried under cover; for as the tanning principle is ſo ſoluble, and the ſubſtance that contains it ſo thin (in a leaf), the dew alone might diſſolve it.

Finally, as the gallic acid does not ſeem to combine with the matter of ſkin, and as its aſtringency will corrugate the ſurface, we may, I think, conclude that its preſence in tanning is not only uſeleſs but detrimental.

On

IV.

On the Plumb Line and Spirit Level. By M. CHEZY.*

A WEIGHT fufpended by a long flender and flexible thread, and acted upon by no other force than that of gravity, will ftretch the ftring into a right line perpendicular to the horizon; or which is the fame thing, the plane to which this line is perpendicular is confidered as the horizon itfelf, or a parallel to that circle.

Such a weight affords, therefore, a very fimple and eafy method of determining the level at all places. But this method is not without its inconveniencies, when the utmoft precifion is required, and in levelling this precifion is often neceffary to be had.

It is fcarcely poffible to ufe a thread, or wire, much finer than one fourth part of the thicknefs of an hair. If to this be given a length of ten feet from the point of fufpenfion to the mark intended to be interfected by the wire for the purpofe of afcertaining the fituation of any inftrument; this inftrument will be inconvenient, on account of its magnitude, and of the agitation which the leaft movement of the air will produce, notwithftanding the guard commonly made ufe of. If the wire be fhort or thick, the precifion will be lefs, and even in the firft cafe it will not be very great unlefs the moft fcrupulous attention of a fkilful obferver be applied to infure it. To judge of this degree of accuracy the diameter of a hair was meafured, and it was found that thirty or thirty-five were required to cover the fpace of one line of the Royal Paris foot; that is to fay, the thicknefs of an hair is about $\frac{1}{35}$ or $\frac{29}{1000}$. The fine or arc of one fecond to a radius of ten feet is 0,0069816 of a line, or nearly 0,007, or the $\frac{1}{4}$ part of the thicknefs of a hair; a quantity, fo fmall, as eafily to elude common obfervation, and much lefs than the derangement which the flighteft current of air can produce in the wire.

The fame force of gravity which ftretches the thread by means of a weight, likewife renders the furface of fluids horizontal. If any very fluid liquor, fuch as fpirit of wine, be included in the cavity of a glafs tube, fo as not to occupy its whole capacity, the empty fpace will be conftantly at the upper part of the tube; that is to fay, at one of the ends if its pofition be not horizontal, or in the middle if it be fo; and by a very flight inclination the bubble will be removed to a great diftance from the middle of the tube. A tube of this kind is confidered as being very ufeful to afcertain the pofition of the horizon, with great accuracy; and, for this purpofe, it is very fuccefsfully ufed under the name of the fpirit level.

Thefe levels are commonly made of glafs tubes in the ftate they are obtained at the glafs-houfe. Of thefe the ftraighteft and moft regular are felected and examined, by filling

* On a former occafion, when fpeaking of thefe Inftruments, (Philof. Journal I. 134.) I forbore to enter upon the methods of grinding the Spirit Level; but having fince met with the prefent valuable memoir I have thought it defireable to treat the fubject more fully.—N.

them

them nearly with ſpirit of wine, and aſcertaining by trial that ſide at which the bubble moves moſt regularly, by equal inclinations of the inſtrument upon a ſtage, called the bubble trier, which is provided with a micrometer ſcrew, for that purpoſe. The moſt regular ſide is choſen for the upper part of the inſtrument, the others being of little conſequence to its perfection. Spirit of wine is uſed becauſe it does not freeze, and is more fluid than water. Ether is better becauſe ſtill more fluid (1). The tube and the bubble muſt be of conſiderable length. The longer the bubble the more ſenſible (2) it is to the ſmalleſt inclination. A very ſmall bubble is ſcarcely ſenſible, appears as if attached to the glaſs and moves but ſlowly.

In the uſe of a level of this kind, conſtructed by Sieur *Langlois*, it was remarked, that when it was properly ſet, in the cool of the morning, was no longer ſo in the middle of the day, when the weather became hot; and that when it was again rectified for the middle of the day it became falſe in the evening, after the heat had diminiſhed. The bubble was much longer in cold than in hot weather, and when longer it was too much ſo, and could not be kept in the middle of the tube; but ſtood a little on the one or the other ſide, tho' the inclination was preciſely the ſame. Theſe defects were ſmall, and ſuch as claim the notice of careful obſervers only; but they appeared of too much conſequence not to produce a wiſh to remedy them (3). It was obſerved, that they aroſe from irregularities in the interior ſurface of the tube; and by examining a great number of tubes, ſelected for levels of the ſame kind, there was reaſon to conclude that all theſe levels would have more or leſs of the ſame defects, becauſe there was not one tube of a regular figure within. They were at beſt no otherwiſe cylindrical than plates of glaſs from the glaſs-houſe can be ſaid to be plane before they are ground. The irregularities were eaſily diſcernable.

It was, therefore, concluded, that it would be adviſeable to grind the inner ſurfaces of the tubes, and give them a regular cylindrical or rather ſpindle form, of which the two oppoſite ſides ſhould correſpond with portions of circles of very long radius. To accompliſh this, a rod of iron was taken, of twice the length of the glaſs tube, and on the middle of this rod was fixed a ſtout tube of copper (cuivre) of the ſame length as the tube of glaſs, and nearly equal in diameter to the bore. The rod was fixed between the centres of a lathe, and the glaſs gently rubbed on the copper cylinder, with fine emery and water, cauſing it to move through its whole length. The glaſs was held by the middle, in order that it might be equally ground, and was from time to time ſhifted on its axis, as was alſo the copper cylinder, in order that the wear might be every where alike. The operation had ſcarcely commenced, before the tube broke; and ſeveral others experienced the ſame misfortune, though they had been well annealed. It

was supposed that the emery which became fixed in the copper* might contribute to split the glass, each grain continuing its impression with the same point, in the same right line, which in some instances might be as well disposed to cut the glass as a diamond. A cylinder of glass was substituted, instead of the copper, and the emery rolling itself on the surface of the last, instead of fixing itself, had better success; so that every part of the circumference of the tube and the cylinder touched each other through their whole length. The same operation was continued, using finer and finer emery to smooth the tube, and prepare it for polishing; after which the tube and cylinder having been well washed, thin paper was pasted round the cylinder, and the paper was very equally covered with a small quantity of Venice Tripoli. The tube was then replaced and rubbed as before, till it had acquired a polish†.

A level thus ground, may be either of the proper sensibility, or be too much or too little sensible. It will be too sluggish, if before grinding, exclusive of the irregularities of the tube, its diameter should much exceed in the middle of the length the diameter of the extremities; or it will be too sensible if this diameter should not sufficiently exceed the other; or lastly, if the middle diameter be smaller than that of the extremes, the bubble will be incapable of continuing in the middle, but will, in every case, either run to one or the other end, or be divided into two parts.

To correct these defects, and to give the instrument the required degree of perfection, it is proper to examine its figure before the grinding is entirely finished. For this purpose, after cleaning it well, a sufficient quantity of spirits of wine must be put into it; and secured by a cork at each end. The tube must then be placed on the forks or Y's of a bubble trier, and its sensibility, or the magnitude and regularity of the space run over by the bubble by equal changes of the micrometer screw, must be ascertained. If the run or spaces passed over be too great (5) they may be rendered smaller by grinding the tube on a shorter cylinder; but if they be too short, they may, on the contrary, be enlarged, by grinding on a longer cylinder. It is necessary, therefore, to be provided with a number of these cylinders of the same diameter, but of different lengths, which it is adviseable to bring to a first figure, by grinding them in a hollow half cylinder of brass. By means of these it will be easy to regulate the tube of the level to any required degree of sensibility, after which the tube may be very quickly smoothed and polished.

* I am informed that our artists use brass (cuivre jaune) without this inconvenience. It is probable that the copper being defended by the emery bedded in its face did not itself grind away so as to fit the glass during the operation, and therefore broke it. This property of copper is well known to glass cutters and other artists. A drill, consisting of a very thin copper tube will, with emery, cut a hole through glass very readily; and a small circular plate of the same metal, no thicker than a card, will cut through a piece of glass or the hardest file, without any perceptible wear of the copper itself.—N.

† Our artists find the polishing rather noxious than beneficial, and have, therefore, discontinued it.—N.

The

The level which was thus ground is one foot in length; and the cylinder on which it was first worked is of the same length. When it was finished it was found to be too sensible. It was, therefore, worked on another cylinder of between nine and ten inches long, which diminished its sensibility so far, that the bubble, which is nine inches and four lines long, at the temperature of 16° of Reaumur, above freezing is carried from the middle of the tube exactly one line for every second of a degree of inclination. This degree of sensibility was thought sufficient; but any greater degree which may be required may be obtained by the process here described.

It may be remarked that a glass tube is very subject to be split by grinding its inner surface; the same tube will not be endangered by grinding its external surface even with coarse emery; and when once the polish of the inside is ground off, the danger is over, and coarser emery may be used without fear. Thick glass is more subject to this misfortune than thinner. The coarsest emery made use of in grinding the tube here spoken of was sufficiently fine to employ one minute in descending through the height of three inches in water.

Annotations by the Author of the preceding Memoir.

(1.) If the ether be not well rectified it is subject to two great inconveniences in this use. If the tube be very slightly agitated, the ether divides itself into several bubbles, which employ a considerable time before they unite. In the second place, as this ether is decomposed in the course of time, it deposits very small drops of oil which adhere to the tube, stop the motion of the bubble, and render the level very faulty. The ether is besides more fluid when rectified and freed from a saponaceous matter which causes its bad effects *.

(2.) A level is said to be more sensible the smaller the inclination required to produce a given change in the position of the bubble †.

Let E M F, fig. 1. pl. xvii. represent the internal irregularity of a tube. It may easily be understood that when the elevation of temperature dilates the liquor in the tube,

* Our artists use ether, and find that the precautions here mentioned are very necessary.——N.

† And the more quickly and rapidly it returns to its first situation when the micrometer screw is suddenly returned to its zero. These three qualities do not constantly accompany each other in common levels.—It is not immediately obvious why a long bubble should be more sensible, as the run is exactly the same whatever may be the size of the vacant space. To explain this let A H B I, fig. 5, plate xviii. represent a hollow ring, filled with fluid to A and B on a level with the center C; then if the vertical point H be inclined to either side, the surfaces A and B will have an apparent run, along the tube, of exactly the same quantity. And the same would follow if the tube were filled to E and D, or to G and F. But the actual rise or perpendicular elevation of the fluid required to pass over a given arc will be as the sine of half the arc left empty, or half the length of the bubble, and consequently the momentum by which the cohesive attraction at the edges of the bubble is to be overcome will b. in the same proportion. The longer bubble will therefore move more briskly and settle more accurately to its station.—N.

and

and contracts the bubble, this laſt may occupy the ſpace A B preciſely in the middle of the tube, and the inſtrument being then rectified, will indicate the level. But when it becomes cold, the liquor being condenſed, the bubble will occupy the ſpace C D, which is no longer in the middle of the tube. A good inſtrument ought always to ſhew the level when the bubble is in the middle of the tube, which is the rule followed in practice; it is therefore ſubject to error when the tube is irregular. Irregularities, not diſcernable to the ſight, are of much conſequence to the ſtation of the bubble. Theſe may vary without limit and produce a correſpondent number of variations in the bubble itſelf, which will in ſome caſes be too ſenſible, and in others not enough for, it will be much affected by the ſame degrees of inclination in ſome caſes, and not at all in others, or it may ſtand to the right or left end of the tube without being capable of fixing itſelf in the middle.

(4.) The length of the radius of curvature of the inſide of the tube depends on its ſenſibility, or rather this ſenſibility is in proportion to the length of the radius. The radius of curvature of the ſurface of the earth is likewiſe of ſome conſequence when the ſenſibility is very great.

The ſenſibility of a level may be expreſſed by the ſpace run over by the bubble in the tube, divided by the degree of inclination which has occaſioned the derangement; or if the degree of inclination be ſuppoſed to be a conſtant quantity, as for example one ſecond, the ſenſibility will be ſimply as the ſpace paſſed over.

Let A B, fig. 2, plate xviii. repreſent this ſpace; A C the radius of the earth, and A D the radius of curvature, A b, of the inſide of the tube W when the tube is inclined ſo as to cauſe the bubble to run over the ſpace A B, its radius b D will apply itſelf upon B C the ſame as A D was before applied upon A C. The angle D B C (or D b C, becauſe the ſpace b B may be conſidered as nothing on account of the extreme minuteneſs of the angle b A B) will be the angle of the inclination of the tube, which we ſuppoſe to be conſtantly one ſecond. The angle A C B will be known if the radius of the earth and the ſpace A B be known. The angle A D B is conſtantly the ſum of the two external angles, and may alſo be known if A D and A B be aſcertained. The angles A D B, A C B, being ſubtended by the ſame arc, will be to each other inverſely as the two radii AD, A C. Of three quantities, A C, A D, A B, two being known, the third may alſo be found.

Let A B $= a$, A D $- r$, A C $=$ R, the angle C B D $= b$, the angle A C B $= m$, and the angle A D B $= n$. If the value of r be ſought, every thing elſe being known, we ſhall have $b + m : m :: R : r = \frac{m R}{b + m}$. If therefore a were one line m being nearly $\frac{1''}{13700}$ we ſhall have $r = 238$ toiſes nearly.

If the value of a be ſought, or, which is the ſame thing, the value of m, the reſt being

being known, we ſhall have $R : r :: b+m : m = \frac{r\,b}{R-r}$. If we make $r = R$, m and a will become infinite, if we make $r = \frac{R}{2}$ we ſhall have $m = b$, and conſequently $a = 16$ toiſes nearly. If r be made $= 50000$ toiſes, placing the tube in the direction of the meridian, we ſhall have at the Equator $m = 0{,}015616''$ and $a = 0{,}2461749$ toiſes, and and at the pole $m = 0{,}015353''$, and $a = 0{,}2461187$ toiſes.

If, laſtly, the value of R be ſought, the angle A D B being known, we ſhall have $n - b : r :: n : R = \frac{n\,r}{n-b}$. This determination will always be very uncertain, on account of the extreme ſmallneſs of A B. We have ſeen in the laſt preceding example that a difference of $\frac{1}{24}$ part of a line in A B produces a difference of about 55000 toiſes in A C, though we have ſuppoſed a very great degree of ſenſibility in the tube.

(5.) In order to form a notion how this practice may increaſe or diminiſh the ſenſibility of a tube, let us imagine any two portions of matter whatever to be placed one upon the other, with very fine ſand between them, ſo that they may be worn by friction; for example, two pieces of glaſs. If theſe two pieces A B, C D, fig. 3, of which it is unneceſſary at preſent to conſider the breadth, be of equal length and rubbed one againſt the other in the direction of their length, there will almoſt conſtantly be a part, either large or ſmall, where one will overhang the other, which part conſequently will not be rubbed while the other continues to be acted upon by the friction. The two pieces are preciſely in the ſame ſituation, becauſe the parts B D, A C, which are leſs rubbed than the middle, are equal to each other; each of theſe pieces is therefore equally diſpoſed to become hollow in the middle, which, however, cannot happen, becauſe it is impoſſible they ſhould wear except at the places which touch, and they tend by grinding to touch each other every where in a right line or a circle, which are the only lines that in the different poſitions here ſuppoſed can touch each other every where. This is evidently impoſſible, provided both ſhould become hollow at the ſame time, and the one cannot become hollow ſooner than the other.

But if the two pieces be unequal in length, as in fig. 4, the longeſt of the two will conſtantly project beyond the other; and if the ſhorter one do project, the longer will project ſtill more at the ſame time. It will therefore be more diſpoſed to wear away and become hollow in the middle. This concavity or curvature will increaſe the longer the grinding is continued, while the ſhorter piece will become convex in a circular line; both having the ſame radius in order that they may conſtantly touch throughout*.

(6.) In

* The above reaſoning is not accurate to the extreme of ſtrictneſs. The figure of ſurfaces ground upon each other is liable to vary from partial expanſions, if the work be careleſsly touched by the hand, and in equal pieces of

(6.) In order to obtain very fine emery, and of different degrees of fineneſs, the ſieve is not ſufficient, but water muſt be uſed. After having well ground the coarſe emery on an iron plate with a muller of the ſame metal, it is thrown into a veſſel which muſt be rather wider at bottom than at top, by a gradual increaſe. The veſſel muſt then be filled with clear water ſo that it may ſtand eight or ten inches above the emery. The whole is then to be ſtrongly agitated with an iron ſpatula; after which it muſt be left to ſettle for an hour. The emery falls to the bottom, but the water remains turbid, being charged with emery or ſome other extremely fine and light matter. Into this water muſt be plunged the ſhorteſt branch of a ſyphon filled with clear water to the depth of four inches below the ſurface of the water, holding the other extremity of the ſyphon cloſed with the finger, which is afterwards withdrawn that the water may flow through the ſyphon without ſtirring the veſſel or agitating the emery which is at the bottom. The water thus drawn off, is to be received in another large veſſel, and the firſt veſſel is to be again filled, and its contents agitated as before. The ſame operation is to be repeated till the water paſſes clear through the ſyphon. The powder which has paſſed over with the water and was received in the ſecond veſſel is too fine to be uſefully employed in grinding glaſs. The veſſel being emptied and cleared, the ſame operations are to be repeated, with this difference only, that inſtead of leaving the water to ſettle for an hour, no longer time is employed than half an hour, and when the water paſſes clear through the ſyphon, the operation is diſcontinued, and the emery obtained by ſubſidence from the water, and carefully defended from all impurities, is reſerved under the denomination of emery of half an hour.

The ſame operations being repeated, allowing only a quarter of an hour for the ſubſidence, affords an emery, which is indeed fine, but leſs ſo than the preceding, and is to be reſerved under the denomination of emery of a quarter of an hour.

By ſimilar proceſſes emery may be obtained of half a quarter of an hour, of four, of two, of one, of one half, or one quarter of a minute. To meaſure the half or quarter of a mniute, a pendulum beating ſeconds (or a weight ſuſpended by a ſtring of 39,2 Engliſh inches) may be uſed. The oſcillations are to be counted from the moment of the agitation, and at the inſtant of the thirtieth or fifteenth oſcillation, the finger being withdrawn from the mouth of the ſyphon, ſuffers the water to flow. The ſieve may be uſed for coarſer emery.

of metal it is well known that the figure does not continue ſtrait unleſs particular care be taken not only to avoid partial preſſure and expanſion, but likewiſe to ſhift them with reſpect to each other, ſo that the pieces may alternately be uppermoſt. Theſe and other precautions which are ſufficiently known to thoſe who make metallic ſpeculums are alſo neceſſary to be attended to in the conſtruction of levels.—N.

V.

A Chemical Examination of the Bath Waters. By George Smith Gibbes, M.D. F.R.S.

(Continued from p. 363.)

ONE hundred and ten ounces of the King's Bath water left after evaporation to dryneſs eighty grains of ſolid matter. I evaporated this water in a new tin veſſel, and I found that the ſolid ſubſtance adhered ſtrongly to the ſides of the veſſel. I made uſe of an ivory, knife to detach this ſubſtance, and I obſerved that the knife was thereby conſiderably abraded. This circumſtance induced me to examine very particularly the nature of the products, and I found that a large proportion remained inſoluble after I had ſubjected them to the action of the three ſtrong mineral acids. To this inſoluble ſubſtance, I added above a thouſand times its quantity of diſtilled water; ſtill I perceived that it remained unchanged. This precipitate deſcended rapidly to the bottom of the veſſel. I ſeparated it by a filtre from the water, and I found it to poſſeſs no ſaline or earthy taſte.

In order to procure a larger quantity of this ſubſtance, I evaporated 168 ounces of the King's Bath water nearly to dryneſs. On this reſiduum I poured a conſiderable quantity of nitric acid, and I left it to ſtand for above an hour, I then added a large quantity of boiling water, from which a white precipitate fell rapidly to the bottom of the veſſel. I filtered the liquor, and I found, after carefully drying the ſubſtance left on the filtre, that it weighed twenty grains. The oxalic acid does not decompoſe it, neither was its quantity diminiſhed by boiling it a conſiderable time in diſtilled water.

Profeſſor *Bergman* obſerves, in his Analyſis of Mineral Waters, that the portion, when the other ingredients are ſeparated, which reſiſts the action of a ſufficient quantity of marine acid is ſilicious earth, which may be farther determined by the blow-pipe; for this earth, when added to the mineral alkali in fuſion, unites with it with a violent efferveſcence, and is thereby totally diſſolved.

Mr. *Kirwan* ſays, that the general method of diſcovering the ſilicious earth, is to evaporate a large quantity of water nearly to dryneſs, then to ſuperſaturate and rediſſolve all that may have been precipitated by adding a ſufficiency of nitrous or vitriolic acids, and then evaporate to dryneſs. If then the dry maſs be once more re-diſſolved in water, and filtered, the ſilicious earth will remain on the filtre. It is diſtinguiſhed by its inſolubility in moſt acids, and its vitreſcibility with two parts ſoda.

I expoſed ſix grains of this ſubſtance mixed with double its weight of ſoda, in a ſmall platina crucible to a very ſtrong heat which I urged with a pair of double bellows, and I found

I found that it acquired a vitreous appearance after having suffered a very considerable effervescence similar to that in making glass. I have already mentioned that this substance resisted the action of the marine, nitric and vitriolic acids.

From the foregoing experiments, I am led to believe, that this insoluble substance is silicious earth; I have, however, been very cautious and exact in my experiments to ascertain it, because no one who has hitherto analysed these waters, has mentioned any thing concerning its presence in them. It is a fact generally admitted, that water is capable of holding in solution a very small portion of silicious earth, but under some circumstances, a considerable quantity as in the Geyser and Rykum waters, as admirably proved by Dr. *Black.* The uniform temperature of the Bath waters would lead us to conclude, that they acquire their heat from a continued and powerful operation in the bowels of the earth. The length of time that they have been known to pour forth their hot streams, would induce us to think that the cause is uniform, and very deeply situated in the bowels of the earth. I beg leave to offer a conjecture respecting that regularity of their temperature; which is, that at some great depth in the earth they are at a very high temperature, and that in coming up to the surface of the earth, their temperature is lowered to the degree they are found to possess. Thus they appear to me to be an analogous to the Geyser in Iceland, and that at a certain depth they would be found to have the same appearances. That they are not of the nature of common springs of water is evident to me from their not being affected by the vicissitudes of weather, or by the alternation of wet and dry seasons. As I do not find that there is any portion of disengaged alkali in these waters, and as I cannot discover any substance which can render silicious earth soluble in them, I must hazard the following conjecture respecting its state of combination. I suppose that at a considerable depth these waters are of a temperature capable of dissolving many substances, which water is incapable of, at any ordinary degree of heat, and among them silicious earth. This earth would be therefore extremely divided, and sufficiently so I apprehend, for a part to remain suspended, or even dissolved by them, when they arrive at the surface of the earth. On evaporation, I suppose, these minute particles to coalesce sufficiently to prevent any fresh union with water. There is a fact which corroborates my opinion, which is that after a few years, so great is the deposition of a fine sand in the reservoir, that they are obliged to clear it away to prevent the pumps, &c. from being obstructed.

(To be concluded in our next.)

VI.

Account of the New Gazometer of Citizen SEGUIN.*

CITIZEN Seguin has contrived a Gazometer, or Inſtrument for meaſuring the gaſes, which he propoſes to ſubſtitute to the Gazometer of *Lavoiſier*, and of which the aim is to diſpenſe with the corrections required to be made for the variations of the barometer, during the courſe of the experiment. By this new Gazometer the elaſtic flui[illegible] is ſubjected to an invariable preſſure, by an artificial regulated preſſure, ſubſtituted inſtead of [illegible] the atmoſphere. This preſſure is effected by means of a quantity of water introduced at pleaſure into the reſervoirs appropriated to contain the gaſes.

The inſtrument is compoſed of four reſervoirs. The firſt performs the ſame office with regard to the ſecond as the reſervoir of a fountain lamp; that is to ſay, it obviates the neceſſity of filling it as often as would otherwiſe be the caſe.—The ſecond tranſmits the water into the third, to produce the required degree of compreſſion.—The third receives one of the gaſes and communicates with the fourth, in which the mixture of the gaſes, ſubjected to the ſame degree of preſſure, is made.—Each reſervoir has a kind of gage or level, which ſhows the proportion of water and gas within. The firſt reſervoir communicates with a bottle, which ſhews the ſtate of its contents. A tube or level open at top, and communicating below with the ſecond reſervoir, ſhews the height of the water within.—A level communicating with a third veſſel, above as well as below, that is to ſay, with the part filled with gas, as well as that which contains the water; likewiſe ſhews the relative quantities occupied by the two fluids reſpectively in the veſſel. A cock, of which the tube communicates with the laſt mentioned level, ſerves to empty this veſſel, by ſuffering the water to flow out when gas is required to be introduced into this third reſervoir.—Three tubes or levels are adapted to the fourth veſſel. The firſt placed in the middle, communicates at the ſame time with the parts of this veſſel which reſpectively contain gas and water. It ſhews the relative proportions of water and gas as they exiſt in the reſervoir. Another tube communicating at top with the tube leading from the third reſervoir, and at bottom with the part occupied by water in the fourth, ſhews the degree of preſſure exerciſed by the condenſed gas upon the water of the reſervoirs, and ſtands lower than the firſt level.—The third tube communicates at bottom with the fourth reſervoir, but is open at top. It ſhews the elevation to which water can be raiſed by the preſſure exerted by the gas in the fourth veſſel. It, therefore, ſtands higher than the firſt veſſel by the ſame quantity as the firſt ſtands higher than the ſecond. The author calls theſe gages by the names of the real level, the level of preſſure, and the level of reaction.

* Communicated to the National Inſtitute of France, and abridged by Hallé, in the Bulletin of the Philomath. Society, No. 10, page 75.

This fourth reſervoir alſo receives the water it contains from the ſecond veſſel by a tube for that purpoſe. It receives the gas from the third by a bended tube, plunged beneath the water it contains, through which the gas paſſes by a cap, like that of a watering pot.

Graduated ſemicircles, into the deſcription of which Mr. *Hallé* does not enter, are employed, to ſhew the preciſe ſtate of the contained fluids.

In Plate 17, the numbers 1, 2, 3, 4 repreſent the reſervoirs. A is the pipe through which gas is conveyed into the third reſervoir ; BBB is the bended tube through which the ſame gas is conveyed from the third reſervoir into the fourth ; C, a perforated cap through which the gas is paſſed beneath the water of the fourth reſervoir. D, the tube through which the other gas is conveyed into the fourth reſervoir, to be mixed with the firſt—a, pipe of communication to convey water from the firſt reſervoir to the ſecond ; b, pipe of communication from the external air of the ſecond reſervoir to the top of the firſt ; c, bottle communicating with the firſt reſervoir ; d, pipe of communication from the bottle to the firſt reſervoir ; e, bended tube plunged in the water of the ſecond reſervoir ; f, ſyphon which delivers water from the bottle into a ſmall cap ſuſpended from the neck of the bottle, which bottle itſelf is ſupported by a connection with the firſt reſervoir ; g, pipe through which water is tranſmitted from the ſecond reſervoir to the third ; h, pipe through which water paſſes from the third reſervoir to the fourth ; k, cock for diſcharging the water of the three reſervoirs ; l, level for the ſecond reſervoir ; m, level for the third reſervoir ; n, real level for the fourth reſervoir ; o, level of preſſure for the fourth reſervoir ; p, level of reaction for the ſame reſervoir ; q, graduated ſemicircles to ſhew the ſtate of the contained fluids.

VII.

Inſtructions for making Red Crayons. By A. F. Lomet.*

It is not without difficulty that Crayons of a good quality can be obtained for drawing-ſchools, particularly at a diſtance from the metropolis. The native ochre ſawed into pieces, which are commonly uſed, is almoſt always hard and of an uneven and gravelly conſiſtence ; ſo that the outlines in drawing, for which it is uſed, cannot be made either with the ſoftneſs or the preciſion requiſite to produce the deſired effect. The only good crayons that can be procured have been hitherto manufactured in Paris excluſively, where they have been long ſold at a very high price. The beſt are known by the name of crayons *de pâte du cit. Deſmareſt*, who was probably the inventor. None

* Annales de Chimie xxx. 293.

of

of thoſe who have written on the compoſition of thoſe crayons, having mentioned the quantity of ingredients which ought to be employed, I have made a ſeries of experiments on all the combinations poſſible to be made with the ſubſtances proper for that fabrication. I have rejected thoſe products that have not anſwered the purpoſe of my enquiries, and I here offer thoſe proceſſes which have afforded ſatisfactory reſults.

Theſe crayons are compoſed of ſoft ochre, which is an oxide or bog ore of iron, containing a mixture of earth, of the nature of the clays, which we called hematite. They incorporate it with ſome binding ſubſtance, ſuch as lime, glue, or roſin, mixed ſometimes with ſoap, to take off the hardneſs of the compoſition. Inſtead of the ochre, called Sanguine in French; the other ochres, ſuch as thoſe known by the name of brown-red, or colcothar of vitriol may be uſed. In this caſe it is neceſſary to choſe them ſoft to the touch, and of a lively colour; thoſe that are intended for ſale being often mixed with clay; which gives them a yellowiſh and dull colour, and ought conſequently to be avoided.

I have made the experiment of incorporating thoſe ſubſtances with the white of eggs and the albumen of the blood; but the crayons were not of a good quality. The ſofteſt ochre ſhould be taken and ground with pure water on a marble, as practiſed with the colours uſed for painting, obſerving to moiſten it no more than may be abſolutely neceſſary, to facilitate the ſliding of the muller.

When the operation of grinding in the large way becomes difficult and too expenſive, another method may be practiſed to divide the colouring matters. After pounding they are ſifted through a ſilk ſieve, then diluted with much water in tubs, and after ſtrong agitation, left to ſettle for a few minutes; that is to ſay, for the time ſufficient to ſuffer the coarſeſt parts to fall to the bottom. The water highly charged with the fineſt particles is then drawn off, and left to ſettle for four and twenty hours, when the clear water is alſo decanted off. A very fine powder is thus obtained. The coarſe remainder is to be ground and treated in the ſame manner till the whole maſs be reduced to a ſtate of extreme diviſion.

The gum, the glue, or the ſoap intended to give a ſufficient degree of ſolidity to the crayons, are to be ſeparately diſſolved, and the ſolutions worked up with the grinded ochre. The mixture is then dried by expoſing it to the ſun, or before a fire by a very moderate heat, taking care to ſtir it often till the paſte has acquired the conſiſtence of butter.

The crayons are then to be faſhioned or moulded, which may be done in two ways. The firſt conſiſts in ſpreading the paſte upon a board, in which a number of channels or half round groves are made, of an indeterminate length, but of a depth and width proportioned to the ſize of the crayons intended to be manufactured. The ſecond method, which is the beſt, conſiſts in preſſing the paſte through the pipe of a ſyringe,

whofe orifice is equal to the fize of thefe crayons. The pieces when moulded in this manner, are left to dry. This deficcation ought to be performed flowly, and in the fhade, to prevent cracks, which would be formed, if this precaution were not attended to.

When the pieces are dryed, they are to be divided into lengths of two inches (or five centimetres) each; the edges are rounded off and they are roughly pointed. They muft then be fcraped to remove a hard external furface which they acquire during their drying, and would prevent their marking.

A flight coating of oil muft be fmeared over the wooden mou[illegible]vent the tafte from adhering to the fides.

Gum arabic and ifinglafs are to be preferably ufed. The gum and foap may be diffolved in water; but the ifinglafs muft be firft cut into fmall pieces, afterwards put into warm water and diffolved on a water-bath. Thefe folutions muft be fufficiently liquid to pafs through a hair-feive, and leave their impurities behind.

It is not without difficulty that the pafte incorporates with the folution of glue. Both muft be warm, and the mixture muft be made over the fire at the boiling water heat.

The pafte muft be well mixed together before it is put into the moulds, in order that it may be uniformly incorporated with the folution, and leave no hard parts. It would be better to work it with the painter's muller, and to grind it for a fhort time on the ftone, before it is put into the moulds.

It is only in the crayons which contain gum that foap can be admitted. None of the experiments in which glue and foap have been ufed together have ever fucceeded; and it muft be fo, becaufe the excefs of the alcali of the foap acting on the gelatine deftroys its adhefive quality.

As thofe crayons, into the corporation of which foap enters, are found to afford a darker fhade, it feems that this combination takes a portion of the oxigen from the red oxide of iron, and renders it brown by bringing it nearer to the ftate of the martial ethiops. I have obferved that all the paftes prepared with the oxide of iron, even if with mere water, become brown on their exterior furface during the drying. This effect takes place in a more evident manner when they are expofed to the fun; and it appears to arife from the light taking a portion of the oxigen to the oxide of iron. I fhall hereafter return to the chemical properties of thefe kinds of preparations, but my prefent purpofe is only to difcribe thofe proceffes of fabrication in which I have conftantly fucceeded, in order that they may be repeated with fuccefs.

The crayons compofed after thefe directions have all the good qualities which can be required; they do not coft one fourth of the price of thofe in the fhops; but it muft be noticed that their compofition requires great exactnefs in the quantity prefcribed, be-

caufe

cauſe the leaſt alteration occaſions a conſiderable difference in the quality of the paſte. It is particularly neceſſary to guard againſt thoſe errors which may happen from inevitable diminution, during the time of the manipulation. The beſt way to prevent this, will be to aſcertain by experiments, the quantity of water and grinded ochre which the ſolutions may contain before mixing them.

By means of the quantities ſtated in the following table for each of thoſe kinds of crayons, it will be eaſy to know the proportional quantities of gum, glue, or ſoap, which may be employed for a determinate weight of ochre or red oxide of iron.

Subſtances to be employed, with their quantities and reſults.

No. 1.		
Dry ochre, or red oxide of iron	10. gram.	Theſe crayons are very friable, but they may be uſed for large drawings. Theſe contain the leaſt poſſible quantity of gum. If leſs be uſed they will not have ſufficient conſiſtence to be of any uſe.
Dry gum arabic	1.311 gram.	
No. 2.		
Ochre, &c.	10. gram.	Soft crayons, rather friable, and excellent for large drawings.
Gum	0.363 gram.	
No. 3.		
Ochre	10. gram.	Smooth and ſolid crayons, fit for common uſe.
Gum	0.415 gram.	
Or ſtill better with	0.441 gram.	
No. 4.		
Ochre	10. gram.	Soft firm crayons, for drawings which require delicacy and preciſion.
Gum	0.467 gram.	
No. 5.		
Ochre	10. gram.	Very firm crayons, fit for ſmall drawings, which require to be highly finiſhed.
Gum	0.519 gram.	
No. 6.		
Ochre	10. gram.	Crayons almoſt too hard to be uſed. This is the maximum of gum that can be employed in their compoſition.
Gum	0.571 gram.	
No. 7.		
Ochre	10. gram.	Theſe crayons are of a darker colour than the others mentioned; they are of a very hard conſiſtence, and ſoft to the
Gum	0.380 gram.	
White dryed Soap	0.519 gram.	

touch; but all crayons into whoſe compoſition any ſoap enters, have the defect of making ſtrokes which ſhine too much when retouched. None of my experiments with ſoap ſucceeded. Theſe crayons very much reſemble thoſe of the compoſition of C? *Deſmarett.*

Chemical

VIII.

Chemical Experiments and Observations on the Production of Sugar, and an useful Syrup from indigeneous Plants. By Sigismund Frederic Hermstadt.

(Concluded from page 339.)

(Concluded from page 339.)

h. Experiments with the Beet-root, or Mangold (Beta cicla alba), instituted for the purpose of making Sugar from it.

THE late professor *Maggraf** made experiments fifty years ago with several species of rapes, in order to ascertain whether it be possible to extract from them a sugar fit for use. Among several examined rapes and other species of roots he was particularly succesful, according to his statement, in preparing an useful sugar and likewise a good syrup from the *beta cicla*. From 2lb. of fresh roots he obtained ½lb. of dry matter, and this last yielded to him, by extraction with alkohol, one half ounce of good sugar.

Relying on that experiment, I had a *Scheffel*† of those rapes, namely, the *white mangold* measured and weighed. It held 130 rapes, weighing 68 pounds. They were then mashed and pressed; the residue from the press was infused in luke-warm water, and pressed again. Forty quarts of liquor were thus obtained; which was evaporated to a third part, then mixed with 20 quarts of fresh lime-water, and boiled for half an hour. The juice, which had now a yellow wine-colour, being refrigerated, filtered, and reduced by inspissation to a syrup like consistence, yielded six pounds of an agreeable, brownish-yellow, and transparent syrup. I poured one part of this syrup into a rather deep glass dish, in which a number of glass rods were fixed; and in that state I exposed the whole in a moderately warm place to spontaneous evaporation for the space of sixteen weeks; after which time I found the glass-sticks covered with crystals of sugar, of the size of from a lentil to a pea, and resembling yellow sugar-candy. Therefore, as the *Scheffel* of these mangolds sells for sixteen groschen ‡, and since the other expences in producing the syrup may be estimated at the highest at four groschen, the pound of such *mangold-syrup* will cost three and ⅓ groschen, which sufficiently evinces its use as a substitute for the common syrup from sugar. In the same manner it is practicable, though by a rather slow process, to produce useful sugar from it, which at the

* Experiences chimiques dans le dessein de tirer un veritable sucre de diverses plants, qui naissent dans nos contrées:—in the Memoires de l'academie des sciences, de Berlin, pour l'année 1747. Also in his Chemischen Schriften part ii. page 70—86. Berlin 1767.

† The Berlin Scheffel holds 2604 french cubic inches, old style. The *English bushel* holds 1801, do. And 61,2 Berlin scheffels are equal to 88½ English bushels.—Transl.

‡ Sixteen *Berlin Groschen* make about 2 shillings sterl. so that the pound of mangold syrup at 3½ groschen will cost about five-pence halfpenny.—Transl.

present

preſent high price of that article, would be cheap enough; though dearer than ſugar from the maple, with which laſt it agrees pretty well, as to its goodneſs or quality.

i. *Experiments made with the Runkelrübe* (Beta vulgaris altiſſima. Beta cicla altiſſima, according to Jacquin), *for the purpoſe of making Sugar.*

The runkelrübe is like the preceding, very common; ſo much ſo, that both ſpecies are employed in Thuringen, and in the environs of Brandenburg for feeding cattle. It differs from the foregoing in its very juicy ſtate and ſweet taſte.

The runkelrübes, which I made uſe of for my experiments, were cultivated at *Schoneberg*, near Berlin, in a moderately good ſoil on the eſtates of the privy-counſellor Mr. *Noldechen*. When they are cloſely examined, they offer to view a great variety of appearance. On the outſide they are all of them covered with a red rind; but internally ſome are perfectly white, ſome are variegated with white and red circles, and ſome are diſtinguiſhed from the others by light and deep-red rings. Others again are thick and knobby, while others are thin and long, like beet-roots. I am unable to determine whether this external difference depends on real varieties, or on the various quality of the ſoil in which they grow. Mine were all taken from the ſame kind of ground. In *Thuringen*, my native country, where thoſe rapes, as far as my remembrance reaches, are cultivated as food for cattle, it is probably owing to the goodneſs of the ſoil that they appear larger and more abundant in juice, than here. Their ſize and richneſs in liquor are, beſides, promoted there by hoeing and covering their protuberant part with earth, and alſo by not lopping off their leaves, but preſerving the vegetation upon them till the time of reaping; though in another point of view it would furniſh a good ſupport to cattle. Such rapes as have been cropped or cut, are externally diſtinguiſhable from thoſe that have not. The firſt are leſs ſmooth and have a larger crown, on which all the ſproutings for new leaves are obſervable, the ſecond are more even, and poſſeſs a ſmaller crown.*

One *Scheffel*, of theſe runkelrübes, held 112 pieces of various ſize, and weighed 125 pounds. After they were ſeparated from the crown and outer rind, they were grated, and the pulp, from which a portion of juice ſpontaneouſly drained off, was ſtrongly preſſed. I thus obtained from this ſcheffel of rapes 24 quarts of a violet-coloured and very ſweet liquor. I firſt reduced it without any addition, to a third part by boiling, which by experience I find to be the beſt method. During this proceſs a

* Theſe rapes are remarkable for their extremely great quantity of juice. I had 5 of them cut into thin ſlices without paring them, and ſuffered them to dry upon a chamber-ſtove; after which only 17 ounces of a dry maſs remained, exhibiting a taſte ſimilar to that of liquorice; which attracted moiſture from the air. Hence one pound of freſh Runkelrübe affords only two ounces dry matter.

run

vaft quantity of albuminous matter was feparated. After refrigeration, the liquor was run through a woollen cloth, then mixed with 24 quarts of lime-water, and boiled together with it for half an hour. All the juice now became a clear fluid of a yellow colour of wine, and there was ftill feparated a large quantity of impurity in the form of fcum. I fuffered it again to cool, paffed it through flannel, and when the whole was infpiffated, it yielded me eight pounds of a well flavoured fyrup.

In the fame method as was ufed with the white rape, I expofed one part of this fyrup to gentle exhalation, in a rather deep glafs-bowl with glafs-rods placed in it. After the lapfe of eight weeks I obtained by this method a real brown-yellow fugar, cryftallized around the fticks and refembling fugar-candy. But as in this fyrup, as well as in the preceding, the cryftallization goes on very flowly, I cannot well afcertain at prefent what quantity of dry fugar may be conveniently produced from it.—However, as this fyrup is more agreeable to the tafte than the common one, and becaufe all the expences being deducted, one pound of it does not amount to above one grofchen ($1\frac{1}{2}$ Engl.) on this double account it may, like that from the mangold, be ufed inftead of common fyrup.*

k, Experiments with the Beet-root (Beta rubra. Beta cicla rubra, according to Jacquin.) *for extracting Sugar from it.*

The poffibility of making genuine fugar from the beet-root, has already been proved by *Marggraf*. (lococit.) On this account I fubjected it to the fame treatment as the former fpecies. From one *fcheffel* of beet-roots I obtained $6\frac{3}{4}$lb. of fyrup, of an harfh, difagreeable tafte, and of much inferior goodnefs to thofe produced from the white mangold and the runkelrübe. This unpleafant additional tafte might probably be deftroyed by fuitable management. I have not yet attempted to extract fugar from it. *Margafra* obtained from one pound of frefh beet-roots, 12 ounces of a dry mafs, and 16 ounces of the dried roots, afforded him $2\frac{1}{2}$ drams of fugar.

* When at another time, I had heated a portion of the liquor from the Runkelrübe with milk of lime, *(lime diluted with water to the confiftence of milk)* inftead of lime-water, I added rather too much; in confequence of which, the whole fluid acquired a tafte refembling that of acetite of lead. And by fubfequent gentle evaporation there cryftallized from it a large portion of malat of lime; from which circumftance it follows that, this rape contains much malic acid, which on one hand renders the cryftallization of the fugar difficult, and on the other hand, poffeffing itfelf a fweetifh tafte, it increafes the quantity of the fyrup procured from the rape. To conclude, on continuing my experiments on this head, I find I fucceeded beft in feparating the fugar, when I fuffered the fyrup to evaporate flowly at a temperature of 70° *Reaum.* ftirring it round now and then, and once only at a time. A larger portion of grained fugar is then depofited at the bottom of the veffel; which cannot however, unlefs by repeated folutions and cryftallizations be produced in the ftate of fugar, which fhall continue dry. I have not yet tried, what may be the habitudes of this rape-fugar, when treated under a layer of clay.

l. Experiments

l. Experiments with Carrots, (Daucus carotta Lin).

Carrots when cut ſmall and boiled with water, produce a ſweet liquor, by the evaporation of which the country-people uſually prepare a ſweetiſh juice under the name of carrot-juice. They either eat it ſpread upon bread, or uſe it to ſweeten their food. It is alſo ſold by them to grocers, who mix it with the common ſugar-ſyrup. But as the common juice from carrots is too much contaminated with mucilaginous parts to admit of being uſed as ſyrup, and has beſides a diſagreeable taſte, I treated it in the following method.

One *ſcheffel* full of carots was cleared of their external thin rinds, then rubbed ſmall on a grater, and the juice expreſſed. This liquor was clarified by boiling with ſome whites of eggs, and reduced to the conſiſtence of a ſyrup. In this manner I procured 6¼lb. of ſyrup, not unpleaſantly taſted, but much inferior in quality to the ſyrups obtained from the mangold and runkelrübe. Nor did I find any means of obtaining a true ſugar from it. Alkohol extracts from it a ſubſtance, very much ſimilar to manna.

m. Experiments inſtituted with Turneps (Braſſica rapa), *for the purpoſe of making Sugar.*

I cauſed twelve turneps to be peeled and ſeparated from their crowns, and then to be reduced to a pulp by means of a grater. The pulpy maſs exhibited a ſweet, agreeable, yet ſlightly ſharp taſte, and yielded after proper expreſſion a colourleſs, ſweet juice. When clarified with a little white of eggs, the clear juice was ſtrained through a woollen cloth, and thickened to a ſyrup. It was then of an agreeable taſte, which, though inferior to the ſyrups produced from the mangold and runkelrübe, deſerves to be ranked with the common ſyrup of ſugar. By another experiment I found that one *ſcheffel* holds 125 of theſe rapes, and weighs 116 pounds; and that eight pounds of ſyrup may be obtained from it. Twelve weeks were elapſed when I found ſugar cryſtals had ſhooted; which, however, were very brown, and could not be ſeparated but with difficulty from the other remaining fluid.

n. Experiments with the Rape, or Cole-wort, (Braſſica napobraſſica).

The quantity of juice contained in this rape, as well as the agreeable taſte of its juice, induced me to examine it for ſugar and ſyrup. Sixty of them weighing together 123 pounds, and were freed from their external coat, and then finely grated and preſſed. They yielded 22 quarts of colourleſs juice, which was agreeably ſweet, but had an additional ſomewhat ſharp taſte like radiſhes. I ſuffered it to boil up twice, during which operation a great quantity of flocculent matter ſeparated, and the liquor became as clear as water. After cooling it was filtered, diluted with 20 quarts of freſhly pre-

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pared lime-water and gently boiled down. The ſharp radiſh-like flavor was driven off during the boiling, and I obtained at laſt ten pounds of a tranſparent brown-yellow ſyrup, in every reſpect nearly reſembling the ſyrup obtained from the black birch (d,) and exhibiting the ſame foreign taſte. It might be profitably uſed inſtead of the common brown ſyrup. For the purpoſe of producing ſugar from this ſyrup, I expoſed a portion of it to ſlow evaporation in a glaſs veſſel, in which ſome ſticks were placed; and after ſome weeks ſmall ſaccharine cryſtals were formed, the quantity of which I was unable to aſcertain. Yet, this rape being dearer than the white mangold and runkelrübe, and yielding a much leſs agreeable ſyrup, than even the turnep itſelf, it muſt; in this reſpect, always give way to thoſe three ſpecies.

o. Experiments with the Skirret. (Sium Siſarum, Lin).

Marggraf (loco citato) obtained from one pound of freſh ſkirrets $4\frac{1}{2}$ ounces of dried extract, and from $\frac{1}{2}$lb. of dried roots he obtained three drams of genuine ſugar. My experiments were this time only directed towards producing a ſyrup that might be fit for uſe; becauſe the extraction of dry ſugar, as was obſerved by *Marggraf* himſelf, is ſubject to many difficulties, on account of the many farinaceous parts theſe roots contain. As theſe roots, from their ſmallneſs, are not eaſy to be grated, I had one *ſcheffel* of them, weighing 26 pounds, bruiſed, with the addition of cold water in a ſtone mortar, and the pulp afterwards preſſed. The reſidue was again moiſtened with cold water, and preſſed a ſecond time, by which management I obtained a turbid ſweet juice. I left this ſtanding for eight days in a cold place, during which a great portion of mealy ſubſtance fell down, and the liquor became clear. It was then clarified with a ſmall portion of white of eggs, and afterwards inſpiſſated; and thus it afforded five pounds of a pleaſant, light-brown ſyrup. It is evident, therefore, that theſe roots are much too dear to be employed in making a cheap ſyrup.

p. Experiments with Parſneps.

(Paſtinaca ſativa, Lin.)

The particular ſweetneſs of this root, which in other reſpects is very ligneous, induced me to ſubject it to ſimilar experiments, though *Marggraf* had already obſerved, that only an inconſiderable quantity of ſugar can be procured from it.

With this intention I treated one *ſcheffel* of the roots, weighing 24 pounds, exactly in the ſame manner as the ſkirrets. The ſyrup amounted to $5\frac{1}{2}$ pound weight, of an agreeable flavour, but did not thoroughly loſe the particular taſte of the parſnep.

If my other occupations had permitted, I ſhould not have neglected to aſcertain the relative proportions of real ſugar contained in a certain determinate quantity of the ſubſtances ſubjected to examination. Such an attempt was at that time out of my power,

and

and I muſt defer the experiments I mean to make for this purpoſe to another opportunity.

Appendix to the Experiments for producing Sugar from indigenous materials. By SIGIS. FRED. HERMBSTADT*.

In the continuation of my experiments on the runkelrübes, I endeavoured to aſcertain the quantity of real ſugar, which is obtainable from a certain quantity of the rape. With this view I weighed accurately three pounds three ounces of the ſyrup obtained by my ſecond experiment, which I poured into a conical veſſel of tinned copper, and left it ſtanding in a temperature of from 65 to 70° Reaum. for ſlow evaporation. After the expiration of eight hours a cruſt of grain or cryſtals of ſugar was formed on the ſurface, which after 24 hours was nearly one third of an inch thick. This was puſhed down, and ſank in the liquid ſyrup to the bottom. Two days afterwards a new cruſt was formed, and again puſhed down. I repeated this operation till a pellicle appeared on the ſurface of the remaining ſyrup, which was not cryſtalline, but merely tenacious. This pellicle indicated that all the cryſtallizable ſugar was now ſeparated from the fluid; which was alſo confirmed by the taſte of the reſidual ſyrup, which was leſs ſweet and more mucilaginous. At this period I transferred the whole of the cryſtallized ſugar, together with the remaining fluid ſyrup into ſmall ſugar-loaf moulds, which I kept for the ſpace of eight days in a temperature of 30° Reaum. During this time all the liquid ſyrup was drawn off, and the ſugar remained nearly in a dry ſtate. The whole operation laſted 36 days, and the raw ſugar, thus obtained, was ſomewhat moiſt within, though it did not liquefy in the open veſſel in which it was left during three weeks. Its weight was two pounds two ounces. The ſyrup, which ſpontaneouſly drained off, amounted to 12 ounces. Hence of the 3lb. and 3 ounces of ſyrup made uſe of, only 5 ounces of watery fluid evaporated. According to this experiment, the *ſcheffel* of theſe runkelrübes would afford about $5\frac{1}{3}$lb. of brown raw ſugar, and $1\frac{5}{8}$lb. of a ſyrup, not quite ill-taſted, and which if not uſed as ſyrup, may with great profit ſerve to make very good brandy by diſtillation. It does not ſignify whether thoſe runkelrübes be cultivated by a particular method or no; for the ſugar and ſyrup were obtained from them in the ſtate in which they grew, near Schoenberg on a moderately good ground. At preſent I am occupied with the refining of my raw ſugar, thus procured, in order to aſcertain what quantities of the finer ſpecies of ſugar, and how much ſyrup I may receive from it.

HERMBSTADT.

* Likewiſe from the Neu. Schr. d. Geſell. d. naturforſch. Freunde zu Berlin. page 450—52.

IX.

On the silent Escapement of Mr. Goodrich. By a CORRESPONDENT.

TO MR. NICHOLSON.

SIR,

I WAS much pleased with the simple escapement of Mr. *Goodrich*, described in your last number, and in common with the respectable Society for the Encouragement of Arts, and yourself, and I doubt not the Inventor also, thought it as new as it is ingenious. But as I have since met with the same Invention, I send you the following notice, and suppose you will join me in opinion that Incidents respecting Improvements in the Arts have a claim for insertion in your miscellany.

In the Volume of *Machines et Inventions approuvées par l'Academie royale des Sciences*, tome viii. page 325, there is an account, with an engraving, of a Crank Escapement, invented in the year 1746, by *L'abbè Soumille*, and a Report explaining its qualities and advantages at length, by Messrs. *Hamel* and *Camus*.

I am, Sir, your obliged servant,

ONE OF THE PUBLIC.

London, 12*th Nov*. 1799.

X.

Results of various Experiments for determining the Quantity of Action which Men can afford by their Daily Work, according to the different manners in which they employ their Strength. By Citizen COULOMB.*

TO give a clear and precise account of this interesting memoir, it is in the first place necessary to fix the meaning of the words *quantity of action*.

The effect which results from the mechanical labour of men can always be reduced to the ascending motion of an heavy body. The velocity with which such a body ascends, would soon be destroyed, if the cause that produced it were to discontinue its action; and it is necessary that the man should make a continual effort to keep it up. Here, therefore, are two quantities capable of numerical enunciation; the velocity which is the number of metres or unities of space uniformly passed over, during the

* Communicated to the National Institute of France, and abridged by *Prony*, in the Bulletin de la Soc. Philomath. No. 16. Messidor, Aug. 6.

unity

unity of time, and the effort which has for its expreſſion and meaſure a certain number of kilograms or unities of weight. The produce of theſe two numbers repreſents and meaſures the action; and this product multiplied by a third number, which is the time, during which the action continues, gives the quantity of action or total effect reſulting the work, which is, therefore, an object capable of being meaſured and ſubjected to computation.

Theſe notions being eſtabliſhed, the fundamental object of reſearch is the compariſon of the work with the *fatigue*, which is the neceſſary conſequence. Any determinate quantity of action (or the number which repreſents it) may be afforded by an infinity of different combinations of the values of numbers, of which the product ſerves as a meaſure; and theſe combinations depend on the various manners in which the force of men is employed. Is the fatigue equal in all caſes for equal quantities of action, or does it vary when in different circumſtances the numbers are varied which repreſent the velocity, the time, and the effort; ſo as, nevertheleſs, to preſerve the ſame conſtant product? *Daniel Bernouilli*, and other celebrated authors have adopted the former opinion; but *Cit. Coulomb* ſhews that they were deceived, and by refuting, by proofs drawn from reaſonings and experiment, an opinion, ſupported by names ſo reſpectable, he has rendered a great ſervice to the ſcientific application of mechanics.

But though fatigue be not ſimply proportioned to the quantity of action, it is one of its functions; that is to ſay, the formula which repreſents it ought to include in ſome manner the velocity, the effort, and the time. It is known by the theory of mathematical analyſis that there muſt, therefore, exiſt a certain relation between theſe three things, ſuch as that a given effect may be produced with the leaſt fatigue; or which is the ſame thing, that when the fatigue is the ſame the quantity of effect or total action may be a maximum. This is the problem which the author has propoſed to ſolve, and in which he has conſidered the various methods of employing the force of men.

In the firſt place he examines the quantity of action which men can produce, when, during a day, they mount a ſet of ſteps or ſtairs, either with or without a burthen. The experiments he mentions on this ſubject immediately prove the falſity of the opinion of *Bernouilli*. He found that the quantity of action of a man who mounts without a burthen, having his own body to raiſe only, is double that of a man loaded with a weight of 68 kilogrammes ($149\frac{1}{2}$lb. avoirdupois,) both continuing at work for a day. Hence it is ſtrikingly obſervable, how much, with equal fatigue and time, the total or abſolute effort may obtain different values by varying the combinations of effort and of velocity.

But the word effect here denotes the total quantity of labour employed to raiſe, not only the burthen, but the weight of the man himſelf; and what is of the greateſt importance to conſider, is, *the uſeful effect*; that is to ſay, the total effect, deducting the value

value which reprefents the tranfport of the weight of the body of the man. This total effect is the greateft poffible when the man afcends without a burthen; but the *ufeful effect* is then nothing; It is alfo nothing if the man be loaded fo much as to be fcarcely capable of moving: and, confequently, there exifts between thefe two limits a value of the load, fuch, that the ufeful effect is the greateft poffible. Citizen *Coulomb* fuppofes that the lofs of quantity of action is proportional to the load (which hypothefis is confirmed by experience) whence he obtains an equation, which treated according to the rules of maxima and minima, gives 53 kilograms (117lb, avoirdupois) for the weight with which the man ought to be loaded, in order to produce, during one day, by afcending ftairs (un efcalier) the greateft ufeful effect; and the quantity of action which refults from this determination, which has for its value 56 kilograms, raifed through one kilometer, does not fenfibly differ from the refults of experience. But this method of working is attended with a lofs of three fourths of the total action of men, and confequently cofts four times as much as work, in which, after having mounted a fet of fteps without any burthen, the man fhould fuffer himfelf to fall by any means, fo as to raife a weight nearly equal to that of his own body.

The author afterwards examines the work of men walking on an horizontal path, with or without a load. This method is here fimilar to the preceding, and affords fimilar refults. The greateft quantity of action takes place when the men walk without being loaded; and is to that of men walking under a load of 58 kilograms, nearly as 7 to 4. The weight which a man ought to carry, in order to produce the greateft *ufeful effect*, namely, that effect in which the quantity of action relative to the carrying his own weight is deducted from the total effect is 50,4 kilogrammes (or 112 pounds, avoirdupois.)

There is a particular cafe which always obtains with refpect to burthens carried in towns; namely, that in which the men, after having carried their load, return unloaded for a new burthen. The weight they fhould carry in this cafe, to produce the greateft effect, is 61,25 kilograms, or nearly 135 pounds avoirdupois. The quantity of ufeful action in this cafe, compared with that of a man who walks freely and without a load, is nearly as 1 to 5, that is to fay, he employs to pure lofs $\frac{4}{5}$ of his power.

The author afterwards fucceffively treats of the cafes in which men convey burthens on a wheel-barrow, or raife a weight for driving piles, or turn a handle. Under each article he gives the abfolute and comparative refults, comparing each kind of labour to the other methods of employing the forces of man. He finds that by caufing him to mount a fet of fteps freely and without burthen, his quantity of action is at leaft double what he affords in all thefe other methods of employing his ftrength. The limits of this abftract will not permit us to follow him to a greater extent, for which reafon we fhall confine ourfelves, in order to give a notion of his method to what we have

have ſaid of man walking in an inclined or horizontal plane. His memoir is concluded by the conſideration of the labour of men employed to cultivate the ground. He has found by experience, that the whole quantity of action thus afforded during a day amounts to 100 kilograms (220lb.) elevated to one kilometer.

Afterwards comparing this work with that of men employed to carry burthens up an aſcent or ſteps, or at the pile engine, he finds a loſs of about $\frac{1}{20}$ part only of the quantity of action which may be neglected in reſearches of this kind.

The author takes great care to warn readers againſt experiments of too ſhort duration, and in ſeveral places ſpeaks of the error to which we are expoſed by making experiments with men of more than common ſtrength. The mean reſults have alſo a relation to the climate. "I have cauſed," ſays the author, "extenſive works to be executed by the troops at Martinico; where the thermometer is ſeldom lower than twenty degrees; I have executed works of the ſame kind by the troops in France; and I can affirm that under the fourteenth degre of latitude, where men are almoſt always covered with perſpiration, they are not capable of performing half the work they could perform in our climate."

XI.

New Reſearches into the Affinities which the Earths exert upon each other in the humid and in the dry way. By Citizen GUYTON.*

IT was not even ſuſpected in the earlier times of chemiſtry that an elective attraction could be exerted between two pure earths. Their union was thought to be the conſequence of a fortuitous aggregation, a ſimple adheſion referable neither to elective attraction nor the equilibrium of compoſition. But when it was obſerved that two earths infuſible ſingly in our furnaces, ſuch as ſilex and lime, were fuſed together with conſiderable facility into an homogeneous matter, of which the principles could no more be ſeparated unleſs by chemical methods; it became neceſſary to admit of a mutual action which became active as a certain temperature, that is to ſay, a true chemical affinity.

The firſt ſtep being made, it ſeems as if chemiſts would not have delayed to multiply their proofs by direct experiments; but it is nevertheleſs true, that very few facts are yet eſtabliſhed in the dry way, and that it begins to be found neceſſary to interrogate nature in the humid way, of which the proceſſes are ſo much the more important, as they more nearly approach the very methods uſed by nature to form theſe earthy compounds, which

* Extracted from a Memoir, read at the ſitting of the National Inſtitute of France on the 16 prairial laſt, (June 3, 1799,) inſerted in the Annales de Chimie xxxi. 246.

conſiſting

consisting of the elements themselves in different proportions, exhibit such a variety of properties.

These considerations have determined me to present the results of my experiments in the humid way to the institution. I am persuaded that they will also conduce to a proper examination of the result of the dry processes under the true point of view.

§. 1. *Experiments in the Humid way.*

If two solutions, each formed of the same solvent and a different substance, be mixed there can be no change of equilibrium and decomposition, unless the two substances held in solution in the same fluid, exert upon each other an attraction more powerful than that which the solvent exerts upon them; and if these substances unite in the concrete form it will be an unequivocal proof, not only of an attraction capable of producing combination, but likewise *an elective attraction*, which destroys one compound in order to form another.

Such are the reasons according to which I have conducted my experiments. The results are as follow.

Experiment 1. I mixed ten centilitres of lime-water with two centilitres of barytic water, the specific gravity of the latter being 1,138. Clouds were speedily formed which fell to the bottom.

It might be expected that the lime-water might contain sulphuric acid which is so often contained in waters and in lime-stones mixed with gypsum. That which I used was previously essayed by the muriate of barytes which produced no change.

Experiment 2. I dissolved pure alumine in a solution of pot-ash. On the other hand I prepared the solution of silex in the same salt by the usual methods. Here were two earths separately combined with the same solvent. The two fluids after filtration were very clear and moderately concentrated; the former had a scarcely perceptible yellow tinge; the latter was faintly green.

I mixed equal parts of each. As soon as the fluids came into contact, a brownish zone was formed, which by agitation diffused itself through the mass, and gave it a fawn colour.

The mixture did not appear to undergo any change for near an hour, though it was agitated with a spatula of glass, but at the end of that time the whole mass was whitish, opake, and of the consistence of a thick jelly.

This experiment as well as the foregoing was repeated in the chemical course of the second division of the polytechnic school on the 29th ventose of the last year, and presented absolutely the same appearances.

It is not possible to obtain a more direct proof that earths, of the number of those in which there is the least reason to suspect alkaline properties, are capable even in the humi

humid way and in pot-ash, to exercise upon each other an elective attraction, which exceeds that which unites either of them to the alkaline solvent.

Experiment 3. The action of lime on silex in the humid way, is demonstrated by the precipitation which lime-water effects in the solution of silex in pot-ash. The precipitate is a true combination of silex and lime as M. *Gadolin* has announced from having seen the experiment performed by me three years ago in my course * at the Polytechnic School.

An earlier observation had before pointed out the truth to us, namely, that glasses, into the composition of which too great a quantity of lime had been admitted, and which are in consequence soluble in acids, do not afford in these solutions either a speedy or a ready separation of the silex from the lime; but, on the contrary form crystalline depositions, which must be treated again in order to have the complete analysis.

I was desirous of ascertaining whether the same phenomenon would take place with barytes. I poured barytic water into a solution of silex in pot-ash; the mixture soon became turbid. The precipitate dried on the filtre was digested in acetous acid, which dissolved o. 3.—It is to be observed, that the precipitate afforded by lime-water poured into the solution of silex in pot-ash when treated in the same manner with acetous acid, lost no more than o. 1. of its weight.

Experiment 4. The action of strontian upon silex was tried in the same manner, and a decomposition also took place, But of 110 parts of this precipitate dried on the sand bath, the muriatic acid redissolved 45 in a digesting heat.

Experiment 6. The aqueous solution of strontian was mixed with that of barytes; no signs of new combination were observed.

Experiment 7. I have long since shewn that the carbonate of barytes like that of lime is soluble in excess of its own acid; and the same property has been proved to exist in the carbonate of strontian. This, therefore, afforded a new means of putting the affinities of the earths into action. The mixtures varied with this intention afforded no phenomenon, which indicated an attraction of sufficient power, either to separate them from the carbonic acid, or to unite them in the state of carbonate.

Experiment 8. These results which even by their variety, appear to characterize more particularly that unequal power of union which we call elective attraction, or affinity, induced me to make experiments on the solutions of earths in more powerful acids. They afforded me unequivocal proofs not only that there exists among the earths a tendency to unite in the humid way; but also that with regard to some of these earths, the union is such as to be capable of resisting an addition of the acid by excess.

* Annales de Chimie xxii. 109 and xxvii. 320.

These obfervations are too intimately related to the formation of ftones, and the art of analyfing them to admit of the principal refults being neglected in this place. They will, no doubt, be thought of equal importance when they announce that the earthy falts were not decompofed, as when they fhew the poffibility of that effect being accomplifhed.

I made a mixture of equal parts of a folution of muriate of lime and of muriate of alumine. The fluid became turbid almoft immediately, even before it was agitated. This effect which was firft feen in the upper part, was very rapidly communicated to the lower; after which the fluid became opake and almoft gelatinous, ftrongly colouring blue paper, and the precipitate did not difappear but by the addition of new acid.

Experiment 9. The mixture of muriate of lime and muriate of magnefia, gave no figns of new combination.

Experiment 10. The folution of muriate of barytes, afforded at the end of three or four minutes, an abundant precipitate, which was not rediffolved by excefs of acid.

Experiment 11. The mixture of the folutions of muriate of lime and muriate of ftrontian, exhibited no change.

Fxperiment 12. The mixture of the folutions of muriate of magnefia and muriate of alumine, affumed at the end of a few minutes, a milky appearance; but the effect proceeded no farther, whether the folution were neutral, or with excefs of acid.

Experiment 13. Muriate of magnefia and muriate of barytes, mixed in folution, afforded an abundant precipitate, which was not rediffolved by the addition of more acid.

Experiment 14. Nothing of this kind was obferved by mixture of the folutions of muriate of magnefia and muriate of ftrontian, in any proportion.

Experiment 15 If the folutions, even diluted, of muriate of barytes and muriate of alumine be mixed, the fluid becomes immediately turbid, and affords a copious precipitate. *(To be continued.)*

XII.

Account of the improvements made on the Farm in the great Park of his Majefty, the King, at Windfor. By NATHANIEL KENT, *Efq.**

SIR,

UPON mentioning to you fome time fince that there had been fome practices in hufbandry on his Majefty's farms under my fuperintendance in Windfor Great Park, which I conceived were not generally known, and upon your giving me reafon to

* Addreffed to the Secretary of the Society for the Encouragement of Arts, and inferted in their Tranfactions for the prefent year.

think

think the society for the Encouragement of Arts, &c. from its laudable desire to communicate to the public every thing that promises advantage to it, would not be unwilling to allow me a few pages in its next publication; and being indulged with his Majesty's gracious permission to state any matter that I may discretionally think proper to communicate, I am induced to lay before you a few particulars which some Gentlemen and farmers, under similar circumstances, may, perhaps, think deserving notice.

But before I enter upon any particular description of what I have to offer, it will not, perhaps, be uninteresting to the Society to know the grounds upon which his Majesty's large system of agriculture has been founded.

In the year 1791, the Great Park at Windsor, about 4000 acres, fell into his Majesty's possession. It might truly be called a rough jewel. The whole, as a natural object, was grand and beautiful, of a forest appearance; but the parts were crowded and indistinct. The soil was various, some parts clay and loam, and some sharp gravel or poor sand; a great part of the former was covered with rushes and mole-hills, and the latter with fern and moss.

About 1000 acres of the lightest part were separated from the rest at one extremity, and formed what is called the Norfolk farm; about 400 acres more at the other extremity, of a good loamy soil, were separated and called the Flemish farm; both being named from the nature of the husbandry meant to be adopted on them.

The rest (about 2,400 acres) remains still in plantations and park, and though so much reduced, yet from the improvements which have been made upon it, is now capable of carrying more stock than the whole 4000 acres did before. All the unsound wet parts have been drained by the Essex mode, so as to be rendered firm and productive of an improved herbage. The mole-hills have been levelled chiefly by dragging, and the coarse and mossy parts fined by repeated harrowing and rolling (being one of the first improvements upon park land of this description) besides which a variety of beauty has been laid open by clearing the valleys and low parts, to give a bolder effect to the woody scenes upon the higher grounds; and by making judicious openings so as to break strait lines and separate parts that were in some places too heavy and samely; so that the same extent of land has not now only a much larger appearance, but exhibits a much greater variety of ground. The truth of this every impartial person who knew the place before his Majesty caused these improvements to be made, must allow. I have only to add, that tho' prejudice may have taken up an idea that there has been too great a sacrifice of timber in effecting these improvements truth will deny it. There has not been a tree taken down but what was either in decay or removed either to give room for the growth of others, or to set them off to greater advantage in picturesque appearance.

I come now to the object in view as before hinted, which is to state the motives which I am inclined to think induced his Majesty to adopt the farming system upon so large a

 scale,

ſcale, and next to ſhew the reſult. Theſe I conceive were chiefly to create uſeful labour for the induſtrious poor in the neighbourhood, and for trying experiments in agriculture to exite imitation where ſucceſs might encourage it.

The Norfolk farm borders on that extenſive waſte called Bagſhot Heath, hitherto conſidered too barren for cultivation, though large tracts of a ſimilar quantity have been long ſince rendered uſeful to the community in the South-weſt part of Norfolk. Arable land of this deſcription is generally managed there under a five courſe ſhift; firſt wheat; ſecond turnips; third barley with ſeeds, which continue laid two years. But as the ſeeds turn to very little account after the firſt year, his Majeſty's, which though a five courſe ſhift likewiſe, of one hundred acres in a ſhift, is upon a much improved courſe; as thus—firſt, wheat or rye; ſecond, the irregular ſhift; third, turneps; fourth, barley or oats; fifth, clover.—The irregular ſhift, which is of great uſe on a light land farm, may, perhaps, want a little explanation. It is meant to be partly productive and partly preparative. Forty acres of it are ſown with vetches to be fed off; forty-one ſown the latter end of Auguſt with rye, for early feed the next ſpring for the ewes and lambs; the remaining twenty acres are planted with potatoes, and the whole comes round for turneps the next year.

From the advantage of running ſheep in the park this farm has been brought ſurpriſingly forward, conſidering the ſhort time it has been cultivated, and a great part of it which produced nothing but heath and moſs, and would have been dear at five ſhillings an acre to rent, now produces crops worth more than the original fee ſimple of the land.

Brevity checks me from going farther into a general deſcription; but the following particulars may deſerve notice.

The comparative advantages of the labour of horſes and oxen have been for ſome time under the conſideration of the public. His Majeſty has unqueſtionably tried the latter upon a larger ſcale than any other perſon, as he does not work leſs than one hundred and eighty oxen upon his different farms, parks, and gardens, and has found them to anſwer ſo well that there is not now a horſe kept. Upon the two farms and the great park two hundred are kept, including thoſe coming on and going off. Forty are bought in every year, riſing three years, and are kept as ſucceſſion oxen in the park; one hundred and twenty are under work, and forty every year are fatted off, riſing ſeven years.

The working oxen are moſtly divided into teams of ſix, and one of the number is every day reſted, ſo that no ox works more than five days out of the ſeven. This day of eaſe in every week, beſides Sunday, is of great advantage to the animal, as he is found to do better with ordinary keep and moderate labour than he would do with high and harder labour. In ſhort, this is the firſt ſecret to learn concerning him; for an ox will

not

not admit of being kept in condition like a horſe, artificially, by proportionate food to proportionate labour.

Theſe oxen are never allowed any corn, as it would prevent their fatting ſo kindly afterwards. Their food in ſummer is only a few vetches by way of bait, and the run of coarſe meadows, or what is called leaſows, being rough, woody paſtures, In winter they have nothing but cut food, conſiſting of two thirds hay and one third wheat ſtraw; and the quantity they uſe in 24 hours is about 24 pounds of hay and 12 of ſtraw; and on the days of reſt they range as they like in the ſtraw-yards; for it is to be obſerved, that they are not confined to hot ſtables, but have open ſheds, under which they eat their cut provender, and are generally left to their choice to go in and out. Under this management, as four oxen generally plough an acre a day, and do other work in proportion there can be no doubt but their advantage is very great over horſes, and the reſult to the public highly beneficial.

The oxen which are brought on in ſucceſſion, run the firſt ſummer in the park, and in the leaſows and temporary ſtraw yards in the winter, by which temporary ſtraw yards I would have it underſtood that they are made in different places, ſo that the manure which they make may be as near the ſpot where it is wanted as poſſible.

The forty oxen which go off are ſummered in the beſt paſture and finiſhed with turneps the enſuing winter. The uſual way has been to draw the turneps, and to give them either ſtalled or in cribs placed in the yard, with plenty of ſtraw to browſe and lie upon; but laſt winter an experiment was tried which anſwered extremely well, and will be again repeated next winter; this was penning the oxen by day on the turnep land in the manner which ſheep are penned. with this only difference, that the turneps were thrown up into cribs inſtead of being left to be trodden into the ground; and in the nights they were driven into a yard with a temporary ſhed, well littered with ruſhes, fern, and leaves, and turneps, and barley-ſtraw given to them in cribs. They thrived very faſt, and every one of them made at leaſt eight loads of good muck in the night-yard, beſides the benefit done in treading and dunging on the land in the day time, which was very great, the ſoil being very light.—The reſult of the ox ſyſtem is, that charging the ox for his agiſtment the firſt year, for the value of the graſs and turneps the laſt year, and putting what he has in three intermediate years as an equivalent for his labour after every allowance for riſk, each ox will pay at leaſt 20 per cent. profit. In what inſtance does a horſe produce ſo much?

I do not allow that the ox can be uſed on all ſoils; upon a very ſtony ſoil he cannot; nor can the horſe in all places be wholly excluded from huſbandry; but every occupier of a large farm may at leaſt uſe ſome oxen to very great advantage. They are all worked at Windſor in collars, as their ſtep is found to be much more free than when coupled together with yokes; and they are found to do their work with much greater eaſe in collars than in yokes, which ought every where to be exploded.

The

The different kinds of oxen are in fome meafure fuited to the foil.—Upon the Norfo farm, which is a light foil, the Devonfhire fort are ufed; upon the Flemifh farm, where the foil is ftrong and heavy, the Hertfordfhire; and in the Park, where the bufinefs is carting, harrowing, and rolling, the Glamorganfhire.—They are all excellent in their different ftations.

It may not be improper to mention a very fimple method which has been difcovered of firft training them to the collar, which is nothing more than putting a broad ftrap round their necks, and faftening one end of a cord to it, and the other to a large log of wood, and letting the ox draw it about as he feeds in his pafture for three or four days before he is put into harnefs, by which means he is very much brought forward in docility.

I have before obferved that 20 per cent may be confidered as the average profit of an ox, ftating them to be bought in at 10l. and allowing them to fell for 25l. taking off 10l. for the two years they are not worked: but laft year beans being of very little value, they were kept longer than ufual by being ftall-fed with bean meal, which anfwered very well as they were brought to an average of nearly 30l. and one of them, a Glamorganfhire ox, originally bought for 8l. and from his compact, round make, always called the little ox, thrived to fuch a furprizing degree that he became too fat to be able to travel to Smithfield, and was therefore fold to Mr. Charlwood, a neighbouring butcher, for 47l.

(To be continued.)

SCIENTIFIC NEWS AND ACCOUNTS OF BOOKS.

ARTIFICIAL PYRITES.

Extract of a Letter from a Correfpondent.

IF you think it worth while you may mention in your Journal an experiment I lately made, of endeavouring to procure Pyrites in a moift way. It is about two years fince I firft made the experiment, and only repeated it lately, to convince fome of my friends, who expreffed their doubts. The experiment is this.

I impregnated water very ftrongly with carbonic acid, and introducing fome iron filings, I continued the impregnation for a day or two, and afterwards allowed the water to ftand in a well corked bottle for fome days, till the acid had taken up as much iron as poffible. I then poured it into an aerating apparatus—threw up the hepatic gas from fulphuret of polafh and fulphuric acid; and after having agitated the water till it had got a good dofe of the gas, I poured the water into a large bafon;—this was in the evening, and next morning when I looked at it I found it covered with a pretty thick film of a moft beautiful variegated pyrites.—I had fo little of it, that the only proof I had of its being this fubftance was, that it was ignited on its being placed on a hot poker.

This

This may, perhaps, explain the beautiful variegations we obferve on fome mineral fprings and ftagnant pools.

Wooden Bridge acrofs the Rhine, at Schaffhaufen.

Mr. Taylor, in Holborn, has publifhed a large print of this curious product of the mechanical ability of *Grubenman*, the architect. The print contains plans of the floor and the roof, with an elevation and fection of the ftructure, fome other detailed parts and a perfpective view in aqua-tinta. Inftead of attempting to defcribe, in mere words, this combination of timber work, which formed two extended low arches, fpanning in the whole 364 feet, I fhall, for the entertainment of the reader, give a fhort hiftorical extract from the pamphlet which accompanies the engraving, firft premifing that this ufeful monument of original genius fell a facrifice to the fury of war, in April 1799, when it was burned by the French, after having ftood upwards of forty years.

"The village of Tueffen boafts the honor of having given birth to an excellent mechanical genius. The wooden bridges of *Ulric Grubenman* are very generally known on the Continent; that which is thrown acrofs the Rhine, near Schaffhaufen, is a fabric contemplated with aftonifhment by every traveller; and it is extolled in all modern works which treat of Switzerland as one of the firft of the curiofities which deferve to be vifited in that country. Indeed the boldnefs and beautiful fimplicity, as well as the apparent fymmetry and intrinfic ftrength of the wooden bridges conftructed by *Grubenman* cannot be fufficiently admired. Confifting of *one* arch they ftretch and bend as if fufpended by huge cables; they rock and tremble even under the feet of the paffenger; and when loaded waggons pafs over them, the fhaking of the bridge increafes to fuch an alarming violence that thofe who are unacquainted with the principle of its conftruction dread every moment that it will give way and plunge them in the waves.

"This fort of bridge aptly ftyled *hanging work* was firft brought to perfection by *Ulric Grubenman*. All the wooden and ftone bridges which had been laid acrofs the Rhine, near to and at the expence of the City of Schaffhaufen, being wafhed away by the impetuofity of that river, it became neceffary in the year 1754, to erect a new one; when among the architects who delivered in their plans, *Grubenman*, then a common carpenter, of Tueffen, prefented himfelf with a propofal for building a bridge, which refting on no pillars in the bed of the Rhine, fhould be fupported only by the rivers oppofite banks. On producing his model for the firft time to the committee appointed to examine the plans that might be offered, he was afked with a fcornful fmile, whether he really thought that a bridge built on the propofed principles would not break down as foon as any confiderable burdens were brought in contact with it? Inftead of making any anfwer, he, with both his feet, ftepped on his little model, which bore him (though a tall and ftout man) exceeding well. Impreffed with this circumftance, the

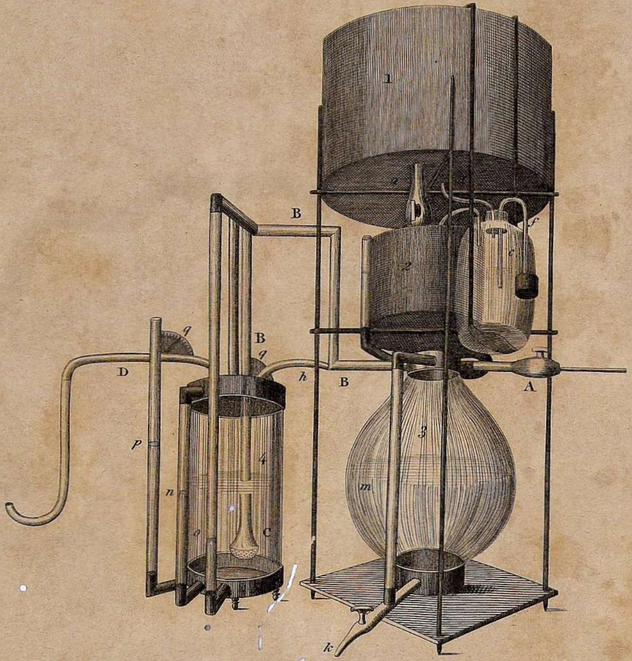
committee

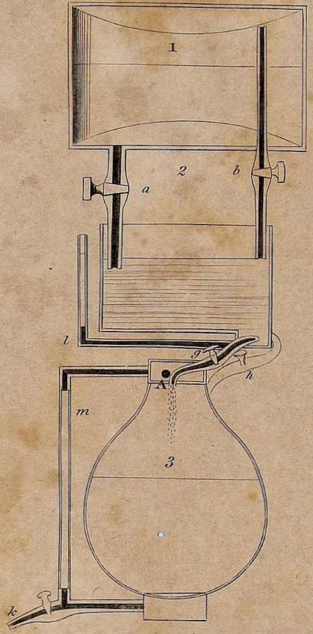
committee more attentively confidered the model of his bridge. It was compleated at the clofe of the year 1758, and ftood without receiving any injury till 1789, when a few decayed beams were replaced by new wood; fince which trifling repair the bridge is a found as ever. The fame artift offered to build a fimilar one-arched bridge acrofs the river Derry, in Ireland, which is 600 feet wide; but this plan was rejected."

A new Syftem of Mineralogy, in the form of a Catalogue, after the manner of Baron Born's Syftematic Catalogue of the Collection of Foffils of Mademoifelle Eleonore de Raab. By William Babington, M. D. Affiftant Phyfician and Lecturer in Chemiftry, at Guy's Hofpital. Quarto, 279 pages, and Index, price 15s. in boards. Sold by Phillips and Robinfons.

My Suffrage will be unneceffary either to add to the reputation of Dr. Babington, or to fupport the truth that the public muft be ever moft effentially benefited; when men of actual refearch and obfervation compofe elementary treatifes, which are more commonly fabricated by mere compilation. The work refers to the Collection of Sir John St. Aubyn, Bart. to whom it is dedicated, and the author has availed himfelf of the beft authorities for thofe experimental refults, of which the life of no fingle individual is fufficient for the inveftigation or even the repetition. His obligations to Baron Born are expreffed in the title page. Rome de Lifle has been his guide on the fubject of cryftallization; moft of his defcriptions correfpond with thofe of Widenmann and Emmerling, and he has chiefly recurred to Kirwan for objects of chemical properties and analyfis. His claffes, orders, genera, and fpecies, are founded on the chemical and his varieties on the external characters of minerals.

Gazometer of Seguin.

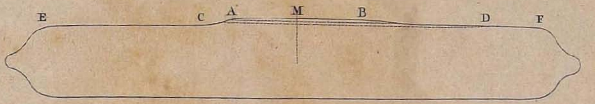




Mutlow sculp.

Method of grinding Spirit Levels.

Fig. 1.



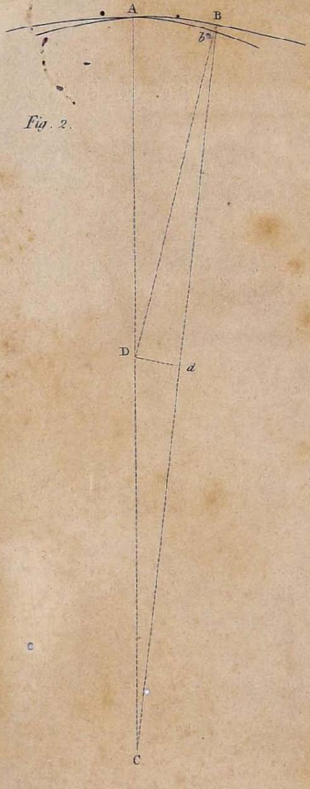


Fig. 2.

Fig. 3.

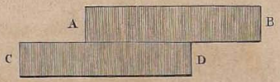
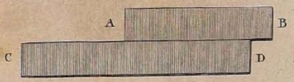


Fig. 4.



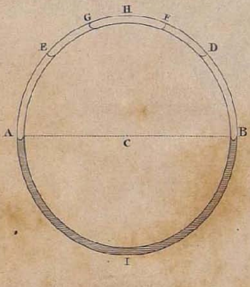


Fig. 5.

Mitlow sculp.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JANUARY 1800.

ARTICLE I.

Account of the improvements made on the Farm in the great Park of his Majesty, the King, at Windsor. *By* NATHANIEL KENT, *Esq.*

(Concluded from our last.)

NEXT to the advantage obtained from oxen, as much benefit as possible has been endeavoured to be derived from sheep by means of the fold. Two ewe flocks are kept, of four hundred each; the soil being light and dry, admits of winter folding (except when the weather is wet) upon the young clover;—a practice much to be recommended, as it produces a great crop of clover, and prepares the land the ensuing autumn for a crop of wheat without any further assistance. Another excellent practice is folding upon light land, in dry weather, immediately upon the sowing of the wheat, or kept back a fortnight or three weeks on that account; and it is not amiss to have the fold rather large, and to give the sheep a turn or two round the fold in a morning before they are let out to tread and settle the land, which does a great deal of good over and above their dung.

A third method of folding has been found to answer almost beyond description. This was first tried in the winter of 1793; but from an idea of the shepherd, that it injured the sheep, has been since disused: but as there is good reason to believe that there was no just ground for such an opinion, it is meant to be revived next winter.

A dry sheltered spot is selected, and sods of maiden earth, a foot deep, are laid over the space of a very large fold. It is then bedded thinly with rushes, leaves of trees, fern, moss, short straw, or stubble; and in hard or wet weather, the flock, instead of being penned upon the clover in the open fields, is put into the warmer fold, where the usual quantity of hay is given to them in racks; and every night they are so penned, the fold is fresh littered. When this has been continued, at intervals, during the winter, a layer of lime, chalk, rubble, or ashes, six inches thick, is spread over the whole surface—and when it has heated together, about the month of April, the whole is turned up, and mixed together, and makes the very best manure that can be used for turneps.

I have been particular in describing these methods of folding, as they are not common in any place, and in others entirely unknown, and to Gentlemen who have parks and large plantations which afford abundance of leaves, this hint may be the more deserving attention.

Upon the Norfolk farm, the land not having been yet marled or clayed, the clover is apt sometimes to fail, which is also the case elsewhere, upon the same sort of land. When this happens, his Majesty does what every other person in a similar situation should do; instead of letting the ground remain unproductive, the next year it is sowed with vetches, which are nearly as valuable as the clover, and wheat always grows remarkably kind after them.

As to implements, the Norfolk plough is chiefly what is used; and upon a light soil, it is certainly preferable to any other. It ploughs a cleaner furrow, by completely moving the whole body of earth, and inverts it much better than any other plough; and to establish its superiority over the common ploughs of the neighbourhood, I need only add, that from its construction it is nearly the draught of an ox easier. There is likewise a Norfolk harrow, very useful for harrowing what are called brush-turneps, or any other turneps, preparatory to their being hoed. I must be allowed, likewise, to mention the drill-roller which consists of cast-iron rings, made at the Norwich foundery, and slipt on upon a round piece of wood, as an axle-tree. This is one of the best things that has been introduced, for the preparation of the land for any sort of corn, where the soil will admit of its being used. By the corn being so well deposited, it takes better root, and at least one fourth of the quantity usually sown may be saved.

The Flemish farm, which I have before mentioned, was so named from an intention, at first, of carrying on a system of husbandry similar to that practised in Flanders, which consists of an alternate crop for man and beast; but the soil being strong and cohesive, upon trial, it has been found to answer best under a four-course shift, more like some parts of Gloucestershire; as thus—first year, wheat; second, cabbage or clover; third, oats; fourth, beans. The quantity of arable land of this farm is one and sixty acres, or forty acres in a shift. There are two things observed upon this farm, which

may

may be worth notice:—The first is the practice which has for these two years past been adopted, by taking off the tops of the beans just as the blossom is set; this not only improves the quality, but increases the quantity, and causes them to ripen sooner, which is a considerable advantage, by giving time to get the succeeding crop of wheat in, perhaps, a fortnight earlier. The other is, that of sowing clover early in the spring, among twenty acres or one half of the wheat, and bush-harrowing and rolling it in. This has produced a very fair crop of clover the next year; and the other half, after the wheat, is winter and spring fallowed, and planted with cabbage. There is a double advantage resulting from this; that one half of this shift, so managed, becomes a summer crop, and the other half a winter crop; and by observing the next year to change the parts, by sowing the clover where the cabbage was before, the clover and cabbage do not come round upon the same ground but once in eight years.

Cabbage has been tried several years, but his Majesty's husbandmen never got into the right management of it till this year; but now the crop is remarkably fine.

It will not be improper to mention, that the drum-headed cabbage is the best sort; that the seed should be sown in August, the plants first set out in November, and transplanted for good in July. The next thing to be noted is their application:—They are certainly inferior to turneps for fatting, but superior in the increase of milk, either of cows or ewes, and therefore they are particularly good where there is a dairy or breeding flock of sheep: and I trust his Majesty will, the next yeaning season, try an experiment, of which I have high expectation, which is to slice or quarter the cabbage, and feed the ewes with them upon such of the meadows as want manuring, which I flatter myself will be of inestimable service to the ewes and lambs, and be the means of increasing the next year's crop of hay considerably.

The true light of viewing these improvements is to consider them as a sort of new creation to the public; for, as it is a fact not to be controverted, that the reduced number of acres in the park, from their improved state, support as many deer and other cattle as the whole did before, the produce obtained from the farms is all clear gain; and as crops of wheat and rye from the 140 acres sown, upon the most moderate calculation, may be set at 3,360 bushels, and allowing six bushels to a human mouth, this gives a yearly provision in bread for 560 people; to say nothing of the fatting-off of forty oxen, the bread of 800 sheep, and the growth of at least 5000 bushels of oats and beans; all of which, it must be observed, goes in aid of the public market, as the work is done by oxen entirely.

As more experiments are in future made, I may perhaps trouble the Society with an account of them, as I am persuaded they cannot be registered any where else, to give them the credit, and to excite the imitation I flatter myself they may deserve: but for the present, I shall close my observations upon his Majesty's farms, with a description of

his mill, which I conſider as the moſt benevolent thing that can be done for the poor, and which I moſt earneſtly recommend to all Gentlemen of landed property, who have like means of doing it. A ſmall over-ſhot mill is erected, and worked by the waſte water from the lake below the Lodge, where a ſufficiency of corn, two thirds wheat, and one third rye, is ground, dreſſed, and given to all the labourers, at ſixteen-pence per ſtone of fourteen pounds, in quantities ſuitable to the ſize of their families, which is the firſt of all comforts to them, and a ſaving of at leaſt twenty per cent. from what it would coſt them to buy it from the mealmen or ſhopkeepers.

I am, SIR,

Your obedient humble ſervant,

NATHANIEL KENT.

Craig's Court,
30th October, 1798.

II.

Extract of a Letter from WILLIAM PETRIE, *Eſq. on Board the Good-Hope, India-man, at Sea, lat.* 35. 40. *S. long.* 44. *E. giving an Account of a ſingular Accident by Lightning. Communicated by* ROB. PETRIE, *M. D.*

SATURDAY, the 13th July, about midnight. a flaſh of lightning of a globular form, came in contact with the fore part of the ſhip, and inſtantly exploding, occaſioned a report, compared by thoſe who heard it, to that of a cannon hard charged with double-headed ſhot; differing much from the ſound of ordinary thunder, which had been frequent in the courſe of the evening. This might have been owing to the nearneſs of the exploſion, the ſound neither vibrating much nor meeting with any body to revibrate it, before reaching the ear, as happens when ſuch exploſions take place high up in the atmoſphere.

At the moment of exploſion ſeveral people on the main deck felt ſenſibly ſhocked in different parts of the body, and William Stanhope, a ſoldier in the 29th L. Dragoons, was inſtantly killed. Thomas Steelman, a ſailor, ſitting wrapped up in a ſail and leaning to the foremaſt, was found in a ſtate of inſenſibility, and ſeemingly lifeleſs. He was immediately carried below, and as the body paſſed me, I was ſenſibly affected with the ſmell of burnt horn or ſinged wool, which, however, was compared by others to the ſmell of burning ſulphur. From the time of his being ſtruck until ſymptoms of returning life made their appearance, at leaſt ſix or eight minutes muſt have elapſed. Being at that moment engaged in fruitleſs attempts to recover the ſoldier, I had not an opportunity of aſcertaining whether the motion of the heart and arteries was entirely interrupted,

nor

nor was this circumſtance attended to by the ſurgeon, but from the general appearances it is extremely probable that both the circulation and reſpiration were completely ſuſpended. In recovering from this ſtate he gave a loud ſcream, accompanied with conſiderable agitation of the body, but no convulſive-like motion. His eyes had a very wild appearance, and the whole countenance was remarkably expreſſive of terror. He foamed at the mouth and made frequent attempts to articulate, but was unable. He ſeemed quite inſenſible to what was paſſing, and continued ſcreeching at ſhort intervals for nearly two hours. Upon examining the body, the whole inſide of both thighs was found ſtripped of the cuticle, as if the part had been bliſtered and the falſe ſkin removed, and a few fingers of each hand were in the ſame manner affected. In the courſe of the night his intellects returned, and ſuffering little pain from the ſuperficial burning, he paſſed the next and following day with little inconvenience, and in ſeeming good health, but could not call to mind any of the circumſtances that had taken place. On the 16th, he complained to the aſſiſtant ſurgeon of a ſlight ſenſe of ſoreneſs in the ſkin of the right ſide of the head, with a want of feeling in one particular ſpot. Upon examining the part no ſore could be diſcovered, but a portion of the ſkin, about the ſize of a dollar, over the right parietal bone felt depreſſed, as if beaten in, and quite inſenſible to the touch. Some ſpiritous application was made uſe of, and next day the part appearing of a darker colour than natural, the hair was removed. The ſame application was continued, but on the 18th, the ſpot had become quite black, felt ſoft, and appeared in a ſtate of gangrene. On the 19th, the gangrene ſeeming to extend, the ſurgeon made a few ſcarifications round the edges, which bringing on inflammation put a ſtop to its further progreſs. In two days the mortified parts ſloughed off, and left a portion of the ſcull fully the ſize of a dollar, including a part of the right parietal and frontal bones with the coronal ſuture running through its middle, entirely bare and denuded of the pericranium. The wound has continued to diſcharge freely of purulent matter, and the edges granulate favourably. The bone, however, has been gradually becoming of a darker colour, and is now (the 7th of Aug,) entirely black in one part. The diſcolouration commenced at the ſuture, and will probably extend as far as the mortification in the ſkin; ſo that before a cure can be accompliſhed an extenſive exfoliation muſt take place. It will be fortunate if the exfoliation does not penetrate deeper than the outer table of the ſcull, as his life will be in imminent danger ſhould ſo large a ſurface of brain be expoſed from ſuch a cauſe.

What appears ſingular in the caſe of this man, is the ſpot on the head running to mortification without any evident exciting cauſe, and without any previous inflammation. It could not have ariſen from either a blow or fall, as he was found exactly in the poſition which he had, previous to the lightning. It was no doubt an effect of the lightning, but how it ſhould have confined its influence to ſo ſmall an extent, and act

in

in a manner fo differently from what it had done in other parts of the body, are circumftances for which it is difficult to account. The extenfive excoriation on the thighs and hands was evidently burning, produced by the electric matter paffing over thofe parts, but no fuch appearance could be traced on the head. The canvas cap he had on was found entire, neither was the hair fo much as finged; fo that the part could not be faid to be burnt. Indeed the whole appearances are againft fuch a conclufion, for had this been the cafe, fome degree of inflammation would have been obfervable previous to gangrene. The only attempt towards an explanation that I have to offer on the fubject, is to refer it to a known law of the effects produced by very powerful ftimulants on the living animal body: which when applied in too great quantity exhauft it inftantly of its vital or irritable principle. It is in this manner that electricity or lightning acts when death is produced; and in a like manner it is probable that the moft powerful poifons, fuch as the Ticunas and Upaz gum produce their effects. So that what happened in the prefent cafe may be confidered as *death* in one fmall fpot, occafioned by the immediate and total extinction of the irritable principle, from an over *dofe* of electric matter, (the moft powerful and moft diffufible of all ftimuli,) and neceffarily running to mortification as a *piece* of dead animal matter. Its partial action is the difficulty for which I pretend not to account.

The foldier had exactly the appearance of people defcribed to have been killed by lightning. The body retained its heat a very confiderable time after death, the mufcles never became rigid, and appearances of putrefcency very foon prefented themfelves.

The local fituation of thofe two men at the time the lightning came in contact with the fhip, neceffarily expofed them more than others. The foldier was leaning againft the water engine, which has a good deal of iron in its conftruction, and being fentry had a bayonet attached to him. The failor, as already mentioned, was leaning againft the foremaft, which being wet by the previous rain was juft in a ftate ready to conduct electric matter, and confequently rendered him more obnoxious to its influence than had he been detached at fome diftance.

Five or fix pigs, in a ftye, near the head of the fhip, were killed, while others in the fame fituation, but feparated by canvas, entirely efcaped. One pig on the gun-deck was killed, and five or fix had palfy produced in their hind quarters.

Aug. 14, 1798. Examined the wound on the head of the failor, this morning, and found a portion of the bone, nearly the fize of the mortified fpot, quite loofe and likely to feperate entirely in a few days. This exfoliation extends to the outer table only.

It fhould have been obferved that no mark of external violence was obfervable on any part of the body of the foldier.

A

III.

A Botanical Defcription of Urceola Elaftica, or Caout-chouc Vine of Sumatra and Pullo Pinang; with an account of the properties of its infpiffated Juice, compared with thofe of the American Caout-chouc. By WILLIAM ROXBURGH, M. D.*

FOR the difcovery of this ufeful vine, we are, I believe, indebted to Mr. *Howifon*, late Surgeon at Pullo-pinang; but it would appear he had no opportunity of determining its botanical character. To Doctor *Charles Campbell* of Fort Marlborough, we owe the gratification arifing from a knowledge thereof.

About twelve months ago I received from that gentleman, by means of Mr. *Fleming*, very complete fpecimens, in full foliage, flower, and fruit. From thefe I was enabled to reduce it to its clafs and order in the Linnæan Syftem. It forms a new genus in the clafs Pentandria, and order Monogynia, and comes in immediately after Tabernæmontana, confequently belongs to the thirtieth natural order, or clafs called Contortæ by *Linnæus* in his natural method of claffification or arrangement. One of the qualities of the plants of this order is, their yielding, on being cut, a juice which is generally milky, and for the moft part deemed of a poifonous nature.

The generic name, Urceola, which I have given to this plant, is from the ftructure of the corol, and the fpecific name from the quality of its thickened juice. So far as I can find, it does not appear that ever this vine has been taken notice of by any European till now. I have carefully looked over the Hortus Malabaricus, *Rumphius's* Herbarium Amboinenfe, &c. &c. Figures of Indian plants, without being able to find any one that can with any degree of certainty be referred to. A fubftance of the fame nature, and probably the very fame, was difcovered in the ifland of Mauritius, by M. *Poivre*, and from thence fent to France; but, fo far as I know, we are ftill ignorant of the plant that yields it.

The impropriety of giving to Caout-chouc the term gum, refin, or gum-refin, every one feems fenfible of, as it poffeffes qualities totally different from all fuch fubftances as are ufually arranged under thofe generic names: yet it ftill continues, by moft authors I have met with, to be denominated elaftic refin, or elaftic gum. Some term it fimply Caout-chouc, which I wifh may be confidered as the generic name of all fuch concrete vegetable juices (mentioned in this memoir) as poffefs elafticity, inflammability, and are foluble in the effential oils, without the affiftance of heat.

In a mere definition, it would be improper to ftate what qualities the object does not poffefs;

* Afiatic Refeasches, vol. v. 167.

poſſeſs ; conſequently it muſt be underſtood that this ſubſtance is not ſoluble in the menſtruums which uſually diſſolve reſins and gums.

Eaſt India Caout-chouc would be a very proper ſpecific name for that of Urceola elaſtica, were there not other trees which yield juices ſo ſimilar, as to come under the ſame generic character ; but as this is really the caſe, I will apply the name of the tree which yields it for a ſpecific one. E. G. Caout-chouc of Urceola elaſtica, Caout-chouc of Ficus Indica, Caout-chouc of Artocarpus integrifolia, &c. &c.

Deſcription of the Plant Urceola. Pentandria Monogynia.

Gen. Char. calyx beneath five-toothed ; corol one petaled, pitcher ſhaped, with its contracted mouth five-toothed : nectary entire, ſurrounding the germs ; follicles two, round, drupacious ; ſeeds numerous, immerſed in pulp.

Urceola Elaſtica.

Shrubby, twining, leaves oppoſite, oblong, panicles terminal, is a native of Sumatra, Pullo-pinang, &c. Malay countries.

Stem, woody, climbing over trees, &c. to a very great extent, young ſhoots twining, and a little hairy, bark of the old woody parts thick, dark coloured, conſiderably uneven, a little ſcabrous, on which I found ſeveral ſpecies of moſs, particularly large patches of lichen ; the wood is white, light, and porous.

Leaves, oppoſite, ſhort-petioled, horizontal, ovate, oblong, pointed, entire, a little ſcabrous, with a few ſcattered white hairs on the under ſide.

Stipules, none.

Panicles, terminal, brachiate, very ramus.

Flowers, numerous, minute, of a dull, greeniſh colour, and hairy on the outſide.

Bracts, lanceolate, one at each diviſion and ſubdiviſion of the panicle.

Calyx, perianth, one-leaved, five-toothed, permanent.

Corol, one petaled, pitcher ſhaped, hairy, mouth much contracted, five-toothed, diviſions erect, acute, nectary entire, cylindrick, embracing the lower two-thirds of the germs.

Stamens, filaments five, very ſhort, from the baſe of the corol. Anthers arrow ſhaped, converging, bearing their pollen in two grooves on the inſide, near the apex ; between theſe grooves and the inſertions of the filaments they are covered with white ſoft hairs.

Piſtil, germs two ; above the nectary they are very hairy round the margins of their truncated tops. Style ſingle, ſhorter than the ſtamens. Stigma ovate, with a circular band, dividing it into two portions of different colours.

Per. Follicles two, round, laterally compreſſed into the ſhape of a turnip, wrinkled, leathery, about three inches in their greateſt diameters—one celled, two valved.

Seeds,

Seeds, very numerous, reniform, immerſed in firm fleſhy pulp.

Explanation of the Figures.

1. A branchlet in flower, natural ſize.
2. A flower magnified.
3. The ſame laid open, which expoſes to view the ſituation of the ſtamens inſerted into the bottom of the corol, the nectarium ſurrounding the lower half of the two germs, their upper half with hairy margins, the ſtyle and ovate party-coloured; ſtigma appearing above the nectary.
4. Outſide of one of the ſtamens } much magnified.
5. Inſide of the ſame }
6. The nectarium laid open, expoſing to view the whole of the piſtil.
7. The two ſeed veſſels (called by *Linnæus* follicles), natural ſize; half of one of them is removed, to ſhew the ſeed immerſed in pulp. A portion thereof is alſo cut away, which more clearly ſhews the ſituation and ſhape of the ſeed.

From wounds made in the bark of this plant there oozes a milky fluid, which, on expoſure to the open air, ſeparates into an elaſtic coagulum, and watery liquid, apparently of no uſe, after the ſeparation takes place. This coagulum is not only like the American caout-chouc or Indian rubber, but poſſeſſes the ſame properties, as will be ſeen from the following experiments and obſervations made on ſome which had been extracted from the vine about five months ago. A ball of it now before me, is to my ſenſe, totally void of ſmell, even when cut into, is very firm, nearly ſpherical, meaſures nine and a half inches in circumference, and weighs ſeven ounces and a quarter, its colour on the outſide is that of American caout-chouc, where freſh cut into of a light brown colour till the action of the air darken it; throughout there are numerous ſmall cells, filled with a portion of light brown watery liquid above mentioned. This ball, in ſimply falling from a height of fifteen feet, rebounds about ten or twelve times, the firſt is from five to ſeven feet high, the ſucceeding ones of courſe leſſening by gradation.

This ſubſtance is not now ſoluble in the above mentioned liquid contained in its cells, although ſo intimately blended therewith when firſt drawn from the plant, as to render it ſo thin, as to be readily applied to the various purpoſes to which it is ſo well adapted when in a fluid ſtate.

From what has been ſaid, it will be evident that this caout-chouc, poſſeſſes a conſiderable ſhare of ſolidity, and elaſticity in an eminent degree. I compared the laſt quality, with that of American caout-chouc by taking ſmall ſlips of each, and extending them till they broke; that of Urceola was found capable of bearing a much greater degree of extenſion, (and contraction) than the American: however, this may be owing to the time the reſpective ſubſtances have been drawn from their plants.

 The

The Urceola caout-chouc, rubs out the marks of a black lead pencil, as readily as the American, and is evidently the fubftance of which the Chinefe make their elaftic rings.

It contains much combuftible matter, burning entirely away, with a clear flame, emitting a confiderable deal of dark-coloured fmoke which readily condenfes into a large proportion of exceeding fine foot, or lamp-black; at the fame time it gives but little fmell, and that not difagreeable; the combuftion is often fo rapid, as to caufe drops of a black liquid, very like tar, to fall from the burning mafs; this is equally inflammable with the reft, and continues when cold in its femi-fluid ftate, but totally void of elafticity; in America the caout-chouc is ufed for torches, ours appear to be equally fit for that purpofe. Expofed in a filver fpoon to a heat, about equal to that which melts lead or tin, it is reduced into a thick, black, inflammable liquid, fuch as drops from it during combuftion, and is equally deprived of its elaftic powers, confequently rendered unfit for thofe purpofes, for which its original elafticity rendered it fo proper.

It is infoluble in fpirits of wine, nor has water any more effect on it, except when affifted by heat, and then it is only foftened by it.

Sulphuric acid reduced it into a black, brittle, charcoal like fubftance, beginning at the furface of the caout-chouc, and if the pieces are not very thin, or fmall, it requires fome days to penetrate to their centre; during the procefs, the acid is rendered very dark coloured, almoft black. If the fulphuric acid is previoufly diluted, with only an equal quantity of water, it does not then appear to have any effect on this fubftance, nor is the colour of the liquid changed thereby.

Nitric acid reduced it in twelve hours to a foft, yellow, unelaftic mafs, while the acid is rendered yellow; at the end of two days, the caout-chouc had acquired fome degree of friability and hardnefs. The fame experiment made on American caout-chouc was attended with fimilar effects. Muriatic acid had no effect on it.

Sulphuric æther only foftened it, and rendered the different minute portions it was cut into eafily united, and without any feeming diminution of elafticity.

Nitric æther I did not find a better menftruum than the vitriolic, confequently, if the æther I employed was pure, of which I have fome doubt, this fubftance muft differ effentially from that of America, which *Berniard* reports to be foluble in nitric æther.

Where this fubftance can be had in a fluid ftate, there is no neceffity for diffolving or foftening it, to render it applicable to the various ufes for which it may be required; but where the dry caout-chouc is only procurable, fulphuric æther promifes to be an ufeful medium, by which it may be rendered fo foft as to be readily formed into a variety of fhapes.

Like American caout-chouc, it is foluble in the effential oil of turpentine, and I find

it

it equally fo in Cajeput oil, an effential oil, faid to be obtained from the leaves of Melaleuca Leucadendron. Both folutions appear perfect, thick, and very glutinous. Spirits of wine, added to the folution in Cajeput oil, foon united with the oil, and left the caout-chouc floating on the mixture in a foft femi-fluid ftate, which, on being wafhed in the fame liquor, and expofed to the air, became as firm as before it was diffolved, and retained its elaftic powers perfectly, while in the intermediate ftates between femi-fluid and firm, it could be drawn out into long, tranfparent threads, refembling, in the polifh of their furface, the fibres of the tendons of animals; when they broke, the elafticity was fo great, that each end inftantaneoufly returned to its refpective mafs, Through all thefe ftages the leaft preffure with the finger and thumb united different portions, as perfectly as if they never had been feparated, and without any clamminefs, or fticking to the fingers, which renders moft of the folutions of caout-chouc, fo very unfit for the purpofes for which they are required. A piece of catgut covered with the half infpiffated folution, and rolled between two fmooth furfaces, foon acquired a polifh, and confiftence very proper for bougies. Cajeput oil, I alfo found a good menftruum for American caout-chouc, which was as readily feperated by the addition of a little fpirit of wine, or rum, as the other, and appears equally fit for ufe, as I covered a piece of catgut with the wafhed folution, as perfectly as with that of Urceola. The only difference I could obferve, was a little more adhefivenefs from its not drying fo quickly; the oil of turpentine had greater attraction for the caout-chouc than for the fpirits of wine, confequently remained obftinately united to the former, which prevented its being brought into that ftate of firmnefs fit for handling, which it acquired when Cajeput oil was the menftruum.

The Cajeput folution employed as a varnifh did not dry, but remained moift and clammy, whereas the turpentine folution dried pretty faft.

Expreffed oil of olives and linfeed proved imperfect menftruums while cold, as the caout-chouc, in feveral days, was only rendered foft, and the oils vifcid, but with a degree of heat equal to that which melts tin, continued for about twenty-five minutes it was perfectly diffolved, but the folution remained thin and void of elafticity. I alfo found it foluble in wax, and in butter in the fame degree of heat, but ftill thefe folutions were without elafticity, or any appearance of being ufeful.

I fhall now conclude what I have to offer on the caout-chouc, or Urceola elaftica, with obferving that fome philofophers of eminence have entertained doubts of the American caout-chouc being a fimple vegetable fubftance, and fufpect it to be an artificial production, an idea which I hope the above detailed experiments will help to eradicate, and confequently to reftore the hiftories of that fubftance by M. *De la Condamine* and others, to that degree of credit to which they feem juftly entitled, in fupport of which it may be further obferved, that befides Urceola elaftica there are many other trees,

 natives

natives of the Torrid Zone, that yield a milky juice, poffeffing qualities nearly of the fame nature, as artocarpus integrifolia (common jack tree) ficus religiofus et Indica, Hippomane biglandulofa, Cecropia peltata, &c.

The caout-chouc or ficus religiofa, the Hindus confider the moft tenacious vegetable juice they are acquainted with; from it their beft bird lime is prepared. I have examined its qualities as well as thofe of ficus Indica and artocarpus integrifolia, by experiments, fimilar to thofe above related, and found them triflingly elaftic when compared with the American and Urceola caout-choucs, but infinitely more vifcid than either; they are alfo inflammable, though in a lefs degree, and fhew nearly the fame phenomena, when immerfed in the mineral acids, folution of cauftic alkali, alkohol, fat, and effential oils; but the folution in Cajeput oil could not be feparated by fpirits of wine and collected again like the folutions of the Urceola and American caout-chouc.

IV.

On the different Sorts of Lime used in Agriculture. By SMITHSON TENNANT, *Efq. F. R. S.**

I WAS informed laft fummer that in the neighbourhood of Doncafter two kinds of lime were employed in agriculture, which were fuppofed to differ materially in their effects. One of thefe, which was procured near the town, it was neceffary to ufe fparingly, and to fpread very evenly over the land; for it was faid that a large proportion of it, inftead of increafing, diminifhed the fertility of the foil; and that wherever a heap of it was left in one fpot all vegetation was prevented for many years. The other fort of lime which was obtained from a village near Ferry Bridge, though confiderably dearer from the diftant carriage, was more frequently employed on account of its fuperior utility. A large quantity was never found to be injurious; and the fpots which were entirely covered with it, inftead of being rendered barren, became remarkably fertile. The different properties afcribed to thefe two kinds of lime were fo very diftinct that it feemed probable they could not be imaginary; and it therefore appeared to be worth the trouble of afcertaining them more fully, and of attempting to difcover the nature of the ingredients from whence the difference arofe. For this purpofe I procured fome pieces of each fort of lime-ftone, and firft tried what would be their effect upon vegetables in their natural ftate, by reducing them to coarfe powder, and fowing in them the feeds of different plants. In both kinds the feeds grew equally well, and nearly in the fame manner as they would in fand or any other fubftance which

* Philof. Tranfactions, 1799. Part II. p. 305.

affords

affords no nourishment to vegetables. Pieces of each sort of stone were then burnt to lime; and after they had been exposed some weeks to the air, that their causticity might be diminished, some seeds were sown in them. In the kind of lime which was found most beneficial to land, almost all the seeds came up and continued to grow as long as they were supplied with water, and the roots of the plants had many fibres which had penetrated to the bottom of the cup in which they grew. Upon examining the composition of this sort of lime, it proved to consist entirely of calcareous earth. By its exposure to the air for about three months it was found to have absorbed four fifths of the fixed air required to saturate it. In the other kind a few only of the seeds grew, and the plants produced from them had hardly any stalks or roots, being formed almost entirely of the two seed leaves which lay quite loose upon the surface. This sort of lime being spread upon a garden soil, to the thickness of about a tenth of an inch, prevented nearly all the seeds which had been sown from growing up, whilst no injury was occasioned by common lime used in the same manner. Upon examining the composition of this substance, which was so destructive to the plants, it was discovered to contain three parts of pure calcareous earth, and two of magnesia. The quantity of fixed air which it had absorbed by being exposed for about the same time as the pure lime just mentioned, was only 42 hundredths of that combined with it before it was burnt.

As it seemed probable that the magnesia contained in this lime was the cause of its peculiar properties, the following experiments were made to determine the effects of that substance upon the growth of vegetables. Some seeds, chiefly of colewort, which were preferred from their growing quickly, were sown in uncalcined magnesia; but though they sprouted, the leaves never rose above the surface, and the plants were entirely without roots, nor did they appear to grow better in magnesia which had been washed in water containing fixed air. Calcined magnesia was, however, much more destructive, as the seeds would not come up in it. To compare its effects on vegetables with those of lime, each of these earths were mixed in different proportions with sand in small cups, in which seeds were then sown. The lime was obtained from marble; and before it was put into the sand, was made to fall to powder by being moistened with water. In a mixture of four ounces of sand with three or four grains of calcined magnesia, it was a long time before the seeds came up, and the plants had hardly any roots or stalks; and ten grains or more of magnesia, there was no appearance of vegetation. Thirty or forty grains of lime did not retard the growth of the seeds more than three or four of magnesia, and the injurious effects were not so lasting. The lime, by absorbing fixed air, soon lost its destructive properties; so that after keeping these mixtures four or five weeks seeds were found to grow in that with forty grains of lime nearly as well as in the pure sand; but in that with four grains of magnesia they produced only the seed leaves, as was described before. It was necessary, occasionally, to break in pieces the sand which had so

much

much lime, as it would otherwife have been too hard to admit the feeds to penetrate through it. Plants will bear a much larger proportion of magnefia in vegetable foil than in fand; with twenty grains, however, of calcined magnefia in as much foil as was equal in bulk to four ounces of fand, the feeds produced only the feed leaves without any roots; and with about forty grains they were entirely prevented from coming up.

The countries where the magnefia lime is employed, it was faid that the barrennefs of any fpot on which a heap of it had been laid would continue for many years. To learn how far it could by time be deprived of its injurious qualities, I procured fome pieces of mortar made of this fpecies of lime from two houfes, one of which had been built three and the other eight years: they were taken from the outfide of the building where they had been expofed to the air. After they were reduced to powder, feeds were fown in them; only a few came up, and even thofe produced merely the feed leaves without any roots. As plants would grow in the lime-ftone from which this fpecies of lime was formed, although not in the mortar made from it, I wifhed to know what proportion of the fixed air originally contained in the lime-ftone had been abforbed by the mortar. For this purpofe a piece of it was finely powdered to render it of an uniform quality; it was then tried how much of this powder and of the lime-ftone would faturate the fame quantity of acid; by this means I afcertained the proportions of lime-ftone and mortar containing equal quantities of the magnefian lime. The fixed air being obtained from them in thofe proportions and meafured in an inverted veffel with quickfilver, it was found that the mortar which had been expofed three years had abforbed 43, and that of eight years only 47 hundredths of the quantity originally contained in the lime-ftone. I was not able to obtain any mortar which had been made earlier, though it deferve to be known how much fixed air it was ultimately capable of abforbing. Common mortar which had been expofed to the air for a year and three quarters, had regained 63 hundredths of its full quantity of fixed air.

As the preceding experiments were tried during the winter, in a room warmed by fire, perhaps under circumftances more favourable to vegetation, the fame quantity of magnefia would not be equally pernicious.

Magnefian lime-ftone may be eafily diftinguifhed from that which is purely calcareous by the flownefs of its folution in acids, which is fo confiderable, that even the fofteft kind of the former is much longer in diffolving than marble. From this property of the magnefian lime-ftone there appeared to be reafon for fufpecting that the kind of marble which had been called Dolomite, from M. *Dolomieu*, who firft remarked its peculiarity in diffolving flowly, might alfo be fimilar in its compofition. An analyfis of this fubftance was lately given in the *Journal de Phyfique*, but this is probably erroneous; for, upon examining three fpecimens, they were found to confift of magnefia and calcareous earth, like the magnefian lime-ftone; fo that it ought, no doubt, to be con-

confidered as the fame fpecies of ftone, but in a ftate of greater purity. The pieces of dolomite were from different places, one of them being found among the ruins of Rome, where it is thought to have come from Greece, as many ftatues of Grecian workmanfhip are made of it, and no quarries of a fimilar kind are known in Italy; the fecond was faid to have been thrown up by mount Vefuvius; and the third was from Tona, one of the weftern iflands of Scotland. In many kinds of common marble fmall particles of veins may be obferved, which are a long time in diffolving. Thefe, upon examination, I difcovered to contain a confiderable portion of magnefia; but as they were probably not quite free from the furrounding marble, I did not afcertain the quantity precifely.

The chryftallized ftructure which may generally be obferved in the magnefian lime-ftone feems to fhew that is has not been formed by accidental union of the two earths, but muft have refulted from their chemical combination. The difficulty of diffolving it may alfo arife from the attraction of the different component parts to each other. The mortar found from this kind of lime is as foluble in acids as common marble, and the fubftances of which it confifts are eafily feparated. The magnefia may be taken from it by boiling it in muriated lime, and lime is precipitated from it by lime water; but neither of thefe effects can be moderated by the ftone before it is calcined.

Magnefian lime-ftone is probably very abundant in various parts of England. It appears to extend for thirty or forty miles from a little fouth-weft of Workfop, in Nottinghamfhire, to near Ferry Bridge, in Yorkfhire. About five or fix miles further north there is a quarry of it near Sherburn; but whether this is a continuation of the ftratum near Ferry Bridge I have not learnt. From fome fpecimens which were fent me I find that the cathedral and molls of York are made of it. I have not been able to learn whether there were any fhells in the lime-ftone of the tract of the country before mentioned. In Mr. *Marfhall*'s account of the agriculture of the Midland counties, he fpeaks of the lime made at Breedon, near Derby, as deftructive to vegetables when ufed in large quantities. I therefore procured fome pieces of it, and they were difcovered to contain nearly the fame proportion of magnefia as that before defcribed. In this quarry the ftone is frequently cryftallized in a rhomboidal form, and petrefied fhells, not calcareous, but fimilar in compofition to the ftone itfelf, are fometimes but very rarely found in it. This fubftance feems to be common in Northumberland. In the third vol. of the Annals of Agriculture Dr. *Fenwick*, of Newcaftle, obferves that the farmers of that country divide limes into hot and mild. The former of thefe is no doubt magnefian, as it has fimilar effects on the foil, and he remarks that it is not fo eafily diffolved in acids as the latter. At Matlock, in Derbyfhire, the two kinds are contiguous to each other; the walks on the fide of the river where the houfes are built being magnefian, and on the other calcareous. The magnefian rock appears alfo to be incumbent

incumbent upon a calcareous ſtratum; for in deſcending a cave formed in this rock, a diſtinct vein of common lime-ſtone may be obſerved which contains no magneſia. The latter ſtratum is very full of ſhells; but though there is ſome alſo in the magneſian rock, yet they are very rare. In the following tables, containing the analyſis of various ſpecimens, ſome other places are mentioned where this ſubſtance is found, but of which I received no further information.

Although it was known that the magneſian marble and lime-ſtone conſiſted of two earths, their proportion was attempted to be diſcovered by trying how much gypſum and epſom ſalt could be obtained by means of vitriolic acid from a certain weight of each ſpecimen. When the ſuperfluous vitriolic acid had been evaporated by heat, the Epſom ſalt was ſeparated from the Gypſum by water. The reſult of theſe trials is expreſſed in the following table.

	Dry Gypſum.	Dry Epſom Salt.
5 grains of lime-ſtone from Breedon gave	3,9	3,15
Matlock	3,95	2,9
Workſop	3,8	3,0
York	3,8	3,1
3 grs. of calcareous ſpar and 1 gr. of calcined magneſia gave	3,9	2,7

As the preceding method of eſtimating the quantities of magneſia and calcareous earth is liable to conſiderable error, I afterwards examined them in the following manner, which ſeems capable of great exactneſs. Twenty-five grains of each ſubſtance were diſſolved by marine acid in a cup of platina, and after the ſolution was evaporated to dryneſs, it was made red hot for a few minutes. The maſs remaining in the cup which conſiſted of muriated lime, and of the magneſia freed from the acid, was waſhed out with water and poured into a phial. There was then added to it a known quantity of diluted marine acid, ſomewhat more than was ſufficient to re-diſſolve the magneſia, and after the ſolution a certain weight of calcareous ſpar, part of which would be diſſolved by the ſuperfluous acid. By the quantity of that remaining undiſſolved it was learnt how much acid was required to diſſolve the magneſia. The iron and argillaceous earth, contained in ſome ſpecimens, were precipitated by the ſpar, and therefore could not occaſion any error. The calcareous ſpar, however, diſſolved more ſlowly where there was argillaceous earth, as it became coated with it; but this incruſtation was occaſionally removed, and in all the experiments the ſpar was left in the ſolution till it ſuffered no further diminution. For this purpoſe it was neceſſary to keep them ſlightly warm for ſome days, during which time the phials were generally cloſed to prevent any eſcape of the acid. The firſt experiment in the following table was made upon known quantities of magneſia and calcareous earth to try the accuracy of the proceſs. For

this

this purpose also the second was repeated upon a piece of lime-stone, previously pow-dered to render every part of it the same quality. The first column shews the quantity of calcareous spar which might have been dissolved by the acid required to take up the magnesia. The second shews the correspending quantites of magnesia in 35 grs. of each substance. The third expresses the quantity of lime. This was inferred by subtracting the weight of the magnesia and of the iron and clay from 13,2 grs. the weight of the whole quantity of earth in 25 grs. of lime-stone. This is probably not very incorrect, as in two specimens which differed most in the proportion of magnesia and lime; the weight of the two earths was nearly the same.

A piece of dolomite from Rome, was wrapped in a thin piece of platina, that no part of it might be lost, and being then exposed to a strong heat left of earth—52,9 per Cent.

Dolomite from Mount Vesuvius . .	52,8
Breedon lime-stone . . .	52,4
Calcareous spar left of lime . .	55,8

In three of the experiments also the calcareous earth was precipitated by mineral alkali; and the quantity of it being tried by that of the marine acid required to dissolve it, it corresponded very nearly with that put down.

A quantity of marine acid which would dissolve 15 grs. of calcareous spar, would also dissolve 5,5 of calcined magnesia, and 2,5 grs. of spar; so that 12,5 grs. of spar required the same quantity of acid as 5,5 grs. of magnesia.

The magnesia used was pure, and made red hot immediately before it was weighed.

Substances examined.	*Quantity of Spar which the Acid required to take up the Magnesia would have dissolved.*	*Quantity of Magnesia.*	*Quantity of Lime.*	*Iron and Clay.*
Mixture of 5,5. grs. of magnesia, and 14 grs. of calcareous spar	12,5	5,5	7,8	0
35 grs. of Breedon lime-stone previously powdered . .	11,53	5,071	7.929	.2
25 grs. from part of the same powder	11,56	5,082	7,913	.2
25 grs. of dolomite from Rome	12,2	5,37	7,73	.1
——— dolomite from Iona	10,1	4,4	7,8	1.0 { insoluble substance

 Vesuvian

Substances examined.	*Quantity of Spar which the Acid required to take up the Magnesia would have dissolved.*	*Quantity of Magnesia.*	*Quantity of Lime.*	*Iron and Clay.*
—— Vesuvian dolomite	10,38	5,565	8,575	.06
Of second exp. from part of the same Vesuvian dolomite	10,03	4,411	8,849	.06
25 grs. of magnesian limestone from Wansworth, near Doncaster . . .	12,75	5,61	7,34	·25
—— Thorpe arch . . .	10,95	4,84	7,8	.6
—— Matlock	12,5	5,5	7,388	·31
—— York Minster . . .	11,	4,84	8,26	.1
—— Worksop	11,6	5,104	7,496	.6
—— Sherburn	11,5	5,08	7,56	·56
—— Westminster-Hall . .	10,1	4,44	8,37	·4

V.

*Account of the strange Effects produced by Respiration of the Gaseou Oxide of Azote.**

THE reader may recollect an extract of a letter from Mr. *Davy*, at p. 93, of our present volume, mentioning the discovery which constitutes the object of the present extract. In the pamphlet of Dr. *Beddoes*, mentioned below, we have a detail of some of the remarkable phenomena there alluded to; but are still uninstructed with regard to the method of repeating them with safety and effect. On this head, however, the Doctor assures us that, the communication of the full experience of the operators will be made without reserve or unnecessary delay in a periodical work, to be entitled *Researches concerning Nature and Man*; the first number of which will appear in less than three months, and will contain a paper on the Philosophy of Medicine, by Dr. B.; part of a vast chemical investigation connected with the gas so often mentioned,

* From Dr. Beddoes's *Notice of some Observations made at the Medical Pneumatic Institution.* Bristol printed 1799.

and

and including its history by Mr. *Davy*; an account of the cases in which it shall have been used; of an account of our experience (viz, of the Medical Pneumatic Institution) in pthisical cases, by Dr. *Kinglake*, together with some communications.

The narrative of effects, in the words of the author, is as follows, p. 6.

" Certain circumstances belonging to the gas, denominated by its great discoverer, Dr. *Priestley*, dephlogisticated nitrous gas, engaged Mr. *Davy's* attention. After making some experiments, which proved that its composition, properties, and mode of action had been mistaken by the latest experimenters, he was induced to respire it in small quantities mingled with common air. In these first trials he thought, without fixing his opinion, that it acted as a depressing power; and I had communicated his suspicion to Mr. *Watt.* Dissatisfied with the result, however, he at last ventured to breathe it pure. The first inspirations of the gas produced giddiness, fulness of the head, and in short, feelings resembling those of incipient intoxication, but unaccompanied by pleasurable sensation. At the next experiment I was present. The quantity was larger, and the gas more pure. The scene exhibited was the most extraordinary I had ever witnessed, except in the case of that epileptic patient, whom I have described (*Considerations on airs*, part iv. p. 13) as agitated, in consequence of the respiration of oxygen gas, with a long succession of the most violent movements. The two spectacles differed, indeed, essentially in one respect. In the former ever thing was alarming; in the latter, after the first moments of surprize, it was impossible not to recognize the expressions of the most extatic pleasure. I find it entirely out of my power to paint the appearances, such as they exhibited themselves to me. I saw and heard shouting, leaping, running, and other gestures, which may be supposed to be exhibited by a person who gives full loose to feelings, excited by a piece of joyful and unlooked for news. As in the case of the epileptic patient, *no weariness or depression followed*: so in this case, *no exhaustion or langour or uneasy feeling took place.* The experiment Mr. *Davy* has very frequently repeated, and generally with the highest pleasurable sensations, and except under particular circumstances, with considerable muscular exertion, which have not in any instance been succeeded by fatigue or sadness.

" Since that time, a number of persons have inhaled the same gas. The following is an abstract of the reports furnished by themselves. The inaccuracies (should any occur) will soon be checked by the full account. Imperfections both accounts will have, for it is impossible for the combined endeavours of the spectator and the subject of experiment adequately to represent what was sometimes seen and felt.

" The individuals mentioned below might be classed in various ways. Many had previous apprehension. Some had never heard of the expected effect. Others disbelieved it. A distinction ought also to be made between those who respired before we had learned to prepare the air with certainty, and those who have respired it since.

 From

From many hundreds of experiments, we have alfo now acquired an idea of the dofe fuitable to different temperaments. But there was a time, when for want of fuch knowledge, the refults were lefs agreeable than might have been wifhed. All thefe circumftances require attention. At prefent, however, without attempting either ftrict arrangement or analyfis, I fhall meerly endeavour to ftate the leading circumftances as briefly as perfpicuity will allow.

"Mr. *J. W. Tobin* (after the firft imperfect trials), when the air was pure, experienced fometimes fublime emotions with tranquil geftures, fometimes violent mufcular action, with fenfations indefcribably exquifite; no fubfequent debility—no exhauftion. —His trials have been very numerous. Of late he has felt only fedate pleafure. In Mr. *Davy* the effect is not diminifhed.

"*Patrick Dwyer* has always exhibited a ludicrous ftruggle between a propenfity to laugh, undoubtedly produced by the air, and an eager defire to continue the inhalation.

"Rev. *Richemond Barbauld* felt exhilarated, and was compelled to laugh, not by any ludicrous idea, but by an impulfe unconnected with thought, and fimilar to that which is felt by children full of health and fpirits—laffitude and languor through the day afterwards, which Mr. B. is difpofed to attribute to hot oppreffive weather, and a preceding fleeplefs night.

"Mrs. *Barbauld—the Children's Friend.* At firft pleafurable fenfations, occafioning involuntary laughter; fome momentary faintnefs afterwards.—We now underftand the regulation of the dofe, fo as perhaps to be able to remove Mr. *Barbauld's* languor, and to give Mrs. *Barbauld* the pleafure without the tranfitory faintnefs.

"Mr. *George Burnet* had never heard of the effect of the air, after inhalation broke out into the moft rapturous exclamations I ever witneffed, breathed at two o'clock P. M. and had all day a moft delightful flow of fpirits.

"Mrs. *Beddoes*---pretty uniform pleafurable fenfations---propenfity to mufcular exertion, could walk much better up Clifton Hill---has frequently feemed to be afcending like a balloon, a feeling which Mr. *Burnet* ftrongly expreffed.

"Mr. *James Thompfon.* Involuntary laughter, thrilling in his toes and fingers, exquifite fenfations of pleafure. A pain in the back and knees occafioned by fatigue the day before, recurred a few minutes afterwards. A fimilar obfervation, we think, we have made on others; and we impute it to the undoubted power of the gas to increafe the fenfibility, or nervous power, beyond any other agent, and probably in a peculiar manner.

"Mr. *Thomas Pople*---at firft unpleafant feelings of tenfion; afterwards agreeable luxurious languor, with fufpenfion of mufcular power---laftly increafed powers of body and mind---vivid and highly pleafurable fenfations. In a fecond experiment, all the faculties abforbed in fine pleafing feelings of exiftence.

"Mr.

" Mr. *Stephen Hammick*, Surgeon of the Royal Hofpital, Plymouth. In a fmall dofe, yawning and languor.---It fhould be obferved, that the firft fenfation has often been difagreeable, as giddinefs; and a few perfons, previoufly apprehenfive, have left off inhaling as foon as they felt this. Two larger dofes produced a glow, unreftrainable tendency to mufcular action, high fpirits and more vivid ideas.

" A bag of common air was firft given to Mr. *Hammick*, and he obferved that it produced no effect. The fame precaution againft the delufions of imagination was of courfe frequently taken.

" Mr. *William Clayfield* has moft refifted the effects of the gas. Pretty ftrong dofes produced a tranfitory intoxication. In two inftances, very large dofes have excited the violent mufcular orgafm, accompanied with exquifite pleafure, and followed by no debility.

" Mr. *Robert Southey* could not diftinguifh between the firft effects, and an apprehenfion, of which he was unable to diveft himfelf. His firft definite fenfations were, a fullnefs, and dizzinefs in the head, fuch as to induce fear of falling. This was fucceeded by a laugh, which was involuntary, but highly pleafurable, accompanied with a peculiar thrilling in the extremities—a fenfation perfectly new and delightful. For many hours after this experiment, he imagined that his tafte and fmell were more acute, and is certain that he felt unufually ftrong and chearful. In a fecond experiment, he felt pleafure ftill fuperior, and has fince poetically remarked, that he fuppofes the atmofphere of the higheft of all poffible heavens to be compofed of this gas.

" ——— *Wilmot, M. D.* Involuntary laughing, with unufual mufcular motions, but no particularly pleafant or unpleafant feeling, heat in the cheft, heat and perfpiration in the feet. On a fecond inhalation the fenfations were pleafurable.

" *Robert Kinglake, M. D.* Additional freedom and power of refpiration, fucceeded by an almoft delirious, but highly pleafurably fenfation in the head, which became univerfal, with increafed tone of the mufcles. At laft, an intoxicating placidity abforbed for five minutes all voluntary power, and left a cheerfulnefs and alacrity for feveral hours.

" A fecond ftronger dofe produced a perfect *trance* for about a minute; then a glow pervaded the fyftem. The permanent effects were, an invigorated feeling of vital power, and improved fpirits.

" By both trials, particularly by the former, old rheumatic feelings feemed to be revived for the moment.

" Mr. *Notcutt*, formerly lecturer on chemiftry at Hackney, was twice thrown into an extatic pleafurable trance; the firft time his fpirits were better for the day; after the fecond, he was languid, but is inclined to impute this to exercife in oppreffively hot weather. Perhaps, however, the fecond dofe was too ftrong for his conftitution.

" Mr.

" Mr. *Wedgwood* breathed atmofpheric air firft without knowing it was fo. He declared it to have no effect, which confirmed him in his difbelief of the power of the gas. After breathing this fome time, however, he threw the bag from him, kept breathing on laborioufly with an open mouth, holding his nofe with his left hand* without power to take it away, though aware of the ludicroufnefs of his fituation, all his mufcles feemed to be thrown into vibratory motion, he had a violent inclination to make antic geftures—feemed lighter than the atmofphere, and as if about to mount. Before the experiment, he was a good deal fatigued after a very long ride, of which he permanently loft all fenfe. In a fecond experiment nearly the fame effects, but with lefs pleafure. In a third, much greater pleafure.

" Mr. *Jofiah Wedgwood*, and Mr. *Thomas Wedgwood* experienced rather unpleafant feelings, of fome duration in the latter. But we doubt of the quality of the air breathed by both, as they took it (except Mr. *T. Wedgwood* once) at an early period of the inveftigation; and in this laft experiment it had an effect rather agreeable.

Mr. *Lovell Edgeworth*, at firft felt tremor, vertigo, dizzinefs of fight, which by degrees fubfided, next a ftrong propenfity to bite the mouth-piece; after finifhing the air, eagerly wifhed for more, then was inclined to laugh, and did burft into a moft violent fit of laugher; and laftly, capered about the room without having power to reftrain himfelf.

" Mifs *Morgan* found her feelings flightly pleafant from a fmall dofe, but fucceeded by giddinefs. A larger dofe raifed the pleafurable fenfations very high, and mufcular power feemed increafed; but upon the whole, the feelings were nearer foothing calmnefs than vivid exhilaration. In another trial with a large dofe, more lively and lafting pleafure followed, the mufcular power feemed unbounded, though on attempting to walk there was the intoxication. After each of thefe trials, a flight difficulty of breathing was felt within doors, but in the open air, the fpirits were very greatly exhilarated. In another trial, fenfation became at each infpiration gradually more indiftinct, and recollection was loft for a few moments, the feelings not being unpleafant. Spirits afterwards rather depreffed than raifed. In all the latter trials the head feemed full, and vifion indiftinct, during inhalation.

" The Author of this notice, notwithftanding the freedom with which he had formerly inhaled oxygen gas, for fome time waved the trial of the other. His apoplectic make, joined to the frequent occurrence of a degree of giddinefs in others, rendered him timid. The perfect fafety, however, with which he faw the two paralytic patients below mentioned, and alfo a third, who is juft beginning a courfe of air, perform the experiment, overcame his fcruples. For if the firft unpleafant impreffion, which has

* This was practifed in all the experiments.

been

been occaſionally obſerved, had the ſlighteſt connection with the ſymptoms of apoplexy, or palſy, he ſuppoſed that either theſe patients, or ſome other perſon, would have been injured by the inhalation. He has now taken it daily for ſome time, in a manner that will be hereafter deſcribed. The firſt ſenſations had nothing unpleaſant; the ſucceeding have been agreeable beyond his conception or belief, even after the rapturous deſcriptions he had heard, and the eagerneſs to repeat the inhalation which he had ſo often witneſſed. He ſeems to himſelf, at the time, (for why ſhould one fear to uſe ludicrous terms when they are expreſſive?) to be bathed all over with a bucket full of good humour; and a placid feeling pervades his whole frame. The heat of the cheſt is much greater from a ſmall doſe than he ever felt from the largeſt quantity of oxygen. A conſtant fine glow, which affects the ſtomach, led him one day to take an inconvenient portion of food, and to try the air afterwards. It very ſoon removed the ſenſe of diſtention, and, he ſuppoſes, expedited digeſtion. He has never tried to bring on the high orgaſm; but has generally felt more alacrity at the moment—not one languid, low, *crapulary* feeling afterwards. It occurred to him that under a certain adminiſtration of this gas, ſleep might poſſibly be diſpenſed with; he is ſure that from leſs ſleep he derives more refreſhment than for many years paſt. And his morning alertneſs equals that of a healthy boy.

" During the printing of this paper, the author took a large doſe before an excellent judge of the phenomena of intoxication, who on obſerving him attentively for ſome time, exclaimed, " *why your eyes twinkle as if you were drunk. You are certainly drunk.* The obſervation was accurate. Intoxication, as indicated by unſteadineſs and ſtammering at the time, and a random feeling for ſome hours after, was produced. It ſubſided into ſimple high ſpirits, and no langour followed. Till he took this air, going to a play always brought on a head-ach next morning. Now he riſes juſt as freſh as on other occaſions.

" Upon the whole, he believes that the Pneumatic Inſtitution might advance a fair claim to the premium, anciently propoſed for the diſcovery of a new pleaſure; and he ventures to ſay that the firſt ſlight unpleaſant ſenſations may be obviated by due management, and the gas exhibited with ſafety even to hyſterical females."

After theſe accounts the Author proceeds to relate the caſe of a young lady ſubject to hyſteric fits, and a little to cramps, who, on the ſuggeſtion of a friend, tried the gas without any previous mention of her nervous affections. She was immediately thrown into a ſucceſſion of hyſterical fits of conſiderable violence and of above an hours duration, which returned at the ſame time the following evening, but gave way to the ſubſequent exhibition of opium and the bark in large doſes. After about a week ſhe declined medicine and the fits returned with increaſed violence. Laughing, crying, ſtarting, painful acuteneſs, and dulneſs of hearing, inordinate motion of almoſt every muſcle,

muscle, with loss of power in each, expectation of immediate death successively occurred, and in a degree so alarming as to render it difficult to remove the apprehensions of the young lady's friends.

These fits appeared to have been certainly produced by the specific power of the gas; and other trials cautiously made by ladies who were acquainted with all the previous facts, and subject to hysteric affections, produced effects which tended to confirm the above inference.

I cannot attempt, with any hope of precision, to give an analysis of the other parts of the Doctor's pamphlet, and still less can I think of making any remarks on a subject so new, so striking, and at the same time so obscure. He seems to be of opinion that the blood may impart to the solids various compounds of oxygen, azote, &c. which are quite unknown to our chemistry;—that the nerves may require one kind of supply and the muscles another;—that the gas in question communicates the principle which gives energy to the nerves;—that of this principle the system of nervous patients possesses a morbid excess, but that the system of paralytic subjects is defective in that respect. From these and other reasonings it was expected to be beneficial in the latter disease, and the few cases yet tried are very strong in confirmation of this induction.

VI.

A Chemical Examination of the Bath Waters, by George Smith Gibbes, *M. D. F. R. S.*

(Concluded from page 404.)

A Great deal of attention seems necessary in ascertaining to any certainty the degree of heat of the Bath waters. So many causes occur to lower their temperature a few degrees, that their precise heat at any one part of the Bath will not be found to answer exactly with any other part. After pumping a considerable time at the King's Bath, the temperature is generally from 114 of *Fahrenheit* to 117, or even 118. The hot bath is nearly of the same temperature with the King's bath. The cross bath is of a somewhat lower temperature, from 110 to 112. As the pumps lead immediately from the springs, the temperature of these waters can be better ascertained from them than from the baths.

Twelve grains of nitrate of silver were added to six ounces of the King's bath waters, and seven grains of precipitate were left to the filtre. No precipitation was observed on the addition of more nitrate of silver to the filtered liquor. To four ounces of this liquor were added two drachms of a saturated solution of acetate of barytes, and twenty grains of precipitate remained on the filtre. There was no farther precipitation

tation of the addition of more acetate of barytes. Six grains of oxalic acid produced, when added to this filtered liquor, fifteen grains of precipitate. The folution of carbonate of ammonia produced a very flight precipitation in the remaining liquor.

The King's Bath waters, after they are cooled, appeared to be precifely of the fame weight as the water which is ufed in this city for ordinary purpofes. Although the Bath waters, when cold, appear to differ but little from common hard waters, yet at their fource they are accompanied with fome very powerful agents. Their high temperature gives to the iron they contain an extraordinary degree of activity. I have obferved in a great variety of cafes, that thefe waters produce very tonic effects, when large dofes of the different preparations of fteel have failed.

There is a prodigious quantity of æriform fluid continually evolved from thefe fprings, which rifes in large bubbles, and is diffipated in the furrounding atmofphere. Having collected a large quantity of this gas, I fubjected it to the following experiments. A meafure of this gas was added to a meafure of nitrous gas, and a very trifling abforption took place, I mixed a larger quantity of it, with nitrous gas, and a very trifling difcoloration took place. The fame quantity of atmofpheric air, and the fame nitrous gas, were united together, and very red fumes immediately appeared. Into a veffel full of this air I poured fome frefh lime water; on agitation the lime water became very turbid, and of a milk-white colour. A fmall portion of this air was abforbed by water. On mixing it with atmofpheric air, and expofing it to the flame of a taper, there was not the flighteft detonation. A burning body was inftantly extinguifhed when placed in this air.

From the foregoing experiments I am led to believe that the Bath waters contain fome very active principles. Befides their heat, which moft affuredly increafes the action of their other component parts, we find that they lower the purity of the air, by the quantity of azotic gas which is continually poured forth into the atmofphere over the baths. Large quantities of this air muft be infpired by thofe who ufe the open bath, and as we know that an alteration in the proportions of the component parts of atmofpheric air will produce evident effects on the human conftitution, this circumftance may, I think, with propriety be pointed out as a fource of medical inquiry. I have been informed by a very learned and fcientific perfon, that filiceous earth has been found to produce, when diffolved in water, fome very confiderable effects on the animal economy. As my experiments lead me to believe, that this earth forms a large proportion of the folid contents of thefe waters, as it appears to be very minutely divided, and as the high temperature may give it activity, I think this circumftance alfo may be regarded as worthy the attention of medical practitioners. I could mention a great variety of inftances where the Bath waters have produced great effects in difeafe; but as this is not intended as a medical communication, I muft content myfelf with obferving,

 that

that those who have hitherto considered these waters as inefficacious, will agree with me, I imagine, in allowing that these principles must have considerable medical properties.

It seems difficult to account for the azotic gas which rises with these waters. The country round Bath contains prodigious quantities of lime-stone, in the greatest part of which I have been able to detect the exuviæ of sea animals. As there must have been a great deposition of animal substances, and as these waters apply a continued and powerful heat to the lower strata of lime-stone, the azotic gas may thereby be detached, and brought to the surface with them. It may also happen that the atmospheric air may find a passage into the bowels of the earth, so that assisted by the great heat of these waters the oxygene may be combined with substances for which it has a great attraction, allowing the azotic gas to pass and appear by itself at the surface. Hence may arise the carbonic acid, which may be detached from the lime-stone by the acids formed by the combinations with the oxygene, producing thereby the sulphate of lime, &c. so easily discovered in the Bath waters.

VII.

Analysis of an Ore of Iron, the Composition of which has been hitherto misunderstood, by Mr. WILLIAM HENRY; *including a Letter on Ores of Iron, addressed to* Mr. THOMAS HENRY, F.R.S. *By* CHARLES HATCHETT, ESQ. F.R.S.

Manchester, Dec. 20, 1799.

ABOUT two years ago, a drysalter of this town requested me to examine chemically, a substance, of which he had purchased a large quantity, presuming it to be plumbago. Mere inspection, however, of its external characters, induced me to suppose that he had been deceived; and, after a careful analysis, my suspicion proved to be well-founded. I send you the following account of the mineral, and its composition; because it appears to have been, hitherto, not sufficiently understood, and even to have been misstated, by that excellent mineralogist, Mr. *Kirwan*.

The resemblance of this mineral to powdered plumbago is striking enough to mislead a hasty observer. It is in the state of a fine scaley powder, of a steel-grey colour, with somewhat of a reddish hue, which appears more evidently when it is finely spread on a white ground. Its lustre is metallic, and it possesses no transparency. When rubbed between the fingers, it has an unctuous feel; and adheres, but does not give a permanent stain to

the

the skin.* It does not, like plumbago, leave a shining trace on paper, but only a few glittering particles, which are removed by a sudden blow of the finger. If sprinkled lightly on the surface of a vessel of water, it subsides slowly; and a small part even remains suspended at last. Its specific gravity is but moderate; but difficult to determine precisely on account of its state of disintegration.

By calcination, in a low red heat, during an hour, it lost about one per cent, which was probably nothing but a little accidental moisture. It was not altered by being heated with free access of air. The dilute mineral acids, by long digestion, took up about ten per cent, which proved to be oxyd of iron. After this treatment, I thought its reddish hue evidently diminished, but its lustre and other external characters remained unaltered. When projected on red hot and melted nitre, not the smallest deflagration ensued; and, after washing off the salt, the mineral was recovered without diminution of weight or change of qualities.

Two hundred grains of the ore were next exposed to a strong red heat, during two hours, with 30 grains of charcoal. The residue weighed 152 grains; i. e. there had been a loss of 48 grains. Of this residue all but 17 grains were dissolved by dilute muriatic acid, with a copious discharge of hydrogeneus gas. In order to ascertain how much of the insoluble part was unconsumed charcoal, it was projected on melted nitre with which it deflagrated. The part that resisted this treatment being collected after the salt was washed off, weighed 11 grains. If, therefore, to 48 we add 6, (the amount of the unconsumed coal) we obtain the true loss of 200 grains of the ore, viz. 54, or 27 per cent. But of these 200 grains, 11 will be shewn not to be oxyd of iron. The loss of 54 grains was therefore sustained in reality by only 189 grains of oxyd of iron; and the truly oxydated part of the ore lost, as nearly as possible, at the rate of $28\frac{1}{2}$ per cent.

The iron dissolved by the muriatic acid, was next precipitated by carbonate of soda, and repeatedly heated to dryness with nitric acid. After this, it was digested with dilute nitric acid, which took up no portion of the oxyd. This shews that no other metal was present.

This insoluble residue of 11 grains, which had the form of a white and impalpable powder, was repeatedly boiled to dryness with strong sulphuric acid, and then washed with hot distilled water. By this treatment it lost $2\frac{1}{2}$ grains, which proved to be a luminous earth. The remaining $8\frac{1}{2}$ grains were found, by the result of their fusion with mineral alcali, to be siliceous earth.

A mixture of 200 grains of the ore with the proper fluxes and charcoal, exposed to a violent fire in a blast furnace, afforded me a button of metal, weighing 144 grains. The iron thus obtained has the specific gravity of 7,300; was somewhat, but not perfectly

malleable; and was judged by an eminent iron master, to whom I shewed it, to be intermediate between crude and good wrought iron.

From the foregoing experiments, the component parts of this ore, and their proportions, may be assigned as follows:

One hundred parts contain

Of oxygen,	$28\frac{1}{2}$
Iron, -	66
Alumine, -	$1\frac{1}{4}$
Silex, -	$4\frac{1}{4}$

From this analysis, we may infer, also, the proportion of foreign ingredients, in the metallic button obtained from the ore. For, since 200 grains of the ore contain only 132 of real metal, while the weight of the iron obtained was 144, it appears that the 144 parts of metal must contain 12 of foreign ingredients, or $8\frac{1}{3}$ per cent.

The general results of the foregoing analysis having been communicated by my father to Mr. *Hatchett*, along with a specimen of the ore, he was favored in reply with the following interesting remarks.

" The substance which Mr. *William Henry* has had the goodness to send me by your hands is, as he supposes, one of the species of the eisen-glimmer. The first, (called by the Germans eisenmann) is of a steel-grey colour and lustre. It does not feel unctuous, nor stain the fingers. The second (called eisonrahm) of which your specimen is a variety, is to be found described in the following authors.

Brann und Rother Eisenrahm
Ferrum Ochraceum Rubrum Inquinans
Mine de Fer Micacé Rougeatre
} Widenmann's Handbuch der Mineralogie, p. 807

Hæmatites Micaceus, Wallerii, spec. 333; also in Mus, Lesk. of Kärsten, tom. 2. p. 453; Werner's Verzeichnis, 1 Band. p. 153; Catalogue de Raab, tom. 2. p. 265-269; and Kirwan, 1st edit. p. 275. The last mentioned author says, that it is combined with plumbago; but this appears to be a mere conjecture, without foundation. In his 2d. edit. vol. 2, p. 166 and 172, he considers the eisenmann and eisenrahm among those ores of iron, which consist of iron, combined with from 24 to 36 per cent. of oxygen. Although I am not acquainted with any regular analysis of this substance, yet, from what is known at present, there is every reason to believe that your son is in the right.

" The various writers, on the systematical arrangement of minerals, have separated the ores of iron, like those of other metals, according to their different ideas; and appear, in general, too much to have overlooked the chain, by which nature connects her products. This chain is peculiarly observable in the mineral kingdom, and is most evident in the natural metallic compounds called ores. The grey iron ores, for instance, may be regarded as differing from each other, only in the proportion of oxygen, by which

they

they are more or less removed from the metallic state. The compass of this letter will not allow me to enumerate them; and I shall only therefore observe, that the eisenmann appears to me to differ from the eisenrahm, merely by a less degree of oxydation; and that the varieties of each are so many gradations, by which the first passes into the second. For example, the eisenmann, in the complete state, has the lustre and colour of steel; is not unctuous to the touch, and when scraped or pulverized, affords a grey powder. But other varieties yield a greyish red, or even a red powder; and, at the same time certain degrees of unctuosity are to be observed. These I consider as gradations of eisenmann into eisenrahm. Again, some varieties of the latter possess the steel-grey colour and lustre, and, in the like proportion, become more and more unctuous, and of a fuller red; till, at length, they resemble an impalpable powder of the red hæmatites. This sort may be observed like an efflorescence on the surface of the hæmatites, found at Ulverstone, and other places on the borders of Lancashire. And, as gradations, similar to those between the eisenmann and eisenrahm, are to be observed in the varieties of hæmatites, I am inclined to embrace the opinion which Wallerius seems to have formed, when he applies the name of hæmatites micaceus to the eisenmann and eisenrahm: For I strongly believe, that a great part of the varieties of hæmatites have been derived from the matter of these by a stalactitical operation with the advantitious accession of some argill and silex.

"The oxydation of the eisenrahm is even carried so far as to form transparent lamellar crystals, about $\frac{1}{16}$ of an inch broad, exactly like mica, of a beautiful deep garnet colour. This variety, I believe, has only been found in Dauphiné; and the only specimen in England I purchased at M. de Calonne's sale. In respect to your son's specimen, it seems to be a variety of eisenrahm, in which some of the characters of eisenmann are blended. A similar kind I have had from Luxillian in Cornwall; and when I was at Exeter, I saw great quantities of the eisenmann for sale; but the place where found was kept a secret. Soon afterwards, however, I had occasion to visit some tin mines on Dart Moor; and in one called Vytifor, I discovered the eisenmann forming thin veins between the main lode, or vein of tin ore, and the walls; and this was the mine from which Exeter had been supplied."

VIII.

An Account of ſome Experiments on the Fecundation of Vegetables. By THOMAS ANDREW KNIGHT, *Eſq.**

THE reſult of ſome experiments which I have amuſed myſelf in making on Plants, appearing to me to be intereſting to the naturaliſts by proving the exiſtence of ſuperfœtation in the vegetable world, and being likely to conduce to ſome improvements in Agriculture I have taken the liberty to communicate them to you.

The breeders of animals have very long entertained an opinion that conſiderable advantages are obtained by breeding from males and females not related to each other. Though this opinion has lately been controverted, the numbers of its oppoſers are gradually diminiſhed; and I can ſpeak from my own obſervation and experience that animals degenerate in ſize at leaſt on the ſame paſture and in other reſpects under the ſame management when this proceſs of croſſing the breed is neglected.

The cloſe analogy between the animal and vegetable world and the ſexual ſyſtem equally pervading both, induced me to ſuppoſe that ſimilar means might be productive of ſimilar effects in each; and the event, has, I think, fully juſtified this opinion. The principal object I had in view was, to obtain new and improved varieties of the apple, to ſupply the place of thoſe which have become diſeaſed and unproductive by having been cultivated beyond the period which nature appears to have aſſigned to their exiſtence. But as I foreſaw that ſeveral years muſt elapſe before the ſucceſs or failure of the proceſs could poſſibly be aſcertained, I wiſhed in the interval to ſee what would be its effects on annual plants. Among theſe none appeared ſo well calculated to anſwer my purpoſe as the common pea; not only becauſe I could obtain many varieties of this plant of different forms, ſizes, and colours; but alſo becauſe the ſtructure of its bloſſom by preventing the ingreſs of inſects and adventitious farina has rendered its varieties remarkably permanent. I had a kind growing in my garden which having been long cultivated in the ſame ſoil had ceaſed to be productive, and did not appear to recover the whole of its former vigor when removed to a ſoil of a ſomewhat different quality. On this, my firſt experiment in 1787, was made. Having opened a dozen of its immature bloſſoms, I deſtroyed the male parts, taking great care not to injure the female ones; and a few days afterwards, when the bloſſoms appeared mature, I introduced the farina of a very large and luxuriant grey pea into one half of the bloſſoms, leaving the other half as they were. The pods of each grew equally well; but I ſoon perceived

* Philoſ. Tranſactions, 1799. Part II. p. 195.

that

that thoſe into whoſe bloſſoms the farina had not been introduced, the ſeed remained nearly as they were before the bloſſom expanded, and in that ſtate they withered. Thoſe in the other pods attained maturity, but were not in any ſenſible degree different from thoſe afforded by other plants of the ſame variety; owing, I imagine, to the external covering of the ſeed (as I have found in other plants) being furniſhed entirely by the female. In the ſucceeding ſpring the difference, however, became extremely obvious; for the plants from them aroſe with exceſſive luxuriance and the colour of their leaves and ſtems clearly indicated that they had all exchanged their whiteneſs for the colour of the male parent; the ſeeds produced in autumn were dark grey. By introducing the farina of another white variety (or in ſome inſtances by ſimple culture) I found this colour was eaſily diſcharged and a numerous variety of new kinds produced, many of which were in ſize and every other reſpect much ſuperior to the original white kind, and grew with exceſſive luxuriance, ſome of them attaining the height of more than twelve feet. I had frequent occaſion to obſerve in this plant a ſtronger tendency to produce purple bloſſoms; and coloured ſeeds than white ones; for when I introduced the farina of a purple bloſſom into a white one, the whole of the ſeeds in the ſucceeding year became coloured; but when I endeavoured to diſcharge this colour by reverſing the proceſs, a part only of them afforded plants with white bloſſoms; this part ſometimes occupying one end of the pod, and being at other times irregularly intermixed with thoſe, which, when ſown, retained their colour. It may, perhaps, be ſuppoſed that ſomething might depend on the quantity of farina employed; but I never could diſcover in this or any other experiment in which ſuperfœtation did not take place, that the largeſt or ſmalleſt quantity of farina afforded any difference in the effect produced.

The diſſimilarity I obſerved in the offspring afforded by different kinds of farina in theſe experiments, pointed out to me an eaſy method of aſcertaining whether ſuperfœtation (the exiſtence of which has been admitted among animals) could alſo take place in the vegetable world. For as the offspring of a white pea is always white, unleſs the farina of a coloured kind be introduced into the bloſſom, and as the colour of the grey one is always transferred to its offspring though the female be white, it readily occurred to me, that if the farina of both were mingled or applied at the ſame moment the offſpring of each could be eaſily diſtinguiſhed.

My firſt experiment was not altogether ſucceſsful; for the offspring of five pods (the whole which eſcaped the birds) received their colour from the coloured male. There was, however, a ſtrong reſemblance to the other male in the growth and character of more than one of the plants; and the ſeeds of ſeveral in the autumn very cloſely reſembled it in every thing but colour. In this experiment I uſed the farina of a white pea, which poſſeſſed the remarkable property of ſhrivelling exceſſively when ripe; and in the ſecond year I obtained white ſeeds from the grey ones above mentioned, perfectly ſimilar

similar to it. I am strongly disposed to believe that the seeds were here of common parentage; but I do not conceive myself to be in possession of facts sufficient to enable me to speak with decision on this question.

If, however, the female afford the first organized atom, and the farina act only as a stimulus, it appears to me by no means impossible that the explosion of two vesicles of farina at the same moment (taken from different plants) may afford seeds (as I have supposed) of common parentage; and as I am unable to discover any source of inaccuracy in this experiment, I must believe this to have happened.

Another species of superfœtation (if I have justly applied that term to a process in which one seed appears to have been the offspring of two males) has occurred to me so often as to remove all possibility of doubt as to its existence. In 1797, the year after I had seen the result of the last mentioned experiment, having prepared a great many white blossoms, I introduced the farina of a white and that of a grey nearly at the same moment into each; and as in the last year the character of the coloured male had prevailed, I used its farina more sparingly than that of the white one; and now almost every pod afforded plants of different colours. The majority, however, were white; but the characters of the two kinds were not sufficiently distinct to allow me to judge with precision whether any of the seeds were produced of common parentage or not. In the last year I was more fortunate; having prepared blossoms of the little early frame pea, I introduced its own farina, and immediately afterwards that of a very large and late grey kind, and I sowed the seeds thus obtained, in the end of last summer. Many of them retained the colour and character of the small early pea, not in the slightest degree altered, and blossomed before they were eighteen inches high; whilst others (taken from the same pods) whose colour was changed, grew to the height of more than four feet and were killed by the frost before any blossoms appeared.

It is evident that in these instances superfœtation took place; and it is equally evident that the seeds were not all of common parentage. Should subsequent experience evince that a single plant may be the offspring of two males, the analogy between animal and vegetable nature may induce some curious conjecture relative to the process of generation in the animal world.

(To be continued.)

IX.

Defcription of an Hygrometer and Photometer. By Mr. John Leslie. *Communicated by the* Inventor.

THESE two inftruments, in their form analogous, however diffimilar in their application, were fuggefted by the fame train of reflections. Experience has amply confirmed the juftnefs of the principles on which they are founded. But the developement of thofe principles, befides the profeffed objects of the inftruments, exhibits feveral claffes of nice and important phyfical inquiries for which they are moft happily calculated. Regarding them as valuable acquifitions to our philofophical apparatus, I was induced to beftow much attention on their improvement, and have through perfeverance fucceeded to my utmoft wifhes. By fucceffive fteps, they have at length attained that fimplicity of conftruction in which the mind refts fatisfied. I confider it my duty, therefore, without farther delay to communicate them to the public. Of the refearches, however, which, by their means, I have made, embracing a confiderable extent of inveftigation, I muft defer the account, as it will form part of a work fhortly to be committed to the prefs, and which it is prefumed will be found to contain a variety of new views in the phyfical fciences. For the prefent, I fhall content myfelf with briefly ftating the progrefs of my ideas, with accurately defcribing the mode of conftructing thofe inftruments, and with pointing out in general terms the fubjects of inquiry for which, by their delicacy and facility of application, they are peculiarly fitted.

My attention was firft directed to the fubject of hygrometry by the perufal of the late Dr. Hutton's very ingenious *Theory of Rain*. The affection of air to humidity, as differently modified by heat, appeared to perform a moft important part in the œconomy of nature. But to afcertain the actual difpofition of the atmofphere was ftill a *defideratum*. Senfible of the imperfection, if not the abfolute inutility, of the ordinary contrivances for that purpofe, I was foon convinced that they depended not only on arbitrary affumptions, but on hypothefis altogether erroneous. Abandoning, therefore, the various expedients to ferve as hygrofcopes, I fought to difcover other principles, and to introduce that geometrical precifion which fo eminently diftinguifhes the more cultivated branches of fcience. An examination of the circumftances attending the action of air on a humid furface feemed to offer the beft profpect of fuccefs.

It is well known that evaporation produces cold, but the nature of the procefs and the true conditions which determine the effect, require ftill to be inveftigated. Water, expofed to free air, fuffers a continued wafte by exhalation; it muft at the fame time fuftain a correfponding expence of heat. In this view, therefore, the temperature of the humid mafs fhould undergo a progreffive and unlimited diminution. Yet fuch is not actually the

the cafe, for the cold induced never paffes certain limits. It is manifeft, therefore, that the evaporable matter muft at laft *receive* heat from fome other fource exactly in proportion as it *lofes* its own. Nor is it difficult to perceive how that will be derived, for each fucceeding portion of air which, in diffolving its due quantity of moifture, touches the humid furface, muft be cooled down to the fame ftandard, and confequently muft depofit the excefs of its heat. Hence, while the repeated *abftractions* of heat are uniform, the correfponding *additions* of heat will continually increafe, till they at laft counterbalance the former, and then the depreffed temperature of the humid furface will remain conftant. But each portion of air, parting with its furplus heat, muft diffolve as much water as will faturate it, and confequently will carry off a quantity of heat proportional to that moifture, and neceffary to its gafeous conftitution and union with atmofpheric air. As thefe two caufes ultimately equalize together, the one will ferve as a meafure of the other; that is, *the cold produced by evaporation will accurately denote the degree of drynefs of the air, or its diftance from the point of faturation.* Thus, the effect of that procefs depends entirely on the difpofition of the air, and is not modified at all by agitation, or the frequent renewal of furfaces. Such means can only accelerate the term of equilibrium, in the fame manner as wind brings a thermometer more quickly to the ftandard than ftill air; though this ftandard in both cafes continues the fame.

Let it be obferved that this reafoning is independent altogether of any hypothefis. By whatever mode the procefs of evaporation is carried on, our general conclufion will hold true, if it be only granted that the conveying of heat and the diffolving of moifture are effects which take place at the fame time. Suppofe it even poffible for the air to hang ftagnant around the humid mafs, and the moifture to be transferred along the contiguous ftrata; ftill the heat being conducted by the fame agents the refult would be unchanged. In reality, however, air, acquiring elafticity by its action on moifture, is quickly fucceeded by frefh portions, which thus maintain a perpetual circulation.

To difcover the drynefs or humidity of the air, therefore, we have only to find the change of temperature induced on a body of water infulated or expofed on all fides to evaporation. This principle I firft eftablifhed in the year 1790. My fituation at that time afforded me all the facilities I could defire of reducing it into practice. Living with the late Mr. Wedgwood, I enjoyed the advantages of his celebrated manufactory, while my efforts were ftimulated by the example of his eminent talents, and by his liberal zeal for the improvement of the fciences.

I procured a cup of unglazed bifcuit-ware which is quite bibulous, about the fize and fhape of a pigeon's egg. This was filled with water, and fufpended freely by a filk thread: befide it was placed a very nice thermometer, having the ordinary degrees fubdivided into tenths, and of a peculiar conftruction, to indicate the difference between its prior and fubfequent ftate. Being plunged into the cup, the mercury quickly defcended and marked on the fcale the depreffion of temperature which the water had fuffered, that is, the meafure of the drynefs of the ambient air. The performance of this double inftrument was very complete,

complete, and for the ſpace of two years I frequently employed it in meteorological obſervations and other reſearches. I was ſtill diſſatisfied, however, with its complex nature, and the attention required in uſing it. Another reſource preſented itſelf in the augmented elaſticity which air acquires in diſſolving moiſture. To meaſure that effect, I contrived a ſimple machine which yet anſwered perfectly the end propoſed. And though, in its original object, it has been ſince ſuperſeded, I found it of the higheſt utility in detecting thoſe minute alterations of volume which take place in the union of various bodies, thus extending the bounds of corpuſcular dynamics, and marking the alliance with philoſophical chemiſtry.

In the ſevere winter of 1795, I was naturally tempted to make experiments on the evaporation of ice, and the cold thereby produced. Inſtead of fixing the thermometer in a block of ice, I had the bulb covered with a congealed cruſt, by repeatedly ſprinkling it with water, and ſuffering this to freeze. Placing in the ſame ſituation another correſponding thermometer with a naked bulb, I was ſurpriſed to remark how quickly and ſteadily their interval reached its *maximum*, extent, the evaporation of a minute film of ice, proving ſufficient to cool down to its proper ſtandard the whole maſs of included quickſilver. Of the legitimacy of this inference I was convinced, when I reflected on the vaſt conſumption of heat which muſt take place during the tranſition into the gaſeous form, of a comparatively ſmall portion of evaporable matter *. Two thermometers, therefore, filled with any expanſible fluid, with quickſilver, alcohol, or air, the bulb of the one being wetted and the other dry, will by their difference denote the ſtate of the air in reſpect to humidity. Nothing was wanted but to combine thoſe inſtruments in ſuch manner that they ſhould indicate merely their difference of temperature. To accompliſh this object, it fortunately occurred to me to employ two hollow glaſs balls, communicating with each other by a narrow tube, which contained ſome coloured liquor. In ordinary caſes, the intermediate liquor would continue ſtationary; for the air in both balls having the ſame temperature, and conſequently the ſame elaſticity, the oppoſite preſſures would preciſely counteract each other. But if, from the action of the external air on the moiſtened ſurface, the one ball became colder, it is manifeſt the liquor would be puſhed towards it by the ſuperior elaſticity of the air included in the other, ſo as to mark by the ſpace of its approach the depreſſion of temperature induced by evaporation †. This contrivance ſucceeded admirably, and after repeated trials I fixed on the moſt ſuitable form and dimenſions. In the courſe of thoſe trials, I was led to the diſcovery of the photometer, which ſhall be hereafter deſcribed. A farther alteration was then made in the conſtruction of the inſtrument, and I was anxious

* A cold of one degree centigrade is produced in a maſs of water by the evaporation of leſs than the 500th part of its weight.

† This ſuppoſes that air expands uniformly with equal additions of heat, a poſition not indeed ſtrictly accurate, but which differs much leſs from the reality than is ſometimes alledged. Experimenters are not even agreed whether thoſe expanſions form a riſing or a deſcending ſeries. At any rate, in the narrow range of the inſtrument, that deviation from regularity becomes altogether inſenſible.

to render it conveniently portable. At laſt I attained my wiſhes ſo completely, that the hygrometer when once adjuſted can ſcarcely be deranged by the roughest treatment. It only remained to hit on ſome coloured liquor which ſhould permanently retain its colour, and at the ſame time ſhould not, by any change of temperature, be liable to modify the elaſticity of the air included with it, either by abſorbing or parting with humidity. Alkaline-lye, tinged with carmine, ſeemed to anſwer the beſt; but ſtill I found that, by the influence of ſtrong light, the liquor gradually changed its colour, while it ſhifted ſomewhat its place and advanced nearer the ball where it expoſed the moſt ſurface. I no longer doubted that the oxygene of the included air united with the colouring matter, and formed a precipitate. Inſtead of common air, therefore, I filled the balls with hydrogene gas, which appears fully to realize my expectation, and from the experience of nearly two months of the brighteſt ſeaſon of the year, I believe I may confidently promiſe on the durability of the colour*. I ſhall now proceeed to deſcribe the hygrometer, and to give ſuch directions as may enable an intelligent perſon, acquainted with ſuch manipulations, to conſtruct it.

To the one end of a ſlender tube, from 4 to 8 inches long, and with an uniform bore from $\frac{1}{30}$ to $\frac{1}{60}$ of an inch wide, is blown a ball about 4 or 6 10ths of an inch in diameter, of coloured glaſs, ſuch as black, blue, or green, and is bent inwards, ſo that its poſterior ſurface ſhall be in a line with the neareſt edge of the tube. A ſimilar tube, though ſomewhat ſhorter, with a bore of equal or greater width, has this enlarged at the one end into a cylinder capable of holding as much liquor as would fill the ſtem of the other tube, and terminated with a clear ball of the ſame ſize as the former. The tubes are a little widened at their other ends, to facilitate the junction. The balls are next to be filled with hydrogene gas, which may be done in various ways. One of the ſimpleſt is to inſert each tube, and faſten it with bees-wax in a narrow necked flaſk containing the gas; then heat the ball with the flame of a candle, and ſuffer it to cool, and repeat this operation two or three times. The ſhorter piece is dipped in deliquiated potaſh tinged with carmine, and a few bubbles of hydrogene gas are forced out by the heat of the hands till, on being allowed to cool, a proper quantity of the coloured liquor riſes. The open ends of the two pieces are then dryed, gently heated, and united by the flame of a blow-pipe in one ſtraight tube. By forcing air with the heat of the hand from one ball into the other, it is eaſy to make the liquor hang together with its ſummit near the middle of the longer ſtem. The whole is then ſuſpended in a cloſe room from the coloured ball, and the other ball plunged in a veſſel of water while a temporary ſcale is attached to the upper ſtem. On adding cold water, the liquor deſcends to near the juncture, and on adding warm water it riſes near the upper ball; the difference between thoſe two temperatures meaſured by a thermometer, and

* It would ſtill be moſt deſireable to employ common air only. Some acid or alkaline liquor, diſpoſed neither to abſtract nor communicate moiſture, may be found capable of retaining its colour under the joint influence of light and oxygene. Probably certain metallic ſolutions have that property. I have very lately tried indigo diſſolved in ſulphuric acid. It ſeems to anſwer well; but at the brumal ſolſtice, and in the murky atmoſpere of London, I cannot eaſily judge what effect ſtrong light would produce.

referred

referred to the intermediate ſpace, gives the magnitude of a degree *. I have adopted, as the moſt natural and convenient diviſion of the thermometer, that of celſius or the centigrade; ſo that each degree of the hygrometer correſponds to the thouſandth part of the interval between the freezing and boiling points. All the liquor is now puſhed back into one of the balls, and by applying the flame of a candle the ſhorter branch is gently bent round till its ball touches the inſide of the oppoſite tube, and lies $\frac{3}{4}$ of an inch below the inflected ball. The ſcale, which ſhould contain from 50 to 150 degrees, is next divided, † and is faſtened between the two branches with cement made of roſin and bees-wax. (See *a drawing of the inſtrument of the full ſize,* Fig. 1. Plate XIX.)

The inſtrument is adjuſted by throwing air from the one to the other ball till the liquor reſts at the top of the ſcale. The lower ball and its annexed cylinder, are covered with thin ſilk of the ſame colour as the upper ball, and a few threads are likewiſe lapped about that part of the tube which it touches. The inſtrument is laſtly cemented into a piece of wood, either end of which admits a cylindrical caſe that ſerves equally to protect or to hold it. On other occaſions, the hygrometer is inſerted into the ſocket of a round bottom piece where it ſtands vertical.

But this inſtrument does not merely point out the dryneſs of the air; it enables us to determine the *abſolute quantity* of moiſture which it is capable of imbibing: for the converſion of water into ſteam is found to conſume 524 ‡ degrees of the centigrade diviſion, and evaporation, analagous in its effect may be preſumed to occaſion the ſame waſte of heat. If, therefore, air had the ſame capacity as water, for each degree of the hygrometer it would depoſit as much heat as it would abſtract by diſſolving the $\frac{1}{5240}$ part of its weight of humidity. But the capacity of air is to that of water as 11 to 6, and conſequently it would require in that proportion a greater evaporation to produce the ſame effect. We may hence conclude, that, for each hygrometeric degree, the air would require $\frac{11}{6} \times \frac{1}{5240}$ or $\frac{1}{2858}$ part by weight of water to effect ſaturation.

Strictly ſpeaking the degrees marked by this hygrometer do not meaſure the dryneſs of the air at its actual temperature, but only its ſtate of dryneſs when cooled down to the

* When a ſtandard inſtrument is once conſtructed, others can be more eaſily, though not quite ſo exactly, graduated by compariſon. In a dry room, they may be uſed for the occaſion as hygrometers, and the magnitude of a degree in each thus determined. If the weather is clear and ſettled, it would be preferable to convert them into photometers, and expoſe them excluſively to the direct rays of the ſun, when moderately elevated above the horizon. Other expedients will ſuggeſt themſelves.

† If the temperature of the room happens to vary during the obſervation, it muſt be allowed for. When the ſeaſen permits, the upper ball may be covered with ſnow, which will render the graduation more certain. There is always a ſmall correction to be made for the inverted poſition of the cylindrical reſervoir in the finiſhed ſtate of the inſtrument: let n denote the width of that cylinder compared with the bore of the tube, and a = the length of 100 degrees of the hygrometer in Engliſh inches; this ſpace muſt be diminiſhed, before diviſion, by the $\frac{a}{8n}$th part. The formula is eaſily derived.

‡ This has been ſtated, but I expect ſoon to aſcertain the quantity with more preciſion.

ſtandard

ftandard of the wet ball. The law, however, being known of the diffolving power of air as affected by heat, it is eafy from the difpofition of the air with refpect to humidity at one temperature to derive that at any other. It will fuffice to mention the refult of a number of careful experiments:—fuppofing air at the freezing point to be capable of holding 50 parts of moifture; at 10 degrees centigrade, it will hold 100; at 20°, 200; at 30°, 400;—thus doubling at each increafe of 10 degrees. Hence a table may be conftructed by which thofe converfions will be eafily made.

To omit nothing that tends to elucidate the theory of the inftrument, I muft obferve, that the air in its contact with the humid furface is not abfolutely cooled to the fame temperature: the air and water really meet each other at an intermediate point determined by their compounded denfity and capacity. Confequently the indications of the hygrometer ought to be augmented by the $\frac{1}{464}$ part, or $\frac{11}{6} \times \frac{1}{850}$. But this quantity is too fmall in any cafe to be regarded.

Inftead of water, the coated ball may be wetted with æther, alcohol, or other volatile liquids, and hence will be determined the attractions of air for thofe fubftances under the feveral modifications of heat, preffure, and previous moifture. It is eafy likewife, to reverfe the experiments by covering the upper ball with fulphuric acid, potafh, or any deliquefcent falt. A vaft field of inquiry thus opens to view, prefenting facts not only important in themfelves, but calculated to correct and expand our ideas on the fubject of chemical affinities.

By help of this hygrometer I have collected a number of meteorological obfervations, and inveftigated the nature and production of dews and their curious affections to the metals, glafs, and vegetable fubftances. But what is more important, I have afcertained the attractions of air for humidity under different preffures, and with different temperatures. I have examined likewife the properties of other gafes in thofe refpects, and have fucceeded in accommodating the refult to a few geometric laws of beautiful fimplicity. I forbear to fay more at prefent, but cannot help confidering the fubject as almoft exhaufted, and the modifications of the atmofphere as at length reduced to a fcience.

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Having fo minutely defcribed the hygrometer, I fhall difpatch the photometer, though the more curious inftrument, in a few words. In fact it is conftructed in the fame manner, only the upper ball is blown of black glafs, or is blackened, and the lower one is quite diaphanous and free of fpecks. The former detains the incident light, while the latter tranfmits it freely. But light, in proportion to its abforption, caufes heat, whether uniting with bodies it really conftitutes the matter of heat, or only excites heat in the act of combination. But though the black ball acquires conftant additions of heat, its temperature will not uniformly and perpetually increafe; for the accumulated heat will at laft be conducted off by the furrounding air exactly as it is received. The depreffion of the liquor, therefore, will meafure the momentary afflux of light. To prevent the irregular effects of winds, which might accelerate that difperfion, the inftrument is inclofed within

a glafs

a glafs cafe. But this cafe ferves alfo an important purpofe, for, by confining the circulation of the ambient air which alone transfers the continual augmentation of heat, it doubles the performance of the inftrument. The cylindrical cafe fhould be made of clear glafs neatly rounded over, and hermetically fealed at the end. Its width is not of much importance, only it fhould leave a free fpace not lefs than $\frac{1}{10}$ of an inch round the balls, and at leaft half an inch at the top. Indeed both the fize and form may be regulated by convenience, for I found a receiver of 2200 inches to afford quantities fcarcely one-tenth lefs than thofe given by a cafe of the ordinary dimenfions.

Since I firft conftructed this inftrument, in the autumn of 1797, I have been delighted with the nicety of its performance. It not only meafures the direct rays of the fun, but the reflected light of the fky, for which it is principally defigned. It is fenfible to every fluctuation of the atmofphere, marks the progrefs and decline of the light of day, and the periodic increafe and diminution of the brightnefs of the year. It enables us likewife to eftimate other lights, fuch as the flame of a candle. By comparing two photometers, it is eafy to determine the relative properties of different coloured fubftances—in reflecting, abforbing, and tranfmitting light. In the fame manner, they will determine the queftion, whether the particles of light are fpread over the prifmatic fpectrum with equal intenfity. By help of this inftrument, too, we can meafure the quantity of light tranfmitted through various diaphanous bodies, and that reflected or abforbed at different angles of incidence from polifhed or rough furfaces;—in fhort, perform with the utmoft facility all thofe ingenious experiments which have exercifed the fagacity of Bouguer and Lambert. Another fet of inquiries for which the photometer is nicely calculated, is to difcover the conducting powers of different fluids for heat. If the glafs cafe, for inftance, be filled with a gas of higher conducting power than common air, the inftrument will be proportionally lefs affected by the fame afflux of light, fince thofe are the two balancing conditions. With air, too, of different denfities, the effects are materially different. In that way I have examined various liquids and gafes, nay jellies and ice. My experiments on thefe and other points are nearly completed, and afford refults which are fatisfactory and important.*

JOHN LESLIE.

X.

Defcription of the Hydroftatic Lamp of Mr. PETER KEIR. *(W. N.)*

IT has at all times been confidered as a moft defireable object, to afford a conftant and equable fupply of oil to the wick in lamps, and it is an object or condition no lefs effential to the œconomy of this ufeful inftrument, that the light from the flame fhould be inter-

* This paper was drawn up at Hamburgh in the beginning of laft July, for the purpofe of infertion in the foreign fcientific journals. Previous to its appearance in Englifh, I have revifed it, and made fome flight alterations and additions.

cepted

cepted as little as poffible. It happens unfortunately that thefe two conditions have ufually been found to militate againft each other. If the receptacle for oil be made tall and thin, the light will indeed be difperfed like that of a candle through a very large portion of the furrounding fpace or fphere; but as the confumption of a fmall portion of oil will occafion a confiderable fall in fo narrow a veffel, the fluid will foon be depreffed too low for the capillary attraction of the wick to raife it with effect, and confequently the light will firft decay and then go out. If on the other hand, the oil veffel be made broad and comparatively fhallow, the flame will certainly be maintained for a long time, becaufe a large quantity of oil muft be confumed before the fall will be of any practical confequence; but at the fame time the fhadow of this broad veffel will obfcure nearly an entire hemifphere, if the wick be in the middle of its upper furface, and it will obftruct much of the light even in the moft favorable pofition of the burner, namely, on one fide or edge of the veffel.

To remove thefe inconveniences as much as poffible, there have been a variety of contrivances, among which the inverted veffel called the Fountain, is the moft remarkable and the moft efteemed. This veffel, or receptacle of oil, may be confidered as a bottle filled and inverted with its neck plunged in a bafon of the fame fluid. The experiment may be familiarly made with a bafon of water, to the fatisfaction of thofe who are unacquainted with the effects of the preffure of the atmofphere, which prevents the water from quitting the bottle. In this fituation it is feen, that when the water in the bafon is by any means depreffed, a bubble or portion of air forces itfelf into the bottle and rifes to the top, while an equal bulk of water defcends into the bafon. Or the fame effect may be feen in a glafs apparatus very common in bird cages. In this manner it is that the oil flows down and fupplies the confumption in lamps, which are provided with a refervoir of this kind.

The fhadow caft by the vafe or refervoir of a fountain lamp, is an inconvenience which may be much diminifhed by fixing the burner at the end of a tube or branch made as long as may be convenient. Another inconvenience is however lefs fufceptible of an adequate remedy; namely, the expanfion of the air included in the upper part of the refervoir, when the temperature of the place is by any means raifed. This expanfion will caufe the oil to defcend in greater quantity than the combuftion demands, and has been found very prejudicial in affembly rooms, and other places of general refort, by the overflow of that fluid upon the floors, as well as the clothes of the company. It feems as if the famous Robert Hooke, who could not have been unacquainted with the fountain lamp, was led to the invention of his very ingenious lamp with a float, from reflections on this imperfection. This lamp, which is defcribed in Birch's Hiftory of the Royal Society, confifted of an hemifpherical veffel to hold oil, with a lip at one edge for the wick. An hemifpherical folid was conftructed fo as very nearly to fit the cavity of the veffel, and was fufpended by an horizontal axis, upon which it could freely move or librate. It would therefore hang in the veffel and almoft totally fill it, when not occupied by any other fubftance; but as its fpecific gravity was defignedly made equal to half the fpecific gravity of oil, it would

would be readily understood that it would float when oil was introduced at the lip. And as the floatage would be regulated or confined by the horizontal axis, there were two very curious properties obtained; that the bulk of the solid above the vessel would always be equal to that of the oil itself, and the oil would remain at the same height till it was all burned. It may perhaps afford some amusement to the reader to deduce these properties from the structure of the machine, which may be done with very little consideration.

Dr. Hooke's contrivance, nevertheless, though worthy of great admiration, appears to be practically nothing more than an improvement on the shallow dish lamp. For at the commencement the light does not illuminate much more than one hemisphere of the surrounding space; and when the float has descended, towards the end of the consumption, the illuminated part is upwards of three-fourths of that space.

A much completer solution of the practical problem of illuminating the greatest possible space, while the fall of the oil is rendered the least possible, was given to the public by Mr. Keir, of Kentish Town, in the year 1787, for which he obtained a patent. It seems probable that the advantages of this contrivance being directed to an object so much less striking than that of the lamp of Argand, which was then new in the mind of the public, might, together with the confined exhibition of an article reserved to a few hands, have prevented it from becoming generally known; and as it is probably unknown to many of my philosophical readers, I have thought an account of its construction would be acceptable.

In plate XX are exhibited a view and section of one of these lamps. The section only requires to be explained. From the slender figure of the vase it is evident that the flame is permitted to throw its light in all directions downwards and upwards, nearly in the same manner as a candle. The interior parts is divided into several compartments by the diaphragms at F and C. The space A A, above F, is open to the atmosphere; but the space B B, beneath C, is close. A tube F G proceeds from the space A A to the space B B, so as to reach nearly to the bottom at G: and another tube C D, proceeds from B B upwards, through A A, without communicating with this last space, and is enlarged at the upper part, so as to receive a wick with the apparatus of Argand, or any other. A solution of sea salt, or the mother water of salt, being first poured in by measure at E, flows down the tube into B B, and fills that space. A like measure of oil is next poured, which also descends into B B, and forces the dense saline liquor upwards, through G F, into the space A A. The specific gravity of this last is adjusted by dilution, so that when the space A A is properly filled the oil shall stand in equilibrio at the requisite height near E; that is to say, the surfaces in A, and at E, are elevated above the lower orifice at G in the inverse proportion of the specific gravities. This proportion is usually about 3 to 4. So that if any part of the oil be taken away from E by combustion, or otherwise, there will be a subsidence of the heavy fluid in A A to preserve the equilibrium; and during the whole subsidence in A A there will be a correspondent depression of the upper surface of oil, near E, which will be measured by four third parts of the first elevation of the dense fluid above the partition,

partition, F D. Now the fall in A A, may be rendered very small by enlarging the diameter of the vessel at that part, and at B B; and the elevation of E above A, and consequently the insulation of the radiant flame may be governed at pleasure by prolonging the interval D C.

It is possible in the manipulation of this lamp that some oil or pieces of snuff may fall into the space A A, and float upon the liquid. This effect is to a certain extent beneficial, because the covering of oil prevents evaporation: but if this should require to be remedied, it is easily done by pouring the whole contents of the lamp into a bason, and after a few moments repose or straining, returning the liquids again into the lamp at E by a syphon or funnel, in which they will take their proper places by means of their relative weights.

We may recapitulate the good qualities of this lamp in a few words. 1. It is capable of any desired form or apparatus for the burners. 2. It presents no obstacle to intercept the emitted light; and 3. As it raises the oil by the mere gravitation of a non-elastic fluid it cannot in any case, like the fountain lamp, raise more than is wanted *.

XI.

Reflections on the Decomposition of the Muriate of Soda by the Oxide of Lead. By CIT. VAUQUELIN.

CHEMISTS agree that the oxide of lead decomposes the muriate of soda; and this, as is well known, is one of the methods which have been proposed to obtain soda from this salt; but the manner in which this decomposition is effected still remains without a satisfactory explanation. In reality all the explanations which have been given imply a

* On the relative consumption and expence of tallow and oil promised (at p. 365 of our present volume) to be resumed, I may here observe that the manufacturers reckon that the lamp of Argand, of the common size, having the mean diameter of the channel containing the wick, = 0, 8, inch, will require half a pint of good whale oil to give its maximum of light for seven hours; and that I found by the method of shadows, some years ago, that such a lamp gives as much light as eight tallow candles of the size, called the middling six, fresh snuffed. The average light of a candle, allowing for the neglect of snuffing, is much lower; but on the other hand the combustion is complete in this lamp, but not so much so in this candle as in those of smaller size. It would be an useful research to determine the economy of the production of light by equal weights of candles of all the different sizes. To return, however, to our comparison, I am disposed on the whole to estimate the lamp upon our present knowledge as equal in duration and effect to one pound of candles; and to conclude, that the quantity of light will follow that of the oil, in all lamps, with thin wicks. And hence, as oil sells at seven shillings and six-pence per gallon, and tallow candles at ten-pence half-penny the pound, the expence of light by oil will be to that by tallow, as 8 to 21. Or if the candle would illuminate, during four hours for a penny, a lamp would at the same expence in oil give equal light for ten hours and a half.---N.

* Read to the French National Institute, on the first Priarial, and in the year 7, (May 20, 1799.) Inserted in the Annals de Chimie, xxxi. p. 2.

manifeft contradiction. The fuperior affinity of the oxide of lead for the muriatic acid, which is urged for the folution of the difficulty, is deftroyed by the decompofition of the muriate of lead, by means of cauftic foda; and that of the carbonic acid contained in the litharge, to which account has been had, is equally contradicted by the abfolute inaction of the carbonate of lead upon marine falt, and by minium which contains little of this acid, but neverthelefs decompofes the muriate of foda.

To avoid this difficulty, fome authors have affirmed that the oxide of lead decompofes fea falt only in part; but this was an error naturally produced by a faulty explanation of a fact which is true in itfelf.

I have, on the contrary, clearly afcertained that the decompofition of this falt is compleat, when the quantity of oxide of lead is fufficient; for how can this partial action take place if pure foda be obtained? and why fhould the procefs ftop at any determined limit, without having been weakened by fome known caufe?

With the intention of clearing up this fubject I made the following experiments:

1. I mixed feven parts of finely ground litharge, with one part of muriate of foda; I fprinkled the mixture with a fufficient quantity of water, to give it the confiftence of thin foup; after which I agitated it for feveral hours, in order to renew the furfaces and facilitate the action of the ingredients.

The oxide of lead loft its natural colour, and gradually became white; its volume was fingularly increafed; and in proportion as the water was abforbed the mixture acquired a confiderable confiftence, fo that I was obliged at feveral reiterations, to add a quantity of water. Laftly, at the end of four days the litharge appearing to have entirely changed its nature, and no further progreffion being obfervable in the effects, I diluted the mafs with feven or eight parts of water, and filtered. The filtered liquor had a very evident alkaline tafte, and contained a fmall quantity of the muriate of lead in folution, without one atom of the muriate of foda. When it was reduced to about $\frac{1}{10}$th of its volume, it afforded cryftals of carbonate of foda, rendered opaque by fome traces of muriate of lead.

2. The oxide of lead wafhed and dried, had a dirty white colour, and its weight was increafed about one eighth part. When gently heated, it acquired a very fine lemon yellow colour, with the lofs of 0,025 of its weight. A part of this oxide, heated with a folution of cauftic foda, prefented the following phenomena:

1. Its orange colour was converted into a dull yellow. 2. Its pulverent form was converted into that of needles, and its bulk was greatly diminifhed; the folution of foda had not perceptibly changed its tafte, but it afforded a very abundant black precipitate, by the hydro-fulphuret of foda; a white depofition, with the muriatic of the nitric acids; but that formed by the latter was re-diffolved in an excefs of acid. Thefe depofitions were perfectly fimilar to the portion of matter which was not diffolved by the foda.

3. One hundred parts of the fame matter were heated with diluted nitric acid, which diffolved the greateft part; the remainder being of a white colour and cryftalline form. This fubftance, feparated from the fluid, was fufed upon ignited coals, affumed a black colour,

colour, and flew off in ſmoke, without leaving any metallic lead; effects which prove that this ſubſtance is the common muriate of lead. The portion diſſolved in the nitric acid being evaporated by a gentle heat, afforded cryſtals of the nitrate of lead, among which there were ſome needles of the muriate of lead, which had been diſſolved by the nitric acid.

4. 100 other parts of the ſame ſubſtance, heated with boiling water, were not perceptibly diſſolved, and the fluid afforded ſcarcely any ſigns of lead, by the hydro-ſulphuret of potaſh.

Theſe experiments appear to me to prove: 1. That litharge, after it has decompoſed the muriate of ſoda, is a muriate of lead, with exceſs of oxide. 2. That the cauſtic alkilis diſſolve this ſalt without decompoſing it. 3. That it is by virtue of the affinity which the muriate of lead has for this oxide that litharge decompoſes ſea ſalt. 4. That it is this quantity of oxide, exceeding the proportion of common muriate of lead, which communicates to this ſalt the property of acquiring a lemon yellow colour by heat; an event which does not happen with the true or common muriate of lead. 5. That it is this exceſs which renders this muriate of lead nearly inſoluble in water. 6. And that laſtly it is this which is diſſolved in the nitric acid, and forms the nitrate of lead, leaving behind it the common neutral muriate of lead.

It is ſo true that the oxide of lead does not decompoſe the muriate of ſoda, but by forming a muriate of lead with exceſs of oxide; that when the common muriate of lead is decompoſed by cauſtic ſoda, it is impoſſible even to deprive it of the whole of its muriatic acid. There conſtantly remains a ſufficient quantity to put the lead into the ſtate in which it is found after the decompoſition of the muriate of ſoda. This is proved by the yellow colour it acquires in the fire, by its decompoſition with the nitric acid, the ſeparation of the common muriate of lead, and the formation of the nitrate of lead, which takes place in this operation.

It is therefore truly, by virtue of a double affinity, that the oxide of lead decompoſes the muriate of ſoda: namely, by the united forces of the oxide of lead for the muriatic acid, and the muriate of lead for an exceſs of oxide.

This conſideration ſhews why ſo large a quantity of the oxide of lead is required for completely decompoſing the muriate of ſoda; becauſe five-ſixths at leaſt of this oxide are employed, not in decompoſing the marine ſalt, but in forming the muriate of lead with exceſs of oxide; and the fourth part at leaſt of this oxide unites with the muriatic acid in the ſtate of true muriate of lead. It will therefore be proper to aſſert that litharge compleatly decompoſes the muriate of ſoda, when the doſe is ſufficient, and that ſoda never totally decompoſes the muriate of lead, however conſiderable the doſe.

If the carbonate of lead do not decompoſe the muriate of ſoda, it ought to follow that the carbonate of ſoda ſhould compleatly decompoſe the muriate of lead; and this in fact is confirmed by experience.

It may moreover be obſerved, that the muriate of lead is not the only ſalt of this kind which

which has the property of absorbing an excess of oxide; for the sulphate and nitrate, and perhaps others also, possess it. The proof of this may be had by decomposing the nitrate and the sulphate of lead by the caustic alkalies; and particularly by ammoniac. There will constantly remain a small quantity of these acids in the oxide of lead, which will become visible the first by the nitrous vapours which are disengaged by heating the washed precipitate; and the second by the residue left by the nitric acid, with the precipitate obtained from sulphate of lead, which residue is sulphate of lead. The decomposition of the muriate of soda is probably effected by the same means. I propose to examine this question speedily, and shall give an account of my operations to the class.

PHILOSOPHICAL NEWS, ACCOUNTS OF BOOKS, &c.

Philosophical Transactions of the Royal Society of London, for the Year 1799. *Part II. London.* ELMSLY.

10. AN Account of the Dissection of an Hermaphrodite Dog. To which are prefixed some Observations on Hermaphrodites in general. By Everard Home, Esq. F.R.S.—11. An Enquiry concerning the Weight ascribed to Heat. By Benjamin, Count of Rumford, F.R.S. M.R. I.A. &c. &c.—12. An Account of some Experiments on the Fecundation of Vegetables. By Thomas Andrew Knight, Esq.—13. Observations on the different Species of Asiatic Elephants, and their Mode of Dentition. By J. Corse, Esq.—14. Some Observations on the Structure of the Teeth of Graminivorous Quadrupedes; particularly those of the Elephant and Sus Æthiopicus. By Everard Home, Esq. F.R.S.—15. Experiments to determine the Quantity of tanning principle and gallic Acid contained in the Bark of various Trees. By George Biggin, Esq.—16. Essay on the Resolution of Algebraic Equations; attempting to distinguish particularly the real principle of every method, and the true causes of the limitations to which it is subject. By Giffon Wilson, Esq.—17. On different Sorts of Lime used in Agriculture. By Smithson Tennant, Esq. F.R.S.—18. Experiments and Observations on Shell and Bone. By Charles Hatchett, Esq. F.R.S.—19. A Catalogue of Oriental Manuscripts, presented to the Royal Society. By Sir William and Lady Jones. Presents received by the Royal Society, from November 1798 to June 1799. Index.

Prospectus or Index to Lord Dundonald's intended Publication. Single Sheet, Octavo; presented to his Friends and Men of Science, by his Lordship, but not (as I suppose) sold.

In this pamphlet the Earl of Dundonald announces that he intends soon to publish a detailed account of all his discoveries, processes, and patents, and of the circumstances which have hitherto prevented their becoming beneficial either to himself or the public. He complains of the most cruel and oppressive usage from individuals, and neglects on the part of the government, and states that he has not yet determined whether he will surrender or

lay open his patents and processes, because he is disposed to wait a few months to ascertain whether he may form any pecuniary connections which may render his discoveries beneficial to himself, as well as the public. In the case of such surrender he shall think himself at liberty to offer his services to foreign nations.

I cannot attempt to give my readers any abridgment of an index, or table of contents; and still less am I enabled to enter upon any of the subjects which are hinted at, and bear relation to the undertakings of the author, and the depressions his exertions may have suffered. The chief objects of the earl's researches and inventions are well known to the public. The preparation and purification of alkalis, the manufacture of soap, and its uses with different waters; new methods of making ceruse, alum, and other salts; improvements in the manufacture of iron; analysis of fossil coal, with the qualities and uses of its products, coak, coal tar, coal oil, ammoniac; analysis of wheat, production of sugar, and of vegetable alkali; examination and treatment of starch makers' liquor, &c. &c. are among the subjects enumerated in this prospectus.

Sugar from the white Beet.

Mr. Accum has presented me with samples of this sugar received from Berlin, where I understand it is now very commonly manufactured. The written account of the culture, produce, and cheapness received at the same time, appearing to want some corrections, I shall only state at present that the samples were 1. a brown or pale straw coloured sugar, in lumps or agglutinated grains, forming a coarse dry powder. It is not very sweet, and has a peculiar, though not strong, smell, which I think resembles that of some articles of confectionary consisting of sugar and flour heated or fried together. Of this sugar the beet is stated to afford five per cent. of its weight, leaving a pulp which is an excellent food for cattle. 2. A refined sugar, in very small crystalline grains, forming a powder of which the particles are slightly disposed to adhere, and which, when laid upon writing paper, has very nearly the same whiteness. I could not ascertain the figure of any of the grains under a deep magnifier, as most of them seem to be partly rounded. It has no foreign smell or taste. Equal weights of this and of good loaf sugar were separately dissolved in equal weights of water; and six out of seven gentlemen, who were present, and tasted the solutions, without knowing which was the beet sugar, determined that the solution of this last was the sweetest. I was among those who thought so: but it appeared to me that its flavour resembled a coarser sugar than that against which it was tried. From this notion I afterwards took two wine glasses of water, and sweetened the one with beet sugar, and the other with loaf sugar, with the addition of a small proportion of fine moist sugar. When the tastes resembled each other as nearly as I could bring them, I submitted them to the judgment of the company present who, from the irregularity of their conjectures, did not seem to find any notable difference. And when I myself again took up the glasses without noticing the distinctive marks, and endeavoured by the taste to determine which was the beet sugar, it

happened

happened that I was miſtaken in my deciſion. This refined ſugar ſeems therefore to be of conſiderable purity and ſtrength. It is obtained from the other ſugar, in the quantity of 55 per cent., together with 25 per cent. of reſidual ſyrup or molaſſes. 3. The other article was a bottle of this molaſſes. It is ſweet with a ſingular vegetable flavour, rather fragrant, and would, I doubt not, afford either a pleaſant vinous liquor by fermentation, or a conſiderable quantity of ardent ſpirit.

Count Rumford's Experimental Eſſays, Political, Economical, and Philoſophical, Eſſay X. Part I. on the Conſtruction of Kitchen Fire-Places and Kitchen Utenſils, together with Remarks and Obſervations relating to the various Proceſſes of Cookery, and Propoſals for improving that moſt uſeful Art. Octavo. 94 Pages. 7 Plates. Price 2s. 6d. Cadell and Davies.

As I have uſually given an abridged account of the valuable publications of this author, and mean to do the ſame with regard to this eſſay, I ſhall at preſent give the contents only, which are; Introduction. Chap. I. Of the imperfection of the kitchen fire-places now in common uſe—Objects particularly to be had in view in attempts to improve them—Of the diſtribution of the various parts of the machinery of a kitchen—Of the method to be obſerved in forming the plan of a kitchen that is to be fitted up, and in laying out the work. Chap. II. Detailed accounts, illuſtrated by correct plans of various kitchens, public and private, that have already been conſtructed on the author's principles, and under his immediate direction. Chap. III. Of the alterations and improvements that may be made in the kitchen fire-places now in common uſe in Great-Britain—All improvements in kitchen fire-places impoſſible, as long as they continue to be incumbered with ſmoke jacks. They occaſion an enormous waſte of fuel. Common jacks that go with a weight are much better. Ovens and boilers that are connected with a kitchen range ſhould be detached from it, and heated each by its own ſeparate fire. The cloſed fire-places for iron ovens and roaſters can hardly be made too ſmall.—Of the various means that may be uſed for improving the large open fire-places of kitchens.—Of the cottage fire-places now in common uſe, and of the means of improving them.—Of the very great uſe that ſmall ovens conſtructed of thin ſheet iron would be of to cottages.—Of the great importance of improving the implements and utenſils uſed by the poor in cooking their food.—No improvement in their method of preparing their food poſſible without it.—Deſcription of an oven ſuitable for a poor family, with an eſtimate of the coſt of it.—Of neſts of three or four ſmaller ovens heated by one fire.—Of the utility of theſe neſts of ovens in the kitchens of private families.—They may be fitted up at a very ſmall expence.—Occaſional remarks reſpecting the materials proper to be uſed in conſtructing the ſides and backs of open chimney fire-places.—Appendix.—An account of our experiment that was made to aſcertain the expence of putting up a ſmall oven, ſuitable for the family of a cottager.

A Tranſlation

A Tranſlation of the Table of Chemical Nomenclature propoſed by Guyton, formerly de Morveau, Lavoiſier, Berthollet, and de Fourcroy; with Explanations, Additions, and Alterations: To which are ſubjoined Tables of ſingle Elective Attraction, Tables of Chemical Symbols, Tables of the preciſe Forces of Chemical Attractions, and Schemes, and Explanations of Caſes of ſingle and double Elective Attractions. Second Edition, enlarged and corrected. By George Pearſon, M. D. Quarto, 156 *Pages, with ten large Tables. Johnſon,* 1799.

I ſhall give ſome account of this excellent ſummary of chemical ſcience and nomenclature in the next Number of the Journal.

A Deſcription, with Plates, of the Time Keeper invented by the late Mr. Thomas Mudge: to which is prefixed a Narrative by Thomas Mudge, his Son, of Meaſures taken to give Effect to the Invention, ſince the Reward beſtowed upon it by the Houſe of Commons in the Year 1793; *a Republication of a Tract by the late Mr. Mudge on the Improvement of Time Keepers, and a Series of Letters written by him to his Excellency Count Bruhl, between the Years* 1773 *and* 1787. Quarto, 176 Pages, with 9 Plates, and a good Portrait of the late Mr. Mudge. London, ſold for the Author by Payne, Cadell, &c.

This work will be conſidered as an excellent ſtore of facts, relative to a great artiſt and worthy man. I am ſorry that there has been ſo much controverſy reſpecting them, that I cannot with any fairneſs give an abridged ſtatement of the narrative of Mr. Mudge, without entering into an ample field of diſcuſſion and enquiry, as to what may be ſaid by his opponents. The impartiality of a journaliſt would require this; and I think that however intereſting the ſubject may be to a certain part of the public, it would ſcarcely be enough ſo to juſtify the detail in our work. The title will ſhew the value of this performance in thoſe reſpects which can form no grounds of diſpute.

An Eſſay on Electricity, explaining the Principles of that uſeful Science, and deſcribing the Inſtruments contrived to illuſtrate the Theory, or render the Practice entertaining. Illuſtrated with ſix Plates; to which is added a Letter to the Author from Mr. John Birch, Surgeon, on the Subject of Medical Electricity. By the late George Adams, Mathematical Inſtrument Maker. Fifth Edition, with Corrections and Additions. By William Jones, Mathematical Inſtrument Maker. Octavo, 584 Pages, with an Index. London, ſold by the Editor, price 8s.

This new edition of Mr. Adams's very uſeful and entertaining collection of electrical facts and experiments, is conſiderably improved by extending ſome of the deſcriptions, ſupplying omiſſions, and inſerting ſuch diſcoveries, particularly thoſe concerning the Doubler, the Plate Machine, and Galearies, as have been made ſince the time of the Author, and were indiſpenſibly neceſſary to bring the treatiſe to the level of our preſent knowledge of the ſubject.

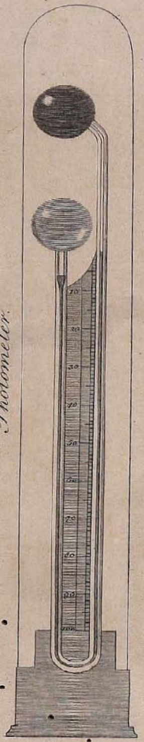
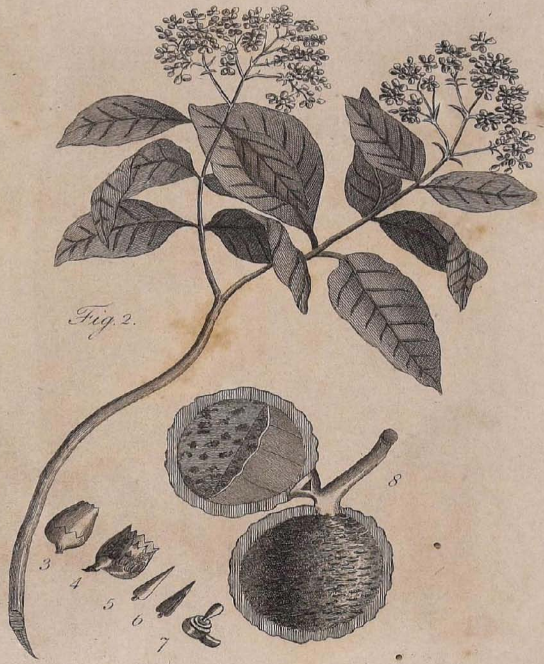
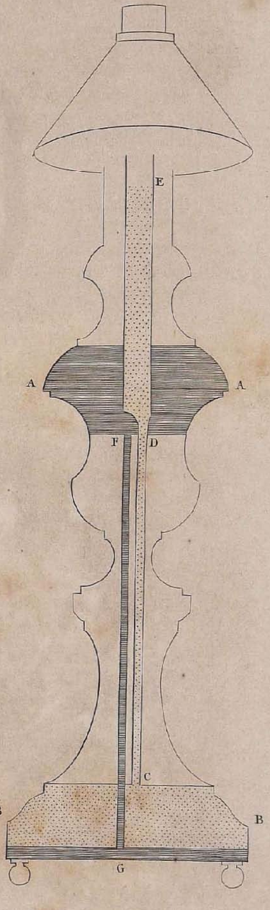


Fig. 1.



Urceola Elastica.

Fig. 2.



Keir's hydrostatic Lamp.

H. Mutlow fc.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY 1800.

ARTICLE I.

*Experiments on Indigo.—By a Correſpondent.**

To. Mr. NICHOLSON.

Edinburgh, Jan. 4, 1800.

SIR,

I WISH, through the medium of your Journal, to make known to chemiſts ſome experiments, which, I hope, will not only be reckoned curious in themſelves, (as they ſeem to throw a new light on a colouring matter procured from organic bodies) but of much moment, as they relate to a ſubſtance, of the utmoſt value to our commerce and manufactures.

The commodity I allude to is the dye indigo, in which I think I have detected a new principle, which has been overlooked by other chemiſts; this principle is of ſo volatile a nature, that it could only be diſcovered by the means I uſed, to which I was accidentally led by my firſt experiment, as you will obſerve in the ſequel; and may mark the reaſon why the accurate Scheele, Bergman, and others, have taken no notice of it, as they invariably calcined the carbonaceous reſidue in the open air.

* This communication was accompanied by a letter containing the name and addreſs of the author, and alſo of the friend to whom he makes his acknowledgments.---N.

It was in consequence of a conversation with an eminent chemist here, that I was led to make the following experiments on indigo; and I beg leave to take this opportunity of making my acknowledgements to him, for the trouble he has taken in repeating most of them, and adding others, which will be particularly noticed here, and which very much illustrate the general opinion I have formed of the changes that indigo undergoes in assuming the blue colour.

That gentleman, considering carbon as a compound, from some analagous reasoning, had been led to think indigo the base of that substance, and which, by its union with oxigen, formed charcoal, and some accounts given of the mode of procuring indigo, strongly favored this supposition; I, on the contrary, had conceived an opinion, that black was owing to an absence of oxigen; and that purple and blue held the next degree in oxigenation, of which we have many examples in the mineral kingdom.

I therefore sought, by means of inflammable bodies, to divest indigo of that portion of oxigen I supposed it to retain, and to discover, if possible, its base. My first experiment was more astonishing than satisfactory; I had mixed a quantity of indigo with sulphur of pot-ash, phosphorus and water, and was boiling them in a glass tube by the flame of a candle, when I was surprised with the appearance of a leaf of a metallic aspect, (in some places of a golden, and in others of a silvery hue) precipitated on the sides of the tube; (I must here observe that there seemed no appearances of solution; that the indigo never came to the green, and that the golden leaf took first a purple appearance, and assumed the metallic one immediately, when the phosphorus came off in white vapour) this leaf I long considered as a compound of phosphorus, or at least the phosphoric acid, or of some change wrought on them; I afterwards procured the same substance in needles, by heating the same matters, a little moistened with water in a crucible, and by phosphorus and indigo only, which confirmed me in the above opinion, till I found that the needles could be obtained by means of a lens concentrating the rays of the sun on indigo, under a jar of atmospherical air; it however should be noted that the lens was of a moderate size, and not powerful enough to inflame the indigo completely. I have since I think got sufficient data to prove, that these needles are a sublimation of a substance sui generis, and that they are probably dispersed in a state of combination through the whole animal and vegetable world, and in such fossile bodies as have once been organized.

I will here divide this account into three parts; I will first take notice of the properties of the needles, and leaf, then detail a singular one which I suppose to belong to the resinous matter, and finally observe the action of certain bodies on indigo in substance, chiefly with a view to show, that indigo acquires a green colour by absorbing oxigen, not by giving it out, according to Berthollet, Bancroft, and others; and that in Hausseman's famous experiments, an acid was most probably present to absorb the alkali, which was the true reason of the indigo's being precipitated.

Needles apparently metallic are to be obtained by sublimating indigo, either with phosphorus and sulphure of pot-ash, with phosphorus alone, in some cases from indigo per se, (if

(if the temperature is well regulated) expoſed to heat in a crucible, and by the rays of the ſun concentrated by means of a middle ſized lens on indigo; and the metallic leaf may be procured by ſulphure of pot-aſh, phoſphorus and water, or volatile alkali and phoſphorus boiled with indigo; and in the dry way by diſtilling indigo per ſe in a reverberatory furnace*.

As my moſt accurate experiments were tried on the reſiduum obtained in the latter mode, I will begin by giving the account of that experiment, and the means I uſed to inſure againſt deception; though I had already drawn concluſions from former ones, of which this might be conſidered only as a correct repetition. I took half an ounce of indigo, and expoſed it in a wedgewood retort, to the heat of a reverberatory furnace for ſeveral hours, and joined it to a pneumatic apparatus for receiving gaſes; I got none, though the heat was very intenſe, but in the receiver there was an alkaline liquor (mixed with a brown oil or tarry matter) which was evidently carbonate of ammoniac, as I aſcertained, by the muriatic acid. I once conſidered this as taking its origin from the pruſſic acid, but was afterwards undeceived, as will be obſerved in the ſequel. The reſidue in the retort apparently conſiſted of a charred matter, which other chemiſts would have calcined in the open air; but as I knew previouſly that it contained the volatile matter (which, except in rapid combuſtion, generally falls down by its own gravity) and the properties of which I was already pretty well acquainted with, I ſubmitted the whole to digeſtion in muriatic acid, after examining it minutely; when I obſerved various apparently metallic leaves interſperſed through it, ſome of which I have preſerved, and, along with the needles, have put into the object ſliders of a microſcope.

The muriatic acid did not act on the leaf otherwiſe than by brightening its appearance, which was before blackiſh, and now took the golden hue, that was obſerved on that procured by phoſphorus, &c. It was not at all acted on by the magnet; (the muriatic acid muſt have taken up both the iron and lime that is uſually found in indigo) neither does alcohol act on the needles, or leaf; conſequently, they can have nothing in common with reſinous or bituminous ſubſtances, neither does cauſtic alkali act on them, though boiling, which precludes the idea of their being a concrete acid; no combination of iron with phoſphorus, or muriate of ammoniac can reſemble them; as ſiderite has neither their volatile properties, nor has phoſphorus ever been found in indigo; beſides, that its combination with iron gives a purple with pruſſiates; a very different effect from that which the ſolution of the needles preſent with thoſe ſalts.

Neither have they any reſemblance to martial flowers, which indeed would be decom-

* As I do not wiſh to take the credit of any thing that may be conſtrued to belong to another, I will here mention, that I have read in O'Brien's calico printing ſince I made theſe experiments, that "the curious may ſublime indigo, and thereby procure flowers, as with zinc, ſulphur, &c." Whether this gentleman ever procured the needles I have mentioned I know not, or what meaning he annexes to flowers of indigo, though I know indigo may be ſublimed in the blue ſtate by a gentle heat.

 poſed

pofed by the muriatic acid, nor to the Duc D'Ayen's fublimate; which, befides requiring a great heat, was attracted by the magnet.

Neither does fulphuric acid act on this fingular product of indigo, though concentrated and boiling. Nitrous acid only diffolves it, which it does rapidly; and on on that folution my experiments, further to afcertain its quality, were performed.

The muriatic acid being wafhed off, the carbonaceous refiduum, (the laft of which gave a faint blue with pruffiates) the leaves were diffolved in nitrous acid, which gave no fymptom of containing iron; and I found alkalis invariably precipitate this folution white; unlefs, as will afterwards be fhown, that the metallic matter be converted into an acid, in which cafe ammoniac diffolves with it, and feems to have the ftrongeft attraction for it of any of them. When the muriatic acid has not been fufficiently wafhed from it, and it ftill retains a portion of iron, very pure pruffiate of lime forms with it a deep green colour, which no acid whatever has the effect of changing to blue; but the air and light make it yield a blue precipitate. I once confidered this to be the colouring matter of indigo regenerated, without the fixing refinous fubftance, and nearly in the fame ftate with what Bergman calls precipitated indigo, as a cauftic alkali turns it yellow; but I have fince difcovered that when fufficiently digefted in muriatic acid, the nitrous folution yields no colour whatever with pruffiate of lime. Galls, either in a watery or fpiritous infufion, give a white precipitate, and the abforbant earths the fame. By diftilling the nitrous acid from indigo, I procured Hauffeman's acid; which I found produced the fame effects, and is undoubtedly this volatile matter converted into an acid by oxigenation, in a manner analogous to many other inflammable bodies; this acid forms an infoluble precipitate with pot-afh, but has a ftronger attraction for cauftic ammoniac, which combination cannot be decompofed but by muriate of pot-afh. I found alfo that this acid formed a green with pruffiates, when not fufficiently purified from iron, which, when fpread on paper, the rays of the fun (to which it was expofed for above a day) changed to yellow, and not to blue. I may here remark, that indigo in fubftance, heated with wax or tallow, produces a pink colour, which ftained the ftick with which I ftirred the mixture, and that the nitrous acid changes it to a yellow, which I have obferved return to the green and the blue by expofure to the light. Thefe facts have caufed me to form an opinion, that this volatile matter is a more general colouring principle of organic bodies than might be fuppofed; and though at prefent I will not enter further on the fubject, I think I have traced it in other fubftances as well as indigo. Though I tried various other acids, both animal and vegetable, they had no peculiar effects on the above nitrous folution.

I now come to the fingular action, that nitrous acid, repeatedly diftilled from indigo, produces (as I fuppofe) on the refinous fixing matter, and which will be a teft to difcover that matter in any other body.

To twenty grains of indigo I added about two drachms and two fcruples of nitrous acid with two waters; after repeated diftillations, the refinous matter was nearly deftroyed, and the cryftals of the acid of Hauffeman were vifible on the fides of the retort; the liquor in

in the receiver was of a clear yellow, containing little or no nitrous acid, but yielding a ſtrong ſmell of bitter almonds; though it was moſt likely that the nitrous acid ſhould have decompoſed the pruſſic, (which I then thought I had reaſon to conſider as exiſting in indigo) yet after adding pot-aſh to it, I tried it with ſulphate of iron, and obſerved no appearances of blue by that teſt.

I could now conjecture it to be nothing elſe than laurel-water, of which it had every indication; I therefore tried it on a kitten, to which about two tea ſpoonfuls were adminiſtered, at three different times; after each doſe, it was ſeized with convulſions, which were perfectly obvious both to the gentleman above alluded to, (who did me the favour to aſſiſt at the experiment) and to myſelf; it, however, ſurvived the trial, owing, no doubt, to the dilution of the liquor, which was not above a third part of that obtained from twenty grains of indigo; and if the above ſhould be proved true, by repeated experiments, it ſhows, that the products of organized bodies may be imitated by art, a principle that has been denied by many celebrated philoſophers.

I will now conclude with ſome general remarks on the effects of certain ſubſtances on indigo itſelf.

My chemical friend, to whom I have been ſo often obliged in the courſe of theſe experiments, expoſed indigo to the action of oxigenated muriatic acid gas, which has the effect of changing it to the green, in contradiction to the theory that ſuppoſes indigo to derive its blue from oxigen, and its green from the loſs of it. The nitrous acid turns it yellow, as has been often remarked, and which yellow I have obſerved, when the acid has been weakened, returns to the green and blue by expoſure to light. He alſo burnt a quantity of indigo in oxigenous gas, ſetting fire to it by means of a lens and the ſolar rays; the reſidue was a browniſh white ſubſtance, which afforded the ſame ſolutions and appearances with the other reſidues of the indigo.

I have all along found that bodies capable of affording oxigen, have made indigo paſs firſt from the blue to the green, and afterwards to the yellow, and in this laſt inſtance to the white, when the bituminous matter was deſtroyed by rapid combuſtion.

I have it in contemplation to repeat moſt of Hauſſeman and Bancroft's experiments, and expect to be able to prove, that when indigo is revived from its ſolution in an alkali, an acid is always preſent, or formed, which renders the alkali incapable of diſſolving the indigo, by combining with it.

I will only beg leave to mention here one concluſion, drawn from Hauſſeman's experiments, which is clearly erroneous.

He expoſed a ſolution of indigo by alkali and orpiment in carbonic acid gas; and got the ſame effect as in oxigenous, viz. the abſorbtion of the gas and the revivification of the indigo: if I have not miſtaken Dr. Bancroft, he brings this as an argument in favour of indigo receiving the blue colour by abſorption of oxigen, which can only be ſo, on the ſuppoſition that that mixture can decompoſe carbonic acid, though it does not appear that any other matter has that power than phoſphorus, and that only when a ſtrong heat is applied.

Whatever

Whatever may be the fate of the foregoing experiments, whether they may be refuted or not, partially, it is clear that the metallic appearance has never before been fo decidedly feparated from indigo, and probably it is an univerfal principle in nature that may occafion thefe golden and filvery hues vifible on many infects, particularly fire flies, which are fuppofed to give out phofphorated hydrogenous gas, and alfo in feveral kinds of fifhes.

On the whole, my opinion is, that indigo confifts of a peculiar volatile matter fui generis, capable of becoming blue by a fmall portion of oxigen, (though that is only rendered probable by the above experiments) combined with a refinous or bituminous fubftance convertible by the nitrous acid into laurel-oil and gazeous products.

I am, Sir, with equal refpect for your character,
as a man and as a philofopher,
your moft obedient fervant,
A FRIEND TO THE EXPERIMENTAL RESEARCH OF TRUTH.

P.S. I forgot to mention that I have tried to procure both the needles and leaf from Pruffian blue by the fame means that I ufed with indigo, but could produce no fuch effect.

Since I finifhed this account, I have repeated one of Dr. Bancroft's experiments, or at leaft on the fame principle with his, and one of thofe that could be moft decifive with regard to his theory. I took a cauftic alkali of the fhops, and treated it again with lime, till it gave almoft no effervefcence, with the fame fulphuric acid, diluted with about eight waters, that I ufed in the latter part of the experiment. I took a drachm of indigo, of a kind that gave no effervefcence with that acid, and two drachms of brown fugar, which did not effervefce either. I boiled thefe with two ounce meafures of the above alkali; in a little time the liquor paffed from the blue to the green and yellow; I then ftopped the procefs, and after trying if it effervefced with the acid, which I found it fcarcely did, I threw a quantity of it up into a jar of atmofpherical air above Mercury, whilft it was almoft boiling; when cold, I marked the height of the liquor; after ftanding two days, it had rifen about two-thirds of an inch in a jar about five and a half high, and, except at the bottom, the indigo was regenerated in the blue ftate; I then threw up a quantity of the above diluted acid, and had a copious effervefcence, and the furface of the liquor, below the air bubbles, defcended to the mark: this was evidently carbonic acid; but to make the matter certain, I injected lime water, fo that it touched the fides of the jar; when at firft its appearance was pellucid, and afterwards a white precipitate of carbonate was evident on the fides of the jar. In attempting to transfer the jar to the water apparatus, I had allowed a good deal of atmofpherical air to mix with the refidual gas, fo had no farther opportunity of examining it. I am fenfible that this experiment ought to be repeated with accuracy, efpecially fo as to determine the real quantity of carbonic acid, which was undoubtedly formed from the oxigen of the atmofpherical air, and carbonaceous matter either in the fugar or indigo.

New

II.

New Refearches into the Affinities of which the Earths exert upon each other in the humid and dry Way. By Citizen GUYTON.

(Concluded from page 422.)

Experiment 16.

BARYTES and ftrontian, in the fame circumftances, fhewed nodifpofition to unite.

Experiment 17. Strontian was prefented to alumine by means of the fame folvent. The mixture affumed a milky colour, and afforded a precipitate, which the acid did not rediffolve.

In fupport of thefe experiments, I may refer to thofe of Citizen Vauquelin, in which he has fhown the action of barytes and ftrontian upon alumine *. This property is here afferted only in the due humid way as well as with regard to lime. They do not determine the vitrification of alumine. My experiments on this fubject perfectly agree with thofe of Macquer, Ehrman, Achard, and Kirwan †.

We are indebted to the illuftrious Scheele, in his Differtation upon Quartz, Alumine, and Lime ‡, for the firft obfervation refpecting the combination of alumine and lime. I have repeated the experiment which proved to him the poffibility of this union, and I obferved, as he did, that the product was a peculiar kind of earth, compofed of alumine and lime, when alum was decompofed by a more confiderable quantity of lime water than was neceffary to precipitate the whole of the alumine.

The confequences to be drawn from thefe experiments naturally prefent themfelves. Two earths held in folution by the fame fluid, water for example, if they be foluble in that fluid or alkali if this folvent be neceffary, or otherwife in one and the fame acid. Thefe exercife a mutual action, which tends to unite them, and feparate them from the folvent, which, according to the laws of elective attraction, is effectual with regard to fome earths, and ineffectual with others.

Thus it is, for example, that in the 1, 5, 10, 11th experiments, barytes and ftrontian act very differently with lime. A confiderable refemblance in the refults of the fame earths, in different folvents, may alfo be remarked. Laftly, in the 10th experiment upon the earthy muriates, there are only four in which the mixtures did not give manifeft tokens of the divellent affinities; and it may perhaps be proper to exclude from the lift of exceptions the muriates of magnefia and alumine, which conftantly affumed a milky tinge.

It muft not however be concluded, that no affinity exifts where there is no decompofition, or that it cannot even prove divellent in fuch cafes; that is to fay, by changing the

* Annales de Chimie, XXIX. 270, or Philof. Journal, III. 122.

† See the Journal Polytechnique. Cah. 3. p. 307, and Kirwan's Mineralogy, I. 56.

‡ See the French edition of his Effays, p. 96, &c.

common

common ſolvent of the two earths. I think it the more proper to inſiſt on this obſervation, becauſe magneſia and alumine themſelves afford a ſtriking example. If they be precipitated together from their ſolution in the muriatic acid by pot-aſh, they contract an union ſufficiently intimate to preſent the character of a peculiar earth, inſomuch that pot-aſh no longer acts upon the alumine. This fact, which is of great value with reg[illegible] preſent queſtion, was obſerved by citizens Clouet and Hachètte in a ſeries of experiments, in which they engaged for the purpoſe of aſſiſting me to fill up certain blanks in a table, in which I propoſed to give all the direct combinations of ſimple or undecompoſed ſubſtances.

§ 2. *Experiments in the dry Way.*

With regard to the affinities of earths to each other in the igneous fluid, or in the dry way, ſome of the facts which ſupport it have been known long before the true cauſe was ſuſpected. The reſults which relate to the property called fuſibility were alone perceived and no farther reſearches were made to explain what daily happens in the workſhops of manufacturers, I mean the vitrification of lime and ſilex by each other.

Three years ago I announced Nos. 35 and 39 of my experiments on the fuſibility of the earths by their mutual attraction, that equal parts of barytes and ſilex afforded in the crucible a very diſtinct vitriform product, and that equal parts of barytes and lime afforded a tranſparent glaſs *. After having mentioned theſe two reſults, I ſhall confine myſelf to add in this place thoſe of ſome new experiments, directed particularly to the ſtrontian and jargon earths.

Experiment 18. Strontian and ſilex mixed in equal portions, and placed in a ſaucer of platina in a fire of 140 pyrometric degrees, afforded a fine white frit, nearly equal in appearance to enamel, which ſlightly ſcratched glaſs. A few ſmall tranſparent vitreous globules were ſeen, and ſome adhered to the veſſel of platina.

Experiment 19. The mixture of ſtrontian and lime alſo placed upon platina, after having undergone a heat of 153 degrees, left a hard white frit, porous beneath with bubbles, without adherence to the veſſel; likewiſe preſenting ſmall vitreous globules, and ſome knobs of enamel.

Experiment 20. The jargon earth treated in the ſame manner with ſilex, produced by heat of 140 degrees a yellowiſh grey frit, pulverulent at its ſurface, with ſome white vitreous points of a beautiful tranſparence ſtrongly adhering to the platina, and ſo hard as to cut glaſs.

Experiment 21. The mixture of jargon earth and lime afforded in the ſame circumſtances a grey frit of little ſolidity, or even granular, and one ſmall globule of enamel adhering to a point of the crucible of platina.

Theſe facts are doubtleſs ſufficient to prove, that theſe earths have alſo a ſufficient affinity to each other to produce vitrification, when the due proportion ſhall have been de-

* Journal Polytechnique 3 cahier, pag. 306 & 308.

termined

termined by repeated trials. But before I draw any more general conclufions, I muft make a few reflections on the manner in which we ought to confider this action of the earths upon each other.

§ 3. *Refpecting the Manner in which thefe Phenomena ought to be viewed.*

Every chemift muft be ftruck with the various points of refemblance which many of thefe facts feem to eftablifh between the alcalis and fome of thefe earths, and may be tempted to think, that they might with equal propriety be placed among alkaline as earthy fubftances, particularly if lime be joined with barytes and ftrontian.

In fact thefe three earths have an acrid tafte like the alkalis; the firft in the higheft degree, the fecond rather lefs, and the third in a degree ftill lefs, but fo characteriftic, that it is from this that our firft idea of caufticity has been derived.

Thefe earths are foluble in water nearly in proportion to their caufticity; but it is the effect, and not the dofe which forms the character; otherwife we fhould have as many claffes as bodies, which had not all their properties alike in the fame degree of power and proportion of effect.

The folutions of the two firft afford with eafe and in abundance very diftinct cryftals; it is difficult to obtain a very regular cryftallization of lime; but this folution, like that of the others, depofits part of its earth by cooling in the folid form. It is a true cryftallization; for cryftallization is not conftituted either by tranfparency, or the vifible regularity of form. There is a real cryftallization whenever a fluid body paffes to the concrete ftate by the rapid or flow abftraction of the fluid which gave it its form, or which is the fame thing, which held it in folution. It is ufual and very proper in the defcription of chemical phenomena, to diftinguifh cryftalline depofitions from thofe which are pulverulent; but this is done to diftinguifh them, and by no means to point out a different nature. If this were not true, we ought to reckon the metals uncryftallizable, becaufe they are without tranfparence, or becaufe they exhibit only a rough exterior, when too fudden cooling prevents us from feeing the fymmetric arrangement of their particles. We might alfo affirm, that the falts which afford the moft beautiful cryftals, do not in fact cryftallize when the water of their folution is fuddenly abftracted by alcohol. Barytes itfelf here affords an example, for its aqueous folution is precipitated by alcohol in the pulverulent form. Here the cryftallization, inftead of being determined by the figure and attractive forces of the firft particles, depends no longer on thefe circumftances, but on the mode of feparation of the diffolving fluid, and all the accidents which may render its products, externally, of greater or lefs regularity with regard to our perceptions. If it were neceffary, I might fupport thefe reflections by an experiment, which is the reverfe of the foregoing, refpecting barytes. As there are circumftances in which it paffes to the concrete ftate without any vifible cryftallization, fo on the other hand I have afcertained, that there are circumftances in which lime may be brought to the cryftalline ftate. For this purpofe, it is neceffary only that lime water fhould be diftilled in a glafs retort, and that new lime

 water

water ſhould be added ſucceſſively for four or five times before the diſtillation be urged to dryneſs; after which the veſſels are to be left to cool without diſturbance. At the bottom of the retort will be found ſmall grains, exhibiting very perceptible faces and angles, and which muſt not be confounded with the pellicle uſually formed by the carbonate of lime.

Barytes, ſtrontian, and lime alſo poſſeſs in common with the alkalis, the property of giving a green colour to the ſyrup of violets, a red to curcuma, and of reſtoring blue colours, which have been reddened by acids; and this, in the language of accurate chemiſtry, implies nothing more, than that theſe ſubſtances have a ſtronger affinity to the acids, than is poſſeſſed by the colouring matter itſelf. I have elſewhere ſhewn, that tin and iron likewiſe reſtore the blue colour by ſeizing the acid *.

From the experiments before related, it follows that the three earths have, as well as the alkalis, a decided affinity with ſilex and alumine, as well in the dry as in the humid way; ſo that lime, takes ſilex from pot-aſh, and ſeizes the alumine at the inſtant it is abandoned by the ſulphuric acid.

The action which theſe earths exerciſe upon the oils, the ſoaps, and animal matters, the union they contract with the Pruſſian colouring principle, ſulphur, and the acids, form ſo many new points of reſemblance with the alkaline ſubſtances.

Theſe numerous reſemblances cannot be diſputed, and appear at firſt ſight to demand a common claſſical denomination, but does this opinion agree with ſound principles; are there not alſo facts which oppoſe it. It will be proper to examine this point.

All bodies have common properties more or leſs numerous; ſo that in order to denote their qualities rigorouſly, it will be proper to admit two claſſes only, or to diſtinguiſh them under two relations; the firſt comprehending ſimilar bodies, or thoſe which have all their properties alike, and the latter thoſe which have not this condition, or poſſeſs diſtinct properties.

In the ſecond place, the properties of bodies are almoſt invariably the different degrees of the ſame effect. We call thoſe ſubſtances fuſible, which flow without requiring an intenſity of heat greater than that of our common furnaces; we ſay that a ſubſtance is ſoluble, when it diſſolves in water to a certain quantity; that ſuch a body is fixed, becauſe it does not riſe in vapour, but with great difficulty, and in the ſame manner we ſpeak of other properties. The limit of our claſſes is always the reſult of ſuch compariſons; and as we can ſcarcely find two ſubſtances which ought to be placed abſolutely on the ſame line, it follows that we muſt either renounce claſſification, or give a certain extent to our limits, by means of which ſimilar effects, though very diſproportioned, may ſerve by their connexion to determine the moſt eſſential characters.

A long ſucceſſion of uſeleſs efforts ought to convince us, that nature has prepared nothing for our methods; but this is not a reaſon why we ſhould deſtroy this inſtrument of

* Recherches ſur la matiere colorante des vegetaux, &c. Ann. de Chemie, tom. xxx. p. 185.

ſcience.

ſcience. It may be denominated a falſe key, but nevertheleſs, ſuch as may aſſiſt us, in our approach certain objects which we ſhould not otherwiſe behold, but confuſedly and at a diſtance.

A third truth, which has certainly ſtruck all thoſe who have meditated upon the true object of a methodical claſſification, is, that the properties which exclude, are very differently characteriſtic from thoſe which bring objects together. This ariſes from the circumſtance, that in the confuſed accumulation of circumſtances of reſemblance, frequently ill determined, we find no baſis of ſufficient ſolidity to aſcertain our diviſions, except in the oppoſitions preſented by ſuch expreſſions as ſubſtances decompoſed or not decompoſed, combuſtible and incombuſtible, ductile and not ductile, ſoluble and inſoluble, and the like. Theſe principles being eſtabliſhed, we have here to conſider the facts of both kinds; thoſe which exhibit an analogy between other ſubſtances and alkalies, and ſuch other facts as form oppoſitions between them.

Among the former it is remarked, that magneſia may unite with ſulphur; that magneſia and alumine produce the ſaponaceous ſtate in oils; that the metallic oxides alſo form ſoap; that the oxides of lead and iron * produce the ſame effect as the alkalis in the vitrification of earths, particularly ſilex, &c. &c.

With regard to the facts which eſtabliſh an oppoſition, and which we have obſerved to be the moſt concluſive, it is ſufficient to obſerve, that the alkaline carbonates are ſoluble in water, but thoſe of the three earths are not; that the ſame difference exiſts between the alkaline and the earthy ſoaps, in which reſpect the latter have a much greater reſemblance to the metallic ſoaps; that the fixity of the earths is much greater than that of the alkalies; and to conclude, by a ſtill more ſtriking fact, the alkalies are ſoluble in alcohol, and the earths inſoluble. If a ſufficient quantity of alcohol be poured into an aqueous ſolution of barytes, the liquor is ſoon rendered turbid by ſmall white flocks, which in a few minutes fall to the bottom of the veſſel, and leave the fluid no longer capable of altering the colours of culcuma and fernambouc.

From the experiments and obſervations contained in this memoir, I think the following concluſions may be drawn:

There is a tendency to union exiſting between all the earths, as well in the humid as in the dry way, which according to the degree of elective attraction, determines their precipitation from a common ſolvent and their vitreous compoſition.

The union of two earths operates like the alloy of two metals, by virtue of the ſame law, which excludes the ſuppoſition of a property in one of the two bodies which may belong to another order.

By comparing the reſults of theſe attractions to the ſolutions, by any ſaline ſubſtances whatſoever, alkaline or acid, we ſhould be often embarraſſed to point out which of the

* Mr. Kirwan has ſhewn, that the oxide of iron, in a double or triple proportion, cauſes ſilex, and even magneſia to flow; that alumine mixed with two parts of oxide of iron is fuſible at 160 degrees of the pyrometric ſcale of Wedgwood. Mineralogy I. 71.

earths act on each other in the manner of theſe ſolvents, becauſe we find lime take ſilex from pot-aſh; this laſt yields alumine to magneſia, and lime vitrifies barytes, as barytes vitrifies ſilex.

Laſtly, the phenomena which may give riſe to reſemblances of this kind, ought not to be conſidered but as the effects of a more general cauſe; that attraction of near bodies, which in the inequality of its relations forms the bond of natural combinations, and the power of the inſtruments employed by chemiſtry to diſunite them.

III.

Concerning the Influence of the Moon upon the Atmoſphere of the Earth. By CIT. LAMARK *.

THE moon has certainly a great influence on the ſtate of the atmoſphere of the earth. For if that univerſal gravitation which produces the attraction between the moon and the earth, can occaſion the flux and reflux of the ſea, how can it be denied, that the ſame cauſe ſhould not produce a kind of flux and reflux of the atmoſpheric air, continually diſplaced by the ſucceſſive changes in the diſtances and poſitions of theſe bodies which gravitate towards each other? This influence of the moon on the atmoſphere of the earth has never been doubted; but no perſon has yet, to my knowledge, examined the nature of that influence, in a manner ſufficiently preciſe, to exhibit its true effects. Philoſophers have been too deſirous of diſcovering in certain aſpects, namely, the ſyzigies and quadratures, the indicating points of the changes which this planet inceſſantly produces in the terreſtrial atmoſphere.

I have directed my obſervations for a great number of years to examine the variations in the ſtate of the atmoſphere, in order to diſcover, if poſſible, the principal cauſes, eſpecially thoſe which act moſt regularly in theſe phenomena; and I have at length ſucceeded in diſcovering the following principles:

1. It is in the elevation and depreſſion of the moon, above or below the equator, that we muſt ſeek for the cauſes of thoſe regularly variable effects which it produces on our atmoſphere.

2. The determinate circumſtances which augment or diminiſh the influence of the moon in its different declinations are, the apogees and perigees of this planet, its oppoſitions and conjunctions with the ſun, and laſtly, the ſolſtices and equinoxes.

It is well known that after every time that the moon croſſes the equator, it remains for about fourteen days in the hemiſphere it has entered, whether auſtral or boreal. Every lunar month therefore preſents one revolution of the moon in the zodiac, which may be divided into two diſtinct parts, and which produce or cauſe two particular atmoſpheric conſtitutions. I call one of theſe the boreal conſtitution, namely, that during which the

* Soc. Philomath. No. 15, p. 117.

moon

moon passes through the six northern signs of the zodiac, and I call the other the austral constitution, because the moon passes through the six southern signs, during this period.

I am convinced by observation, that during a boreal constitution, the winds which chiefly prevail in this climate blow from the south, south-west, and from the west. Sometimes in summer they pass to the south-east. In general the barometer during this constitution, presents but moderate elevations in the column of Mercury. The weather is usually rainy or damp, and the air is loaded with numerous clouds. And lastly, it is particularly during this constitution that we observe the effects of storms and tempests, when the causes which occasion them become active

On the contrary, during an austral constitution, the winds which chiefly predominate, blow from the north and north-west, and in summer north-east, and even easterly winds. In general during this constitution, the barometer exhibits considerable elevations in the column of Mercury, at least if the wind is not very strong; the weather is then most usually clear, cold and dry, and in the summer it is seldom (I might almost say never) during this constitution that storms are formed.

These two atmospheric constitutions are not however so permanently characterised as to render it easy to distinguish them at all times by the state of the atmosphere. The atmospheric air is so moveable a fluid, and so easily displaced, that it is not surprising that in the temperate zones where the influence of the heavenly bodies acts less strongly, than between the tropics from various causes, that they counteract very often the regular influence of the moon, and tend to mask and even change its effects *.

The perturbations which these variable causes produce on the regular effects of the influence of the moon on the atmosphere, occasion in fact many variations in the two atmospheric constitutions I have been describing; and this is doubtless the reason why they have been hitherto disregarded. But I can positively assert, that these perturbations, though frequent, and sometimes very considerable, do not prevent the character of each of these constitutions from being remarked in the greatest number of cases.

The probability that I have found is, according to my observations, estimated as 5 out of 8; that is to say, out of 48 atmospheric constitutions comprehended in the lunar year, I estimate there will be found at least 30 agreeing with the principles pointed out in this memoir; and I must add, that among the disturbing causes which modify the before-mentioned effects, several may be foreseen, and perhaps even appreciated as to their quantity of effect.

It is not an opinion that I here offer, it is a fact I announce;—an order of things that I point out, and that any one may verify by observation. It appears to me to be altogether unnecessary to give a detail in this place, of all the reasons which might be adduced to prove the great utility of this knowledge.

LAMARK.

* To avoid lengthening this extract, I pass over in silence the enumeration and developement of such of the variable causes as I have ascertained.

Directions

IV.

Directions for making the beſt Compoſition for the Metals of reflecting Teleſcopes, and the Method of Caſting, Grinding, Poliſhing, and giving the great Speculum the true parabolic Figure. By the Rev. JOHN EDWARDS, B. A*.

THE methods in general uſed for caſting, grinding, and poliſhing the metals for reflecting teleſcopes being well known to workmen, and having been treated of in the moſt full and ſatisfactory manner in Dr. Smith's optics, and alſo by Mr. Mudge, in the Philoſophical Tranſactions, Vol. LXVII. Part. I. I ſhall not dwell upon theſe points, but ſhall add ſuch directions and obſervations of *my own*, as I have found by experience to anſwer much better than the method taught by thoſe writers. Some teleſcopes conſtructed by me have been tried by the Rev. Dr. Maſkelyne, Aſtronomer Royal, and found very greatly to excel in brightneſs †, and to equal in other reſpects teleſcopes of the ſame ſize, conſtructed by the beſt artiſts in London.

Of the beſt Compoſition for reflecting Specula.

That I may not be tedious upon this point, it may be neceſſary to acquaint my reader, that I have made experiments upon the following metals and ſemi-metals, in order to diſcover a compoſition for a ſpeculum which ſhould reflect the greateſt quantity of light, and conſequently be capable of receiving the fineſt poliſh. I combined them in ſeveral proportions, and ground and poliſhed them. The metals and ſemi-metals I tried were ſilver, platina, iron, copper, braſs, lead and tin, crude antimony, regulus of antimony, martial regulus of antimony, arſenic, biſmuth, zinc, and antimony combined with cawk-ſtone ‡. Having tried many compoſitions of them, (ſee the Appendix) I found that 32 ounces of copper, with 15 or 16 ounces of grain tin (according to the purity of the copper) with the addition of a little § braſs and arſenic; viz. one ounce of each to the above proportion of copper and tin, will form a metal, capable when poliſhed in a proper manner, of reflecting much more light than any other metal, that has as yet been offered to the public. When I

* From the Nautical Almanac for 1787, inſerted at the requeſt and ſuggeſtion of a correſpondent, who obſerves, that the almanac for that year has become very ſcarce in conſequence of the demand for this treatiſe, which is not elſewhere to be found.---N.

† Mr. Edwards' teleſcopes ſhew a white object perfectly white, and all objects of their natural colours; very different from common reflecting teleſcopes, which give a dingy copperiſh appearance to objects. I found by a careful experiment, that they ſhew objects as bright as a treble object glaſs achromatic teleſcope, both being put under equal circumſtances of areas of the aperture of the object metal and object glaſs, and equal magnifying powers; whereas the diameter of the aperture of a common reflecting teleſcope muſt be to that of an achromatic teleſcope as 8 to 5, to produce an equal effect.

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‡ See a moſt curious experiment upon cawk ſtone and antimony in the Philoſophical Tranſactions, No. CX.

§ If one ounce of ſilver be added to this compoſition, the metal will be much better and whiter.

ſay

ſay that the proportion of tin is from 15, or 16 to 32 ounces of copper, I would be underſtood, that the proportion of tin will not always be accurately the ſame, as copper will take more or leſs tin to perfectly ſaturate it, according to its purity. It might be of uſe previouſly to purify the copper as much as poſſible. A very little experience in theſe matters, will enable any one to know exactly when the copper is compleatly ſaturated; as the compoſition will, if broken, appear of a moſt beautiful bright and gloſſy nature, very much reſembling the fine face of quickſilver. My method to aſcertain that point accurately, is to melt 32 ounces of copper, and to add to it when ſufficiently fuſed, 15 ounces of tin, and to pour the mixture into an ingot; then to a certain known portion of this compoſition, I add a very ſmall, but known portion of tin; and thus by a few trials I can eaſily find the point of complete ſaturation, and the maximum of perfection. Having then aſcertained what portion of tin I added to the above known quantity of the compoſition, I add the proportional quantity of tin to the whole when melted a ſecond time. Thus if I find that I muſt add a quarter of an ounce of tin to one pound of the compoſition, ſo as to obtain the *ne plus ultra* of brilliancy *, then I know that when I ſhall melt the remainder of the metal a ſecond time, in order to caſt the ſpeculum, I muſt add one of grain tin to four pounds of the compoſition, made according to the proportion of 32 ounces of copper to 15 ounces of tin. The arſenic muſt be added in the *ſecond melting*, when the ſpeculum is intended † to be caſt, as the heat of the mixture in the *firſt* melting is ſo great, as to render the moſt part of the arſenic volatile, and in a great meaſure prevent its action upon the metals. It is ſomewhat ſingular that arſenic, though particularly recommended by Sir Iſaac Newton ‡ for this purpoſe, ſhould be haſtily thrown aſide by the founders, as well as paſſed over unnoticed by the writers upon this ſubject. This imprudent diſuſe I can only attribute to the diſagreeable fumes, or vapours, which ariſe when it is introduced into the crucible to the melted mixture, which may produce diſagreeable effects upon the operator, if proper care is not taken to prevent them from being received into the lungs §. All the precaution neceſſary is, to bruiſe the arſenic coarſely, and introduce it into the crucible with a pair of tongs, having tied it up in a piece of paper; give it then a ſtir with a wooden ſpatula, retaining your breath, avoid it till you can ſee no more vapours ariſe from the crucible, when the metal will be ready to be poured into the flaſks to caſt the ſpeculum. The great uſe of arſenic in this compoſition, is to render

* If too much tin ſhould be added; viz. if 17 ounces of tin is put to 32 ounces of copper, the compoſition is not *brilliant* when broken, but of a *grey blue* and dull colour. If the quantity of tin be further increaſed, the metal will be almoſt black.

† Sir Iſaac Newton melted the copper firſt, then added the arſenic, and laſtly the tin; as without doubt, he knew that the tin ſhould remain in a fluid ſtate the ſhorteſt time poſſible. It is true that Sir Iſaac Newton added the arſenic to the melted copper; but as he well knew that a great part of it would be rendered volatile, he therefore added a very large quantity of it; viz. arſenic 1 to copper 6.

‡ See Dr. David Gregory's optics, by Dr. Brown and Dr. Deſaguliers, p. 219, or Philoſophical Tranſactions, No. LXXXI.

§ I have been aſſured, by two ingenious experimental philoſophers, that the fumes of arſenic, even when the garlic ſmell is very ſtrong, are not in the leaſt prejudicial to the lungs.

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the metal much more compact and ſolid, and indeed much more *beautiful*, as one may experience by comparing the compoſition *with* arſenic, with the ſame compoſition of copper and tin *without* arſenic. In general I find one * ounce of arſenic ſufficient for one pound of the metal. A much greater quantity of arſenic may be uſed, without any diſadvantage to the beauty or compactneſs of the metal; but then it is too apt to tarniſh if expoſed to the air for ſome time: three quarters of an ounce, or an ounce of arſenic, to one pound of compoſition, will not tarniſh in the leaſt degree. Indeed, the reaſon why the metals generally made uſe of for ſpecula, tarniſh when they are much expoſed to the air, is becauſe the quantity of copper in their compoſition is not nearly ſaturated, and the acid contained in the air, by acting upon it, extracts the copper from the tin, and turns the metal into a dirty or dingy colored ſpeculum, and which (beſides the great loſs of light) cauſes the common reflecting teleſcopes to ſhew all objects of a dirty red, or yellowiſh colour. This, however, is not the caſe in the metals made of the above compoſition; for as the copper is compleatly ſaturated, the air cannot act upon it in the leaſt degree. I muſt not, however, paſs over one caution in the mode or manner of melting the compoſition; and that is, that the copper muſt be melted firſt of all, and rendered as fluid as poſſible, then the braſs and ſilver muſt be added, and the whole fluxed with the common black flux, made of two parts of tartar to one of nitre, or by ſtirring the melted mixture with a wooden ſpatula of birch, and made as fluid as poſſible. The tin muſt now be added, and the whole poured off immediately, after it is once ſtirred together; for if the mixture is *continued* on the fire ſome time after the tin is added to it, it will always prove porous afterwards, though it be melted a ſecond time with the ſmalleſt heat poſſible. As I ever found this to be the caſe, I naturally conjectured, that the metal would be moſt ſolid and free from pores, when the tin remained the leaſt time poſſible in a ſtate of calcination. Experience determined the truth of my conjecture, and I now find, that the beſt method poſſible to make this compoſition to the greateſt advantage, is to melt the copper as fluid as poſſible, and flux it with the black flux; to melt the tin in a ſeparate crucible by itſelf; to take the two crucibles out of the fire, and pour the melted tin into the fluid copper; and ſtir it inſtantly with a wooden ſpatula, and pour the whole immediately into a large quantity of cold water. The ſudden chill from the cold water divides the melted maſs into an infinite number of ſmall particles, and by that means cools it inſtantaneouſly, and conſequently prevents the tin from calcining ſenſibly; and hence I have always found that in the ſecond melting, the compoſition was entirely free from pores, even though no *arſenic* had been employed. Yet the addition of *arſenic* ever rendered it much more compact, and indeed † ſpecifically heavier, as well as more brilliant and beautiful. On reverſing the proceſs, if the tin is put into the bottom

* One ounce of arſenic will, however, ſufficiently act upon and bind three pounds weight of the metal, ſo that it ſhall never tarniſh by the air.

† The ſpecific gravity of the compoſition itſelf is 8,78, with the addition of one ounce of arſenic to one pound of metal, is 8,89.

of the crucible, and the copper at the top of it, which I have frequently done, the copper will melt with a very little heat; whereas when copper is put into the crucible by itſelf, it requires a pretty ſtrong heat to cauſe it to melt. When I firſt made uſe of this method, I imagined I had diſcovered a very eaſy one to melt the copper, and conſequently I thought I had greatly improved the common method; and as Mr. Mudge aſcribed the pores in the metal to the tin being calcined by the great heat of the fluid copper at its firſt melting, I naturally expected to find the metal made by the above proceſs totally free from pores, eſpecially in the ſecond melting, as the heat was conſiderably leſs than if the copper had been melted firſt by itſelf. However, I always found it full of pores, much more porous than I had ever ſeen it before. For ſome time I could not diſcover the true cauſe, having no idea that the pores were owing to the tin remaining ſo long in the fire in a fluid ſtate, and therefore in a ſtate of perpetual calcination. I attributed the poroſity of the metals which I made of this compoſition to a multitude of cauſes, till, thoroughly tired of experiments and conjectures, to aſcertain the true reaſon, I was determined to melt the copper firſt, and the tin afterwards, as I had always done, before I dropped upon this improved method as I imagined. The reſult was, the metal was infinitely more compact, and much leſs porous. By melting the copper firſt, and then adding the tin to it, I ſoon diſcovered that the longer the tin remained in the fire, the more porous the metal turned out; and *vice verſâ*, the ſooner I poured it off, after I had added the tin, the more compact and free from pores it proved. From theſe obſervations, I determined to try the effect of adding the tin in a fluid ſtate to the melted copper, and to cool the whole immediately, to prevent, as far as I was able, calcination from taking place. Experience confirmed my conjectures; and I ſoon found, that by pouring the whole melted maſs the inſtant they were mixed and ſtirred together into cold water, the metal always proved in the ſecond melting ſolid, and much more compact, beautiful, and white, than I had ever ſeen it by any other proceſs. One thing I cannot paſs over, as it affords a clear proof of the uſe of *arſenic* in rendering the metal much more ſolid and compact, and conſequently more free from pores, than if no arſenic had been uſed in the compoſition. Whenever I made the compoſition by melting the copper and tin together, by putting them into the crucible at the ſame time, and melting them down together, the metal was always porous, as I obſerved before: however, frequently I melted it afterwards, and though I gave it no more heat than was barely neceſſary to melt it, yet if I added to this very porous metal, after it was melted, a ſmall quantity of arſenic, viz. one ounce to one pound of the metal, it was really aſtoniſhing to ſee how much better the metal turned out, being conſiderably harder than before, and incomparably leſs porous. I mention this circumſtance, which any one may eaſily try, to ſhew the very great advantage of uſing a ſmall portion of arſenic to render it more compact; and as Sir Iſaac Newton juſtly obſerves *, more *white* than before. The uſe of the ſmall portion of braſs in this compoſition, is to render it more tough, and not ſo exceſſive

* See Appendix to Gregory's Optics, p. 219, or the Philoſophical Tranſactions, No. LXXXI.

brittle as this compofition without the brafs would prove. A fmall portion of filver will make the metal much whiter, though if too much is added, it is apt to be porous. Having faid fo much relative to the compofition of the metal, which indeed is a capital article, I pafs on to

The Manner of cafting the Metal.

The fand moft proper for cafting this, and indeed any other metal, is a fine fand, with no more loam or clay mixed with it naturally than is abfolutely neceffary to make it tenacious enough to adhere together when properly moiftened. If too much clay is mixed by nature with the fand, it will always *blow* the metal in different directions, fometimes, indeed, to the great danger of the operator. On the contrary, if the fand does not contain a fufficient quantity of clay, it will not remain in the flafks, or take a proper impreffion from the pattern or model. The beft fand I could ever meet with for the purpofe of cafting fpecula is the common *Highgate* fand, (near London) generally ufed by the London founders. It fhould be as little wet as may be, and well beaten, but not too hard. The flafks fhould be at leaft two inches wider than the metal intended to be caft. If the fand is not of a fufficient thicknefs round the metal, it will inftantly become dry when the hot fluid metal is poured into it, and, confequently, will contract, and, of courfe, the fluid metal will run out of the flafks. A proper thicknefs of fand will, however, prevent this accident. The metal or pattern fhonld be made of brafs, or hard pewter, and muft be a little larger and thicker than the fpeculum intended to be caft from it, as the thing caft is always a little lefs than the pattern, owing to its contracting a fmall degree in cooling. A wooden pattern will not quit the fand near fo well as one made of metal; befides wood will always warp by the moifture of the fand, and, confequently, will give a falfe figure or form to the intended fpeculum. As the compofition I have given for the fpeculum is the *hardeft*, and, confequently, the moft brittle of any metal yet known, fo it is the moft difficult to caft. The common method of cafting other fpecula will not avail in the leaft degree here; and it was a very confiderable time before I found out a certain and infallible way to caft them free from faults or flaws in the face. In general, they cracked in the cooling, from the moifture of the fand. The only method poffible to caft them well (for indeed I have tried many methods) is to caft them with the face *downwards*. The ingate or git fhould be at the back of the metal, and at the very edge of it; its breadth, where it joins the metal, fhould be at leaft half the diameter of the metal, and its thicknefs muft be half the thicknefs of the metal at the edge; the upper part of the git fhould contain as much metal at leaft, or even more, than the fpeculum itfelf: I could give my reader fufficient reafons for every part of the procefs above directed, but I might be thought too tedious: fuffice it then that I inform him that he will find thefe directions will anfwer in practice; and I believe I can fay, that no one whatever can caft fpecula of this brilliant and brittle compofition by any other means than what I have now pointed out. When the pattern, with its ingate or git, is taken out of the fand, ten or a dozen fmall holes fhould be made through the fand at the back of the mould with a fmall wire, or common knitting

knitting needle, to permit the *air* to efcape as the metal is poured into the mould. I have found by experience that feveral fmall holes are infinitely better for that purpofe * than one large hole. When the metal is melted a fecond time, which muft be done with as fmall a degree of heat as poffible, add the proportional quantity of crude arfenic in coarfe powder, and ftir it well with a wooden fpatula; when the fumes are gone off, take the metal off the fire, take away the drofs, and add half an ounce or an ounce of powdered rofin, or equal parts of powdered rofin and nitre, in order to give the metal a good face; ftir it well with a ftick, and pour it immediately into the flafks. When the git is filled up with the fluid metal, ftrike the flafks gently, fo as to fhake or jog the metal in them in a fmall degree; this will prevent any flaws in the face from any air-bubbles being lodged there. When the metal has remained in the flafks for a few minutes, fo as to become entirely folid, open the flafks while the metal is red hot (it cannot crack in this ftate, though it is expofed to the air, as all metals are malleable when they are red hot) and take out the fpeculum with a pair of tongs, laying hold of it by the git, but take care to keep the face downwards, to prevent it from finking. Force out the fand from the hole in the middle of the mirror with a piece of wood or iron, and place the fpeculum in an iron pot, with a large quantity of hot afhes or fmall coals, fo as to bury the fpeculum in them a fufficient depth. If the fand is not forced out of the hole in the manner above directed, the metal, by finking as it cools, will embrace the fand in the middle of the fpeculum fo tight, as to caufe it to crack before it becomes entirely cold. And if the metal is not taken out of the fand, and put in a pot with hot afhes or coals to anneal it, I can affert, that the moifture from the fand will always break the metal. Let the fpeculum remain in the afhes till the whole is become quite cold. The git may be eafily taken off by marking it round with a common fine half round file, and giving it then a gentle blow. The metal is then to be rough ground and figured.

Of the rough grinding and figuring the Speculum.

In rough grinding, figuring, and polifhing the metal, two tools only are neceffary befides a common grind ftone. One chief reafon why workmen do not give a good figure to their fpecula is, becaufe by purfuing Dr. Smith's or Mr. Mudge's method, they ufe too many tools, which, in a great meafure, deftroy each other's effects. As nature always acts in the moft fimple manner, fo if we could always imitate her in this refpect, we fhould arrive at a much greater degree of excellence in moft of our mechanical purfuits. Befides the tools generally made ufe of by workmen are confiderably too large in diameter ever to give a direct and uniform figure. All the tools I make ufe of are a rough grinder, compofed of lead and tin mixed together, or elfe of pewter; this rough grinder ferves alfo for a polifher; this tool, with a bed of ftones or hones, are all that are neceffary. A bruifer (as directed by Dr. Smith and Mr. Mudge) is totally unneceffary, caufes confiderably more work, and,

* If feveral fmall holes are made for the air to efcape, the back of the metal will be caft much neater than if one hole only is ufed for this purpofe. Befides, when one hole only is ufed to let the air efcape, the metal is very apt to crack in that place, owing to the finking of the metal in cooling.

after all, is really detrimental. The beſt method I have ever found to rough grind the ſpeculum is to grind the ſurface of it quite bright upon a * common grindſtone, made nearly to the figure or focus of the ſpeculum by a gage. Take it then to a convex tool, made of lead and tin, or elſe of pewter, and grind the metal upon it with fine emery. This emery, however fine it may be, will break up the metal very much, but we can eaſily cure that proceſs, as I ſhall ſhew hereafter. This tool or rough grinder ſhould be made of an *elliptical form*, and not circular, (for a reaſon I ſhall point out hereafter) and of ſuch dimenſions that the ſhorteſt diameter of the ellipſe ſhall be equal in breadth to the diameter of the mirror, and the longeſt diameter of the elliptical tool ſhould be to the ſhorteſt diameter in the proportion of ten to nine accurately, for a reaſon to be mentioned hereafter. The manner of working or figuring the metal upon this tool, and indeed upon all the ſucceeding tools, is taught in Dr. Smith's Optics, or the before mentioned volume of the Philoſophical Tranſactions. I ſhall refer my reader to thoſe publications, as I only mean to give my own improvements. When the metal is brought to a true figure, it muſt be taken to a convex tool, formed with ſome ſtones from a place called Edgedon, in Shropſhire, ſituated between Ludlow and Biſhop's Caſtle. Theſe ſtones or hones are of a fine grain, and will eaſily cut the metal, and bring it to a fine face. Indeed, the blue hones † uſed in general by the opticians for this purpoſe will ſcarce touch the metal, and it will be a laborious undertaking to bring the metal to a fine face, ſo as to take out all the breaks up from the emery by the common blue hones. By means, however, of the above mentioned ſtones, they may be eaſily ground and truly figured. The bed of ſtones ſhould be of a circular figure, and but very little larger than the metal intended to be figured upon it, viz. about two-tenths of an inch, but not more for a ſpeculum of four or five inches in diameter. If the tool is made conſiderably larger than the metal, it will grind the metal perpetually into a larger ſphere, and by no means of a good figure: if the metal and tool are of the ſame ſize exactly, the metal will work truly ſpherical; but it is apt to ſhorten its focus leſs and leſs, unleſs the metal and tool are worked alternately upwards. It had, therefore, better me made a little ‡ larger than the mirror, when it will not alter its focus. Too much water ſhould not be uſed at a time upon the hone pavement, or the figure will be very bad, which may eaſily be ſeen by the face of the metal appearing of different degrees of brightneſs in different parts of it. When the metal is brought to a very fine face and figure by the bed of ſtones, it is ready to receive a poliſh: but before I ſhall give any directions with regard to the manner of poliſhing it, I muſt mention a circumſtance or two I had inadvertantly paſſed over. The metal

* The grindſtone may eaſily be brought to the form of the gage by holding the ſharp end of an iron bar againſt it while it is turned round, till ſo much is worn away from its ſurface as ſhall cauſe it to take the true curvature of the gage

† Should any one, however, make uſe of the common blue hones, he ſhould uſe as little water as poſſible when the metal is put upon them, as they will cut much better when barely wet, than if much water is uſed upon their face.

‡ About one-twentieth part greater in diameter.

muſt

muſt not be caſt too thick, or it will never take the parabolic figure intended to be given to it. The beſt proportion I have found for this purpoſe is, a metal of four inches and a half diameter, and eighteen inches focus, ſhould be four-tenths of an inch thick at the edge of it: the back of the mirror ſhould be convex to ſtrengthen it, and to cauſe it to ſpring and adhere to the poliſher uniformly. Its convexity ſhould be equal to its concavity on the face, that the metal may be every where of an equal thickneſs. The handle ſhould be made of lead of the ſame convexity and concavity as the metal, its thickneſs about double that of the metal, and its diameter three-fourths of that of the ſpeculum; it ſhould have a hole in the middle, with a copper or iron ſcrew on it, ſo as to put it together with the mirror, to which it is faſtened with pitch on a collar lathe, in order to ſmooth and finiſh the edge of the metal, which may be done by holding a fine file to it when in the lathe at the firſt, and afterwards one of the above mentioned ſtones.

(To be concluded in our next.)

V.

Early Developement of the Antiphlogiſtian Theory of Combuſtion. By ROBERT HOOKE.

To Mr. NICHOLSON,

London, January 7, 1800.

SIR,

MUCH pains have with juſtice been beſtowed on the publication and developement of the diſcoveries of John Mayow; but I think the prior claims of another man, of no leſs ability, ſeem to be overlooked. The *Tractatus quinque Medico-Phyſici* of Mayow were publiſhed at Oxford in 1674, and the licence of Lord Brouncker, firſt preſident of the Royal Society, for printing Hooke's *Micrographia*, bears date Nov. 23, 1664, which itſelf appeared in 1675. I ſend the following extract from page 103 of that work, and have no doubt but it will prove acceptable to the readers of your uſeful Miſcellany.

I am, &c. R. B.

From the experiment of charring of coals (whereby we ſee that, notwithſtanding the great heat and the duration of it, the ſolid parts of the wood remain, whilſt they are preſerved from the free acceſs of the air undiſſipated) we may learn that which has not, that I know of, been publiſhed or hinted, nay, not ſo much as thought of, by any; and that, in ſhort, is this:

Firſt, *that the air* in which we live, move, and breathe, and which encompaſſes very many and cheriſhes moſt bodies it encompaſſes, that this air is the *menſtruum*, or univerſal diſſolvent of all *ſulphureous* bodies.

Secondly,

Secondly, *that this action* it performs not till the body be firſt ſufficiently heated, as we find requiſite alſo to the diſſolution of many other bodies by ſeveral other menſtruums.

Thirdly, *that this action* of diſſolution produces or generates a very great heat, and that which we call fire; and this is common alſo to many diſſolutions of other bodies made by menſtruums, of which I could give multitudes of inſtances.

Fourthly, *that this action* is performed with ſo great a violence, and does ſo minutely act, and rapidly agitate the ſmalleſt parts of the *combuſtible* matter, that it produces in the *diaphanous medium* of the air the action or pulſe of light, which what it is I have elſewhere already ſhewn.

Fifthly, *that the diſſolution* of ſulphureous bodies is made by a ſubſtance inherent, and mixt with the air that is like it, if not the very ſame, with that which is fixed in ſaltpetre, which, by multitudes of experiments that may be made with ſaltpetre, will, I think, moſt evidently be demonſtrated.

Sixthly, *that in this diſſolution* of bodies by the air, a certain part is united, and mixt or diſſolved, and turned into the air, and made to fly up and down with it in the ſame manner as a *metalline*, or other body, diſſolved into any *menſtruums*, does follow the motions and progreſſes of that *menſtruum* till it be precipitated.

Seventhly, that as there is one part that is diſſoluble by the air, ſo are there other parts, with which the parts of the air mixing and uniting, do make a *coagulum* or precipitation, as one may call it, which cauſes it to be ſeparated from the air; but this *precipitate* is ſo light, and in ſo ſmall, or rarified, or porous cluſters, that it is very volatile, and is eaſily carried up by the motion of the air, though afterwards, when the heat and agitation that kept it rarified ceaſes, it eaſily condenſes, and commixt with other indiſſoluble parts, it ſticks and adheres to the next bodies it meets withal; and this is a certain ſalt that may be extracted out of ſoot.

Eighthly, that many indiſſoluble parts being very apt and prompt to be rarified, and ſo whileſt they continue in that heat and agitation, are lighter than the ambient air, are thereby thruſt and carried upwards with great violence, and by that means carry along with them not only that *ſaline concrete* I mentioned before, but many terreſtrial or indiſſoluble and irrarefiable parts, nay, many parts alſo which are diſſoluble, but are not ſuffered to ſtay long enough in a ſufficient heat to make them prompt and apt for that action. And therefore we find in ſoot not only a part that, being continued longer in a competent heat, will be diſſolved by the air, or take fire and burn, but a part alſo which is fixt, terreſtrial, and irrarefiable.

Ninthly, that as there are theſe ſeveral parts that will rarify and fly, or be driven up by the heat, ſo are there many others that, as they are indiſſoluble by the *aerial menſtruum*, ſo are they of ſuch ſluggiſh and groſs parts, that they are not eaſily rarefied by heat, and therefore cannot be raiſed by it; the volatility or fixedneſs of a body ſeeming to conſiſt only in this, that the one is of a texture, or has component parts, that will be eaſily rarified into the form of air, and to the other that it has ſuch as will not, without much ado, be brought to ſuch a conſtitution; and this is that part which remains behind in a white body called aſhes, which contains a ſubſtance or ſalt which chemiſts call *alkali*: what the particular natures of each

each of theſe bodies are, I ſhall not here examine, intending it in another place, but ſhall rather add, that this hypotheſis does ſo exactly agree with all phenomena of fire, and ſo genuinely explicate each particular circumſtance that I have hitherto obſerved, that it is more than probable that this cauſe which I have aſſigned is the true, adequate, real, and only cauſe of theſe phenomena; and therefore I ſhall proceed a little further to ſhew the nature and uſe of the air.

Tenthly, therefore the diſſolving parts of the air are but few that is, it ſeems, of the nature of thoſe *ſaline menſtruums* or ſpirits that have very much phlegm mixed with the ſpirits, and therefore a ſmall parcel of it is quickly glutted, and will diſſolve no more, and therefore unleſs ſome freſh part of this menſtruum be applied to the body to be diſſolved, the action ceaſes, and the body leaves to be diſſolved and to ſhine, which is the indication of it, though placed or kept in the greateſt heat; whereas *ſaltpetre* is a *menſtruum*, when melted and red hot, that abounds more with thoſe diſſolvent particles, and therefore as a ſmall quantity of it will diſſolve a great ſulphureous body, ſo will the diſſolution be very quick and violent.

Therefore, in the eleventh place, it is obſervable that, as in other ſolutions, if a copious and quick ſupply of freſh menſtruum, though but weak, be poured on, and applied to the diſſoluble body, it quickly conſumes it; ſo this *menſtruum* of the air if by bellows, or any other ſuch contrivance, it be copiouſly applied to the ſhining body, is found to diſſolve it as ſoon and as violently as the more ſtrong menſtruum of melted nitre.

Therefore, twelfthly, it ſeems reaſonable to think that there is no ſuch thing as an element of fire that ſhould attract or draw up the flame, or towards which the flame ſhould endeavour to aſcend out of a deſire or appetite of uniting with that as its *homogeneal* primitive, and generating element; but that the ſhining tranſient body which we call flame is nothing elſe but a mixture of air and volatile ſulphureous parts of diſſoluble or combuſtible bodies, which are acting upon each other whilſt they aſcend, that is, flame ſeems to be a mixture of air and the combuſtible volatile parts of any body, which parts the encompaſſing air does diſſolve or work upon, which action, as it does intend the heat of the *aerial* parts of the diſſolvent, ſo does it thereby further rarify thoſe parts that are acting, or that are very near them, whereby they growing much lighter than the heavy parts of that menſtruum that are more remote, are thereby protruded and driven upwards; and this may be eaſily obſerved alſo in diſſolutions made by any other *menſtruum*, eſpecially ſuch as either create heat or bubbles. Now this action of the *menſtruum* or air on the diſſoluble parts is made with ſuch violence, or is ſuch that it imparts ſuch a motion or pulſe to the *diaphanous* parts of the air, as I have elſewhere ſhewn is requiſite to produce light.

This hypotheſis I have endeavoured to raiſe from an infinite of obſervations and experiments, the proceſs of which would be much too long to be here inſerted, and will perhaps another time afford matter copious enough for a much larger diſcourſe; the air being a ſubject which (though all the world has hitherto lived and breathed in, and been unconverſant about) has yet been ſo little truly examined or explained, that a diligent enquirer will be able to find but very little information from what has been (till of late) written of it; but being once well underſtood, it will, I doubt not, enable a man to render an

intelligible,

intelligible, nay probable, if not the true reaſon of all the *phenomena* of fire, which, as it has been found by writers and philoſophers of all ages a matter of no ſmall difficulty, as may be ſufficiently underſtood by their ſtrange *hypotheſes,* and unintelligible ſolutions of ſome few *phenomena* of it; ſo will it prove a matter of no ſmall concern and uſe in human affairs, as I ſhall elſewhere endeavour to manifeſt, when I come to ſhew the uſe of the air in reſpiration, and for the preſervation of the life; nay, for the conſervation and reſtoration of the health, and natural conſtitution of mankind, as well as all other aerial animals, as alſo the uſes of this principle, or propriety of the air in chemical, mechanical, and other operations. In this place I have only time to hint an *hypotheſis,* which, if God permits me life and opportunity, I may elſewhere proſecute, improve, and publiſh.

VI.

Experiments and Obſervations on Shell and Bone. By CHARLES HATCHETT, *Eſq. F. R. S.* *

SOME experiments, which I lately made at the requeſt of Mr. Home, and which he has done me the honor to mention in his ingenious paper on the teeth of granivorous quadrupeds, induced me to turn my attention more particularly to the chemical examination of ſhell and bone, eſpecially as the former appeared to have been hitherto much neglected.

The time ſince theſe experiments were began, has not been ſufficient to enable me to enter into all the minutiæ of the chemical analyſis of theſe ſubſtances; but as ſome remarkable facts were aſcertained, I have now ventured to bring them forward, with the addition of ſome obſervations, although as yet the whole is little more than a very imperfect outline.

The firſt of theſe experiments were made on the ſhells of marine animals; and to avoid repetition and prolixity, I ſhall, in a great meaſure, once for all, deſcribe the menſtrua, the precipitants, and the mode of operation.

When ſhells were examined they were immerſed in acetous acid, or nitric acid diluted, according to circumſtances, with 4, 5, 6, or more parts of diſtilled water; and the ſolution was always made without heat.

The carbonate of lime was precipitated by the carbonate of ammoniac, or of pot-aſh; and phoſphate of lime (if preſent) was previouſly precipitated by pure or cauſtic ammoniac.

If any other phoſphate like that of ſoda was ſuſpected, it was diſcovered by ſolution of acetite of lead.

Bones and teeth were alſo ſubjected to the action of the acetous, or diluted nitric and muriatic acids.

* Philoſ. Tranſ. 1799.

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The diſſolved portion was examined by the above-mentioned precipitants; and in experiments where the quantity of the ſubſtance would permit, the phoſphoric acid was alſo ſeparated by nitric or ſulphuric acid. The phoſphoric acid thus obtained, was proved after concentration by experiments, which being uſually employed for ſuch purpoſes, are too well known to require deſcription.

It is neceſſary moreover to obſerve, that as the ſubſtances examined were very numerous, and my principal object was to diſcover the moſt prominent characters in them, I did not for the preſent attempt in general, to aſcertain minutely the proportions, ſo much as the number and quality of their reſpective ingredients.

The greater part, if not all, of marine ſhells, appear to be of two deſcriptions in reſpect to the ſubſtance of which they are compoſed. Thoſe which will be firſt noticed have a porcellaneous aſpect, with an enamelled ſurface, and when broken, are often in a ſlight degree, of a fibrous texture.

The ſhells of the other diviſion have generally, if not always, a ſtrong epidermis, under which is the ſhell, principally or intirely compoſed of the ſubſtance called *nacre*, or mother of pearl.

Of the porcellaneous ſhells, various ſpecies of voluta, cypræa, and others of a ſimilar nature were examined.

Of the ſhells compoſed of nacre, or mother of pearl, I ſelected the oyſter, the river muſcle, the *haliotis iris*, and the *turbo olearius*.

Experiments on Porcellaneous Shells.

Shells of this deſcription, when expoſed to a red heat in a crucible during about a quarter of an hour, crackled and loſt the colours of their enamelled ſurface; they did not emit any apparent ſmoke, nor any ſmell like that of burnt horn, or cartilage. Their figure remained unchanged, excepting a few flaws; and they became of an opaque white, tinged partially with pale grey, but retained part of their original gloſs.

The ſhells which had not been expoſed to fire, (whether intire or in powder) diſſolved with great effervefcence in the various acids; and the ſolution afterwards remained colourleſs and tranſparent.

But the ſhells which had been burned, upon being diſſolved, depoſited a very ſmall quantity of animal coal; and thereby the preſence of ſome gluten was denoted, although the proportion was too ſmall to be diſcovered in the ſolution of the ſhells which had not been burned.

The various ſolutions were filtrated, and were examined by pure ammoniac and acetite of lead; but I never obtained any trace of phoſphate of lime, nor of any other combination of phoſphoric acid.

The carbonate of lime was afterwards precipitated by carbonate of ammoniac; and from

many experiments it appeared, that porcellaneous ſhells conſiſt of carbonate of lime, cemented by a very ſmall portion of animal gluten.

Previous to the experiments on ſhells compoſed of nacre, or mother of pearl, I examined ſome patellæ from Madeira.

When theſe were expoſed to a red heat in a crucible, there was a perceptible ſmell, like that of horn, hair, or feathers.

The proportion of carbonic matter depoſited by the ſubſequent ſolution, was more conſiderable than that of the ſhells above mentioned; and the proportion of carbonate of lime relative to their weight was leſs.

When the recent ſhells were immerſed in very dilute nitric acid, the epidermis was ſeparated, the whole of the carbonate of lime was diſſolved, and a gelatinous ſubſtance, nearly liquid, remained, but without retaining the figure of the ſhell, and without any fibrous appearance.

Theſe ſhells evidently, therefore, contain a larger portion of a more viſcid gelatinous ſubſtance, than thoſe before mentioned; but the ſolution ſeparated from the gelatinous ſubſtance afforded nothing but carbonate of lime.

Experiments on Shells compoſed of Nacre, or Mother of Pearl.

When the ſhell of the common oyſter was expoſed to a red heat, the effects were the ſame as thoſe obſerved in the patellæ, and the ſolution of the unburned ſhell was ſimilar, only the gelatinous part was of a greater conſiſtency.

A ſpecies of the river muſcle was next ſubjected to experiment. This when burned in a crucible emitted much ſmoke, with a ſtrong ſmell of burned cartilage, or horn; the ſhell throughout became of a dark grey, and exfoliated. By ſolution in the acids a large quantity of carbonic matter was ſeparated; and much leſs of carbonate of lime was obtained from a given weight of the ſhell, than from thoſe already mentioned.

Upon immerſing an unburned ſhell in dilute nitric acid, a rapid ſolution and efferveſcence at firſt took place, but gradually became leſs, ſo that the diſengagement of the carbonic acid gas was to be perceived only at intervals.

At the end of two days I found nearly the whole of the carbonate of lime diſſolved, but a ſeries of membranes retaining the figure of the ſhell remained, of which the epidermis conſtituted the firſt.

In the beginning the carbonate of lime was readily diſſolved, becauſe the acid menſtruum had an eaſy acceſs; but after this it had more difficulty to inſinuate itſelf between the different membranes, and of courſe the ſolution of the carbonate of lime was ſlower.

During the ſolution the carbonic acid gas was entangled, and retained in many places between the membranes, ſo as to give to the whole a cellular appearance.

The *haliotis iris* and the *turbo olearius* reſembled this muſcle, excepting that their membranaceous parts were more compact and denſe.

 Theſe

These ſhells, when deprived by an acid menſtruum of their hardening ſubſtance, or carbonate of lime, appear to be formed of various membranes, applied ſtratum ſuper ſtratum.

Each membrane has a correſponding coat or cruſt of carbonate of lime; which is ſo ſituated, that it is always between every two membranes, beginning with the epidermis, and ending with the laſt formed internal membrane.

The animals which inhabit theſe ſtratified ſhells, increaſe their habitation by the addition of a ſtratum of carbonate of lime, ſecured by a new membrane; and as every additional ſtratum exceeds in extent that which was previouſly formed, the ſhell becomes ſtronger in proportion as it is enlarged, and the growth and age of the animal becomes denoted by the number of the ſtrata which concur to form the ſhell.

Although the *haliotis iris*, and the *turbo olearius*, are compoſed of the true mother of pearl, I was induced to repeat the foregoing experiments on ſome detached pieces of mother of pearl, ſuch as are brought from China.

Theſe experiments I need not deſcribe, as the reſults were preciſely the ſame.

I muſt, however, obſerve, that the membranaceous or cartilaginous parts of theſe ſhells, as of the pieces of mother of pearl, retained the exact figure of the ſhell, or piece which had been immerſed in the acid menſtruum; and theſe membranaceous parts diſtinctly appeared to be compoſed of fibres placed in a parallel direction, correſponding to the configuration of the ſhell.

The ſame experiments were made on pearls; which proved to be ſimilar in compoſition to the mother of pearl; and ſo far as their ſize would enable me to diſcern, they appeared to be formed by concentric coats of membrane and carbonate of lime; by this ſtructure they much reſemble the globular calcareous concretions found at Carlſbad, and other places, called piſolithes.

The wavy appearance, and irreſdeſcency of mother of pearl, and of pearl, are evidently the effect of their lamellated ſtructure and ſemi-tranſparency; in which, in ſome degree, they are reſembled by the lamellated ſtone, called adularia.

When the experiments on the porcellaneous ſhells, and on thoſe formed of mother of pearl, are compared, it appears that the porcellaneous ſhells are compoſed of carbonate of lime, cemented by a very ſmall portion of gluten; and that mother of pearl, and pearl, do not differ from theſe, except by a ſmaller proportion of carbonate of lime; which inſtead of being ſimply cemented by animal gluten, is intermixed with, and ſerves to harden a membranaceous or cartilaginous ſubſtance; and this ſubſtance, even when deprived of the carbonate of lime, ſtill retains the figure of the ſhell.

But between theſe extremes, there will probably be found many gradations; and theſe we have the greater reaſon to expect, from the example afforded by the patellæ, which have been lately mentioned.

Some few experiments were made on certain land ſhells; and in the common garden ſnail I thought that I diſcovered ſome traces of phoſphate of lime; but as I did not find

 any

any in the *helix memoralis*, it may be doubted whether the presence of phosphate of lime should be considered as a chemical character of land shells *.

Experiments on the covering Substance of Crustaceous Marine Animals †.

As I was not acquainted with any experiments, by which the chemical nature of the substance which covers crustaceous marine animals had been determined, I was desirous to ascertain in what respect it was different from shell; and I began these experiments on three species of the echinus, with which I had been favoured by the Right Hon. President.

I was the more inclined to begin with the echini, because naturalists do not appear to be perfectly agreed, whether to call them testaceous or crustaceous animals.

Klein, who has written a work upon echini, after having noticed the various opinions of Rondelet, Rumphius, and others, determines that they are to be regarded as testaceous animals. His words are, "Sic plurimas testas marinas, in statu naturali consideratas, cum "echinodermatis potius quam cum crustis astacorum vel cancrorum conferre licebit. "Itaque echinoderma cum Aristotele qui echinos inter testacea quibus facultas ingredi-"endi est reponit, nec non cum Belonio, Aldrovando, et excellentissimo Sloanio religiosè "testam appellamus, quam satis duram in nonnullis offendimus ‡."

But Linnæus was of the contrary opinion, as appears from his definition of the echinus. "Corpus subrotundum *crusta ossea tectum* spinis mobilibus sæpius aspera §."

Now as the experiments above related had proved, that the shells of marine animals were composed of carbonate of lime, without any phosphate, I thought it very possible that the covering of the crustaceous animals might, in some respect, be different; and if so, I should thus by chemical characters, be enabled to ascertain the class to which the echinus was to be referred.

Of the three echini which were examined, one had small spines; the second had large obtuse spires; and the third was of a very flat form.

Portions of these echini were separately immersed in acetous, muriatic, and diluted nitric acid, by each of which they were compleatly dissolved with much effervescence; depositing at the same time a thin outer skin, or epidermis. The transparency of the solutions was also disturbed by a portion of gluten which remained suspended, and communicated a brownish colour to the liquors.

The solutions in acetous and diluted nitric acid were filtrated; after which, from the acetous solution of each echinus, I obtained a precipitate of phosphate of lead, by the ad-

* Some experiments which I have lately made upon the cuttle bone of the shops have proved, that the term bone is here misapplied; if the presence of phosphate of lime is to be regarded as the characteristic of bone; for this substance in composition is exactly similar to shell, and consists of various membranes hardened by carbonate of lime, without the smallest mixture of phosphate.

† Under this head I have included my experiments upon echini star-fish, crabs, lobsters, &c.

‡ *Klein Naturalis dispositia Echinodermatum*, &c. p. 10.

§ *Systema Naturæ*. Edit. Gmelin, p. 3168.

dition

dition of acetite of lead; and having thus proved the presence of phosphoric acid, I saturated the nitric solutions with pure ammoniac, by which a quantity of phosphate of lime was obtained, much inferior however in quantity to the carbonate of lime, which was afterwards precipitated by carbonate of ammoniac.

The composition of the crust of the echinus is therefore different from that of marine shells; and by the relative proportions and nature of the ingredients, it approaches most nearly to the shells of the eggs of birds; which in like manner consist of carbonate, with a small proportion of phosphate of lime cemented by gluten.

It remained now to examine the composition of those substances which are decidedly called crustaceous; but previous to this some experiments were made on the asterias, or star fish, of which I took the species commonly found on our coasts, and known by the popular name of five fingers (asterias rubens.)

The asterias is thus described by Linnæus. "Corpus depressum, subtus sulcatum; "crusta coriacea, tentaculis muricata *."

When the asterias was immersed in the acids, a considerable effervescence was produced, and a thin external stratum was dissolved; after which it remained in a perfectly coriaceous state, and complete in respect to the original figure.

The dissolved portion being examined by the usual precipitants, proved to be carbonate of lime, without any mixture of phosphate; but in another species of the asterias, which had twelve rays (asterias papposa,) I discovered a small quantity of phosphate of lime. I am therefore induced to suspect that in the different species of the asterias, nature makes an imperfect attempt to form shell on some, and a crustaceous coating on others; and that a series of gradations is thus formed between the testaceous, the crustaceous, and the coriaceous marine animals.

It was now requisite to ascertain if phosphate of lime is a component part of the substance which covers the crustaceous marine, or aquatic animals, such as the crab, lobster, prawn, and crayfish.

Pieces of this substance, taken from various parts of those animals, was at different times immersed in acetous and in diluted nitric acid; those which had been placed in the diluted nitric acid produced a moderate effervescence, and in a short time were found to be soft and elastic, of a yellowish white colour, and like a cartilage, which retained the original figure.

The same effects were produced by acetous acid, but in a less degree; in the latter case also a colouring matter remained, and was soluble in alcohol.

All the solutions, both acetous and nitric, afforded carbonate and phosphate of lime, although the former was in the largest proportion.

There is reason to conclude, therefore, that phosphate of lime, mingled with the carbonate, is a chemical characteristic which distinguishes the crustaceous from the testaceous substances; and that the principal difference in the qualities of each, when compleat, is

* Systema Naturæ. Ed. Gmelin. p. 3160.

caused

cauſed by the proportion of the hardening ſubſtances relative to the gluten by which they are cemented; or by the abundance and conſiſtency of the gelatinous, membranaceous or cartilagenous ſubſtance, in and on which the carbonate of lime, or the mixture of carbonate and phoſphate of lime, has been ſecreted and depoſited. Moreover as the preſence of phoſphate of lime, mingled with carbonate, appears to be a chemical character of cruſtaceous marine animals, there is every reaſon to conclude that Linnæus did right not to place the echini among the teſtacious ones.

The preſence of phoſphate of lime in the ſubſtance which covers the cruſtaceous marine animals, appears to denote an approximation to thenature of bone, which, not only by the experiments of Mr. Gahn, but by the united teſtimony of all chemiſts, has been proved principally to conſiſt (as far as the oſſifying ſubſtance is concerned) of phoſphate of lime.

This conſideration, therefore, induced me to repeat the above experiments on the bones of various animals.

It is ſcarcely neceſſary for me to mention the uſual effects of acids on bones ſteeped in them, as they are known to every phyſiologiſt and anatomiſt.

In every operation of this nature, the oſſifying ſubſtance, which is principally phoſphate of lime, is diſſolved, and a cartilage or membrane of the figure of the original bone remains; ſo that the firſt origin of bones appears to be by the formation of a membrane or cartilage of the requiſite figure, which, when the ſubſequent ſecretion of the oſſifying ſubſtance takes place, is penetrated by, and thus becomes more or leſs converted into the ſtate of bone.

It is alſo known that the nature of the bone is more influenced by the greater or leſs predominance of the membranaceous or cartilagenous part than by any other cauſe. It is not, therefore, for me to add any thing to this part; and in reſpect to the ſubſtance which is the cauſe of oſſification little alſo requires to be mentioned, for this (as has been already obſerved) is known principally to conſiſt of phoſphate of lime. I ſhall only therefore briefly mention the reſult of certain experiments.

(*To be continued.*)

VII.

Experiments upon the Reſiſtance of Bodies moving in Fluids. By the Rev. Samuel Vince, *A. M. F. R. S. Plumian Profeſſor of Aſtronomy and experimental Philoſophy, in the Univerſity of Cambridge**.

In a former paper upon the motion of fluids, I ſtated the difficulties to which the theory is ſubject, and ſhewed its inſufficiency to determine the time of emptying veſſels, even in the moſt ſimple caſes; I alſo proved, by actual experiment, that in many inſtances there was no agreement between their reſults, and thoſe deduced from theory. The great

* Philoſ. Tranſ. 1798.

difference

difference between the experimental and theoretical conclusions, in most of the cases which respect the times in which vessels empty themselves through pipes, necessarily leads us to suspect the truth of the theory of the action of fluids under all other circumstances. In the doctrine of the resistance of fluids, we see strong reasons to induce us to believe, that the theory cannot generally lead us to any true conclusions. When a body moves in a fluid, its particles strike the body; and in our theoretical considerations after this action, the particles are supposed to produce no further effect, but are conceived to be, as it were, annihilated. But in fact this cannot be the case; and what we are to allow for their effect afterwards, is beyond the reach of mere theoretical investigation. Whatever theory therefore we can admit, must be that which is founded upon such experiments, as include in them every principle which is subject to any degree of uncertainty: we must therefore have recourse to experiments, in order to establish any conclusions upon which we may afterwards reason. In the paper above mentioned, I described a machine to find the resistances of bodies moving in fluids; since which time I have made a variety of experiments with it upon bodies moving both in air and water, and have every reason to be satisfied of its great accuracy. In this paper I propose to examine the resistance which arises from the action of non-elastic fluids upon bodies.

This subject divides itself into two parts; we may consider the action of water at rest upon a body moving in it; or we may consider the action of the water in motion when the body is at rest. We will first give the result of our experiments in the former case, and compare them with the conclusions deduced from theory. Now the radius of the axis of the machine made use of, was 0,2117 inches; the area of the four planes, was 3,73 inches; the distances of their centres of resistance from the axis, was 7,57 inches; and they moved with a velocity of 0,66 feet in a second. The first column of the following table exhibits the angles at which the planes struck the fluid; the second column shews the resistance by experiment, in the direction of their motion in troy ounces: the third column gives the resistance by theory, assuming the perpendicular resistance to be the same as by experiment; the fourth column shews the power of the sine of the angle to which the resistance is proportional.

Angle.	Experiment.	Theory.	Power.
10°	0,0112	0,0012	1,73
20	0,0364	0,0093	1,73
30	0,0769	0,0290	1,54
40	0,1174	0,0616	1,54
50	0,1552	0,1043	1,51
60	0,1902	0,1476	1,38
70	0,2125	0,1926	1,42
80	0,2237	0.2217	2,41
90	0,2321	0,2321	

The

The fourth column was thus computed: let s be the ſine of the angle to radius unity, r the reſiſtance at that angle, and ſuppoſe r to vary as s^m; the $r^m : s^m :: 0{,}2321 : r$, hence $s^m = \frac{1}{0{,}2321}$, and conſequently $m = \frac{\log. r - \log. 0{,}2321}{\log. s}$; and by ſubſtituting for r and s their ſeveral correſponding values, we get the reſpective values of m, which are the numbers in the fourth column. Now the theory ſuppoſes the reſiſtance to vary as the cube of the ſine; whereas the reſiſtance decreaſes from an angle of 90° in a leſs ratio than that, but not as any conſtant power, nor as any function of the ſine and coſine that I have yet diſcovered. Hence the actual reſiſtance is always greater than that which is deduced from theory, aſſuming the perpendicular reſiſtance to be the ſame; the reaſon of which, in part at leaſt is, that in our theory we neglect the whole of that part of the force, which after reſolution acts parallel to the plane; whereas (from the experiments which will be afterwards mentioned) it appears that part of that force acts upon the plane; alſo the reſiſtance of the fluid which eſcapes from the plane into the ſurrounding fluid, may probably tend to increaſe the *actual* reſiſtance above that which the theory gives, in which that conſideration does not enter; but as this latter circumſtance affects the reſiſtance at all angles, and we do not know the quantity of effect which it produces, we cannot ſay how it may affect the *ratio* of the reſiſtances at different angles.

In theory the reſiſtance perpendicular to the planes is ſuppoſed to be equal to the weight of a column of fluid, whoſe baſe = 3,73 inches, and altitude = the ſpace through which a body muſt fall to acquire the velocity of 0,66 feet. Now that ſpace is 0,08124 inches, conſequently the weight of column = 0,1598 troy ounces; but the actual reſiſtance was found to be = 0,2321 ounces. Hence the actual reſiſtance of the planes; the reſiſtance in our theory :: 02321 : 0,1598, which is nearly as 3 : 2.

I am aware that experiments have been made upon the reſiſtance of bodies moving in water, which have agreed with our theory. An extenſive ſet was inſtituted by D'Alembert, Condorcet, and Boſſut, the reſult of which very nearly coincided with theory, ſo far as regards the abſolute quantity of the perpendicular reſiſtance. Their experiments were made upon floating bodies drawn upon the fluid, by a force acting upon them in a direction parallel to the ſurface of the fluid. There can be no doubt but that theſe experiments were very accurately made. The experiments here related were alſo repeated ſo often, and with ſo much care, and the reſults always agreed ſo nearly, that there can be no doubt but that they give the actual reſiſtance to a very conſiderable degree of accuracy. In our experiments the planes were immerſed at ſome depth in the fluid; in the other caſe the bodies floated on the ſurface; and I can ſee no way of accounting for the difference of the reſiſtances, but by ſuppoſing, that at the ſurface of the fluid, the fluid from the end of the body may eſcape more eaſily than when the body is immerſed below the ſurface; but this I confeſs appears by no means a ſatisfactory ſolution of the difficulty. The reſiſtances of bodies deſcending in fluids manifeſtly come under the caſe of our experiments.

Two

Two ſemi-globes were next taken, and made to revolve with their flat ſides forwards. The diameter of each was 1,1 inches; the diſtance of the centre of reſiſtance from the axis, was 6,[illegible] inches; and they moved with a velocity of 0,542 feet in a ſecond; and the reſiſtance was found to be 0,08339 ounces by experiment. By theory the reſiſtance is 0,05496 ounces, hence the reſiſtance by experiment: the reſiſtance by theory :: 0,08339 : 0,05496, agreeing very well with the above mentioned proportion. But when the ſpherical ſides moved forwards with the ſame velocity, the reſiſtance was 0,034 ounces. Hence the reſiſtance on the ſpherical ſide of a ſemi-globe : reſiſtance on its baſe :: 0,034 : 0,08339; but this is not the proportion of the reſiſtance of a perfect globe, to the reſiſtance of a cylinder of the ſame diameter, moving with the ſame velocity, becauſe the reſiſtance depends upon the figure of the back part of the body.

I therefore took two cylinders of the ſame diameter as the two ſemi-globes, and of the ſame weight; and giving them the ſame velocity, I found the reſiſtance to be 0,07998 ounces; therefore the reſiſtance on the flat ſide of a ſemi-globe : the reſiſtance of a cylinder of the ſame diameter, and moving with the ſame velocity, :: 0,08339 : 0,07998. This difference can ariſe only from the action of the fluid on the back ſide of the ſemi-globe, moving with its flat ſide forwards, being leſs than that on the back of the cylinder, in conſequence of which the ſemi-globe ſuffered the greater reſiſtance. The reſiſtance of the cylinders thus determined directly by experiment, agrees very well with the foregoing experiments. The reſiſtance *cæteris paribus*, varies as the ſquare of the velocity very nearly, and may be taken ſo for all practical purpoſes, as I find by repeated experiments made both upon air and water, in the manner deſcribed in my former paper. Hence for the different planes the reſiſtance varies as the area $\times$ the ſquare of the velocity. Now the reſiſtance of the planes whoſe area was 3,73 inches, moving with a velocity of 0,66 feet in a ſecond, was found to be = 0,2321 ounces. Alſo the area of the two cylinders was 1,9 inches, and their velocity was 0,542 feet in a ſecond; to find therefore the reſiſtance of the cylinders from that of the planes, we have $0{,}66^2 \times 3{,}73 : 0{,}542^2 \times 1{,}9 :: 0{,}2321$ ounces : 0,07973 ounces; for the reſiſtance on the cylinders differing but a very little from 0,07998 ounces, the reſiſtance found from direct experiment.

Now to get the reſiſtance on a perfect globe, we muſt conſider that when the back part is ſpherical, the reſiſtance is greater than when it is flat in the ratio of 0,08339 : 07998; hence the reſiſtance on a globe : the reſiſtance on a ſemi-globe in the ſame ratio; but the reſiſtance on the ſemi-globe was 0,034 ounces; hence 0,07998 : 0,08339 :: 0,034 ounces : 0,0354 ounces, the reſiſtance of a globe, conſequently the reſiſtance of a globe : the reſiſtance of a cylinder of the ſame diameter moving with the ſame velocity in water, :: 0,0354 : 0,07998 :: 1 : 2,23.

We proceed next to compare the actual reſiſtance of a globe, with the reſiſtance aſſumed in our theory. In the firſt place, the abſolute quantity of reſiſtance has been found to be greater than that which we uſe in theory, in the ratio of 0,2321 : 0,1598; but by theory the reſiſtance of the globe : the reſiſtance of the cylinder :: 1 : 2, or as 1,115 : 2,23;

 hence

hence by theory we make the resistance of the globe too great in the ratio of 1,115 : 1; and it is too small, from the former consideration, in the ratio of 0,1598 : 0,2321 : therefore the actual resistance of the globe : the resistance in theory :: 0,2321 : 0,1598 × 1,115 :: 0,2321 : 0,1782, which is nearly in the ratio of 4 : 3. Thus far we have considered the resistance of bodies moving in a fluid; we come next to consider the action of a fluid in motion upon a body at rest.

A vessel, five feet high, was filled with a fluid, which could be discharged with a stop cock, in a direction parallel to the horizon. The cock being opened, the curve which the stream described was marked out upon a plane set perpendicular to the horizon; and by examining this curve, it was found to be a very accurate parabola, the abscissa of which was 13,85 inc. and the ordinate was 50 inc. hence the latus rectum was 180,5 inc. one-fourth of which is 45,1 inc. which is the space through which a body must fall to acquire the velocity of projection; hence that velocity was 189,6 inc. in a second. And here by the by we may take notice of a remarkable circumstance. The depth of the cock, below the surface of the fluid, was 45,1 inc. hence the velocity of projection was that which a body acquires in falling through a space equal to the whole depth of the fluid, whereas, through a simple orifice the velocity would have been that which is acquired in falling through half the depth; the pipe of the stop cock, therefore, increased the velocity of the fluid in the ratio of $1 : \sqrt{2}$, and gave it the greatest velocity possible; the length of the pipe was 3 inc. and the area of the section 0,045 inc. also the base of the vessel was a square, the side of which was twelve inches.

The area of the section of the pipe may be found very accurately in the following manner: The vessel being kept constantly full, receives the quantity of fluid, run out in any time t'', and then weigh it, by which we shall be able to get the quantity in cubic inches. Now, if $v =$ the velocity of the fluid when it issues from the pipe, $a =$ the area of the section of the pipe, $l =$ the length of the cylinder of water run out, whose base $= a$ and $m =$ the quantity of fluid discharged in t'', then $v : l :: 1'' : t''$; hence $l = v\,t$; but $a\,l = m$; therefore $a\,v\,t = m$; hence $a = \frac{m}{v\,t}$. In the present instance $t = 20$ $m = 170,63$ cubic inches $v = 189,6$; hence $a = 0,045$.

Let A B C D (Fig. 1. Plate XXII.) be a solid piece of wood, upon which are fixed two upright pieces $r\,s$, $t\,u$; between these a flat lever, $e\,a\,c$, is fastened in a perpendicular position on the axis $x\,y$, and nicely balanced; and let a be a point directly against the middle of the axis in a line perpendicular to the plane of the lever. This apparatus is placed against the stop cock, at the distance of about one inch, and when the water is let go, let us suppose the centre of the stream to strike the lever perpendicularly at e; take $a\,c = a\,e$, and on the opposite side to that on which the stream acts fasten a fine silk string at c, and bring it over a pulley p, and adjust it in a direction perpendicular to the plane of the lever, and at the end which hangs down fix a scale Q, the weight of which is to be previously determined. All the

the apparatus being thus adjuſted, open the ſtop cock, and let the fluid ſtrike the lever, and put ſuch weight into the ſcale as will firſt keep the lever in its perpendicular ſituation, and that weight with the weight of the ſcale muſt be juſt equivalent to the action of the fluid. Thus we get the perpendicular effect of the water. Now incline the plane of the lever at any angle to the direction of the ſtream, and adjuſt the ſtring perpendicular to the plane as before; then put ſuch a weight into the ſcale as will keep the lever perpendicular to the horizon whilſt the fluid acts upon it, and you get that part of the effect of the fluid which acts perpendicular to the plane. In this manner, when the fluid acts oblique to the plane, we get the perpendicular part of the force. The ſecond column of the following table ſhews this effect by experiment, for every tenth degree of inclination ſhewn in the firſt column, and the third column ſhews the effect by theory from the perpendicular force, ſuppoſing it to vary as the ſine of inclination.

Angle.	Experiments.			Theory.		
	oz.	dwts.	grs.	oz.	dwts.	grs.
90°	1	17	12	1	17	12
80	1	17	0	1	16	22
70	1	15	12	1	15	6
60	1	12	12	1	12	11
50	1	18	10	1	18	17
40	1	4	10	1	4	2
30	0	18	18	0	18	18
20	0	12	12	0	12	19
10	0	6	4	0	6	12

It appears from hence that the reſiſtance varies as the ſine of the angle at which the fluid ſtrikes the plane; the difference between the theory and experiment being only ſuch as may be ſuppoſed to ariſe from the want of accuracy, to which the experiments muſt neceſſarily be ſubject.

Let us now firſt conſider what the whole perpendicular reſiſtance by experiment is when compared with that by theory. Now by theory the reſiſtance is equal to the weight of a column of the fluid, whoſe baſe =0,045 inc. and altitude =45,1 inc. and the weight of that column is = 1 oz. 1 dwt. 10 grs. Hence reſiſtance by theory : the reſiſtance by experiment : : 1 oz. 1 dwt. 10 grs. : 1 oz. 17 dwts. 12 grs. : : 514 : 900.

In the next place let us examine what is this reſiſtance compared with the reſiſtance of a plane moving in a fluid. We here prove, that the reſiſtance of the fluid in motion acting on the plane at reſt : the reſiſtance by theory : : 900 : 514; and we have before proved that the reſiſtance by theory : the reſiſtance of a plane body moving in a fluid : : 1598 : 2321; hence the reſiſtance of a fluid in motion upon a plane at reſt, the reſiſtance of the ſame plane moving with the ſame velocity in a fluid at reſt : : 900×1598 : 514×2321 : : 1438200 : 1192954 : : 6 : 5 nearly. Now we know that the actual effect on the plane muſt be the ſame in both caſes;

cafes; and the difference, I conceive, can arife only from the action of the fluid behind the body in the latter cafe, there being no effect of this kind in the former cafe. For in refpect to the preffure before the body, that will probably be the fame in both cafes; for there is a preffure of the column of the fpouting fluid acting againft the particles which ftrike the body at reft, fimilar to the action of the fluid before the body upon the particles which ftrike the body moving in the fluid. Hence the refiftance of the planes moving in the fluid with the velocity here given, is diminifhed about one-fifth part of the whole by the preffure behind the body; but with different velocities this diminution muft increafe as the velocity increafes.

The effect of that part of the force which acted *perpendicular* to the plane being thus eftablifhed, we proceed next to examine what part of the whole force which acts *parallel* to the plane is effective. To determine which, the axis *w v* (Fig. 2.) was fixed perpendicular to the plane of the lever *a b c d*, and the ends of the axis were conical, and laid in conical holes; and the thread from which the fcale was hung was fixed to the edge at *e*, and acted perpendicular to it, and the weight drew the lever in the direction *e s*, contrary to that in which the fluid tends to move the lever, and it acted at the fame perpendicular diftance from the axis below as the fluid acted above it. Let *x m z* be a line parallel to the horizon when the lever is perpendicular to it, and which paffes through the centre of the ftream, and let *x m z* be alfo the direction of that part of the force which acts parallel to the plane. This apparatus being adjufted, the experiments were made for every tenth degree of inclination; and here a circumftance took place for which I can give no fatisfactory reafon. Having gone through the experiments once, and noted the refults, I repeated them; and to my great furprize I found all the fecond refults to be very different from the firft. The experiments were therefore repeated again, and the refults were ftill different. Being certain that the experiments were very accurately made each time, I was totally at a lofs to conjecture to what circumftance this difference of refults was owing. By repeating, however, the experiments, and obferving at what point of the line *x m z*, the centre of the ftream, acted, I difcovered that the effect varied by varying that point, that it was greateft when the ftream ftruck the lever as near as it could to *x*; lefs when it ftruck it at the middle *m*, and leaft when it ftruck it as near as it could to *z*, notwithftanding that the ftream acted at the fame perpendicular diftance from the axis in each cafe, and the parallel part of the force always acted in the line *x m z*. At the angles 80° 70° 60° the fluid ftriking as near as it could to the edge *z*, gave the lever a motion not in the direction *x m z*, but in the oppofite direction *z m x*, as appeared by taking away the fcale. I have therefore marked fuch refults with the fign ———, the motion produced being then in a direction oppofite to that which ought to have been produced by that part of the force of the ftream which acts parallel to the plane of the lever. The forces which are here put down are thofe which take effect in a direction parallel to the plane of the lever for every tenth degree of inclination; the perpendicular force being 1 oz. 17 dwts. 12 grs.

At

		dwts.	grs.
At 80° incl.	Edge *z*	—	—
	Middle *m*	3	3
	Edge *x*	10	17
At 70° incl.	Edge *z*	—	—
	Middle *m*	6	2
	Edge *x*	11	10
At 60° incl.	Edge *z*	—	—
	Middle *m*	7	9
	Edge *x*	11	22
At 50° incl.	Edge *z*	0	17
	Middle *m*	8	20
	Edge *x*	13	21
At 40° incl.	Edge *z*	1	16
	Middle *m*	8	6
	Edge *x*	13	15
At 30° incl.	Edge *z*	3	20
	Middle *m*	7	2
	Edge *x*	12	15
At 20° incl.	Edge *z*	4	16
	Middle *m*	6	0
	Edge *x*	11	12
At 10° incl.	Middle *m*	5	12

It is a remarkable circumftance, that the effect of the fluid at *z* increafed regularly as the angle decreafed; for though I did not meafure the negative effects, I could plainly perceive that that was the cafe; whereas the effects at *m* and *x* increafed to about the middle of the quadrant, and then decreafed. At 10° the obliquity was fuch, that the fection of the ftream extended very nearly from one fide of the lever to the other.

As it appears by experiment that the velocity of the fluid flowing out of the veffel was equal to the velocity which a body acquires in falling down the altitude of the fluid above the orifice, the fquare of the velocity muft be in proportion to that altitude. To find, therefore, in this cafe whether the refiftance varied as the fquare of the velocity, I let the water flow perpendicularly againft the plane (Fig. 1.) at different depths, and I always found the refiftances to be in proportion to the depth, and therefore in proportion to the fquare of the velocity, agreeing with what takes place when the body moves in the fluid.

An

VIII.

An Account of a Kettle for boiling inflammable Fluids. In a Letter from THO. P. SMITH *to* ROBERT PATTERSON*.

Philadelphia, June 14, 1798.

SIR,

WHEN we conſider the many unhappy accidents that occur from veſſels, containing inflammable fluids, boiling over, and ſetting fire to buildings, in which manufactures of them are carried on, it muſt ſtrike us a matter of importance to form a veſſel which ſhould be ſo conſtructed as to prevent any of thoſe accidents, and yet of ſo ſimple a form, as to render it fit for general uſe. Impreſſed with theſe ideas, I take the liberty of offering, for your approbation, the following plan.

Let A B C D (Plate XXI.) repreſent a large kettle, D E a ſpout running out at the diſtance of three or four feet, commencing at D, four or five inches from the brim of the kettle, and the termination of it, E juſt as high as the brim C. Let the bottom of this ſpout be covered with wet ſponges or rags. Now ſuppoſe the kettle to be filled up to D with any fluid, then as ſoon as it commenced boiling it would riſe in the kettle, and in riſing but a ſmall perpendicular height would paſs conſiderably up the ſpout D E: here the liquor would ſoon cool, and of conſequence fall back into the kettle, and the whole ſubſide to its original height. This would occur as often as the fluid roſe above D, as the evaporation from the wet ſponges or rags would keep D E conſtantly cool.

It would perhaps be beſt to paſs the ſpout through the ſide of the building into the open air, as thereby the evaporation would be increaſed, and, conſequently, the ſpout kept at a lower temperature; in this caſe it might be covered.

In caſe of the fluid to be boiled poſſeſſing a very ſtrong elective attraction to caloric, or the matter of heat, the ſpout might be extended to the width of the diameter of the kettle, or a projecting ſhelf might be formed all round, lined below with wet ſponges or rags.

I remain, dear SIR,

Your's, &c.

Mr. Robert Patterſon. T. P. SMITH.

P.S. In conformity to the wiſh of the ſociety, I procured a veſſel in the form here propoſed. I firſt tried the experiment with water; it boiled very rapidly; but every time the water roſe into the ſpout, it immediately ſubſided, although the ſpout had been for ſome time directly expoſed to the heat of one of Lewis's furnaces; I afterwards attempted it with oil, but before the oil boiled, the ſoldering of the veſſel, which was made of tin, melted.

* American Tranſactions, 1799.

Letter

IX.

Letter from Mr. DAVY, Superintendant of the Pneumatic Inſtitution, to Mr. NICHOLSON, *on the Nitrous Oxide, or Gaſeous Oxide of Azote, on certain Facts relating to Heat and Light, and on the Diſcovery of the Decompoſition of the Carbonate and Sulphate of Ammoniac.*

SIR,

SINCE my diſcovery of the reſpirability, and extraordinary effects of the gaſeous oxide of azote in April, 1799, a great portion of my time has been devoted to experiments on its properties, compoſition, and mode of action on living beings. Theſe experiments are nearly compleated; but as at leaſt two months will elapſe before they can be publiſhed, and as ſome of the facts to which they relate have been made known to the world in Dr. Beddoes's Notice, to prevent dangerous and inconcluſive experiments, I beg leave to communicate to the public, by means of your Journal, a ſhort account of the mode in which it has uſually been prepared for experiments on reſpiration. Nitrate of ammoniac perfectly neutraliſed, and rendered as dry as poſſible, muſt be expoſed to a heat not below 310, or above 400° of Fahrenheit *. At this temperature it is decompounded into water and gaſeous oxide of azote, or as I would rather call it, nitrous oxide.

The gas muſt be paſſed through water, and ſuffered to remain in contact with it at leaſt an hour and an half before it is reſpired. A ſufficient teſt of its purity is the combuſtion of ſulphur in it, with a vivid roſe coloured flame. The ſame water ſhould be uſed for receiving it through, and retaining it in different experiments. A pound of dry nitrate of ammoniac properly decompoſed, produces rather more than four cubic feet of air. I have found that the nitrous oxide may be likewiſe procured in a ſtate of great purity, by expoſing nitrous gas to dry ſulphite of pot-aſh. 1 of nitrous gas decompoſed in this manner, produces nearly 0,5 of nitrous oxide.

The ſolution of metals in dilute nitrous acid never produces air ſufficiently pure for reſpiration, and the decompoſition of nitrous gas by the ſulphures, wetted iron, &c. is a proceſs too ſlow ever to be employed with advantage.

My inveſtigation of the nature, properties, &c. of the nitrous oxide, and the aeriform fluids relating to it, will conſiſt,

1. Of experiments on its production, from the decompoſition of nitrous acid and nitrous gas in different modes, its analyſis, and the analyſis of the ſubſtances connected with it.

2. On its action on different incombuſtible ſubſtances, on the combuſtion of charcoal,

* This decompoſition was diſcovered by the illuſtrious Berthollet. I have found that at a temperature above 500°, nitrous gas and nitrogene are evolved as well as nitrous oxide. Whenever there is a luminous appearance in the retort, more or leſs of theſe two ſubſtances will be produced.

ſulphur, iron, phoſphorus, and hydrogene in it, on its decompoſition by the compound combuſtible bodies, &c.

3. On its abſorbtion in reſpiration, with a general inveſtigation of this proceſs, and the changes effected in different gaſes by it.

4. A hiſtory of its effects, containing the experience of different individuals who have reſpired it, furniſhed by themſelves. Every day we gain new evidences in favor of its powers. A number of perſons have breathed it ſince the publication of Dr. Beddoes's Notice; all have been affected, and by far the greater number pleaſantly. As yet we have tried it in no diſeaſe, except palſy; but as it ſupplies to the ſyſtem two principles ſo eſſential to perceptive exiſtence, as oxygene and nitrogene, and increaſes the powers of life generally without producing any aſcertainable exhauſtion; there is reaſon to hope that it will be a powerful agent in many diſeaſes of debility.

An experiment on the colliſion of flint and ſteel in vacuo, and in carbonic acid, publiſhed in my Eſſay on Heat and Light*, differs conſiderably in its reſults from a ſimilar one made long ago by the ingenious Mr. Hawkſbee†. In repeating my experiment a number of times under new circumſtances, I have diſcovered the cauſe of this difference; when the gunlock is ſnapped in carbonic acid, or in water, if a ſharp and thin flint be made uſe of, and the ſprings be ſtrong, a faint red light is generally perceptible: but if the flint be thick, and ſufficiently ſharp only to ſtrike off particles from the ſteel, without ſuffering conſiderable abraſion of its own parts; though vivid ſparks are produced in the atmoſphere, not the ſlighteſt luminous appearance is perceptible in carbonic acid. If inſtead of flint, fluor ſpar, phoſphate of lime, or ſugar, be rubbed prettly briſkly againſt ſteel in carbonic acid, as much light is produced as when they are rubbed againſt it in common air. Theſe facts induce me to believe, that whenever light is produced by the colliſion of flint and ſteel in carbonic acid, it ariſes from the colliſion of ſmall particles of flint againſt each other, and never from the ignition of ſteel. I have often examined in a microſcope, the ſmall particles of ſteel ſtruck off by flint in carbonic acid; the edges are generally deprived of metallic luſtre, and exhibit an appearance of fuſion; it is probable, however, that this appearance is owing to a partial oxidation of the particle, from the decompoſition of the water held in ſolution by carbonic acid. In Mr. Hawkſbee's experiments, the apparatus was contrived in ſuch a way, as to produce perpetual abraſion of particles of flint. In my experiment the flint was not ſufficiently thin to produce light in carbonic acid, conſequently it ought to have produced none in vacuo; but if (as the facts I am about to detail render probable) the light produced by flint, fluor ſpar, phoſphate of lime, &c. on colliſion be electrical, there are ſtrong reaſons for believing, that in a Torricellian vacuum, or in a vacuum formed by the abſorbtion of carbonic acid, by a ſolution of pot-aſh ſufficiently concentrated to form a ſolid compound with it, no light would be viſible.

* Weſt Country Contributions. † Phil. Tranſ. No. XXIV. 2185. (or Hawkſbee's Phyſico-mechan. Exper. 2d edit. octavo, London, 1719. page 26.)

Mr. T. Wedgwood found that fluor ſpar, phoſphate of lime, flint, &c. were luminous, not only when heated, but when ſtruck together, and that under water, or in any kind of air. Scheele diſcovered that fluor ſpar, after calcination, loſt its phoſphoreſcence. I expoſed to a long continued red heat fluor ſpar, phoſphate of lime, ſulphate of ſtrontian, ſulphate of barytes, glaſs, ſulphate of lime, and carbonate of lime, all of which were before phoſphoreſcent, and produced light by colliſion under water.

After being ſuffered to cool in the light, they were placed on a heated iron ſucceſſively. The fluor ſpar, the phoſphate of lime, and the ſulphate of ſtrontian and barytes, were not phoſphoric at any temperature. The calcareous ſpar, which had loſt a portion of its carbonic acid, and the gypſum, were nearly as luminous as before; the phoſphoreſcence of the glaſs and flint was barely perceptible.

Two pieces of the calcined fluor were now rubbed together, they produced as much light as before. The phoſphate of lime, the calcareous ſpar, and the gypſum, had loſt their coherence; ſo that the pieces could not be rubbed againſt each other with ſufficient force. The ſulphate of ſtrontian and barytes, the glaſs, and the ſilex, produced as much light as before on colliſion.

I found that all theſe bodies were non-conductors of electric fluid. On rubbing a large cryſtal of quartz with woollen, it became highly electric. Fluor ſpar was likewiſe made electric when heated and ſtrongly rubbed. To prove, however, more ſatisfactorily whether the light produced by the colliſion of two non-conductors was electric, two cylinders of glaſs were ſtruck againſt each other, ſo as to produce light, and one of them was placed in contact with a Leyden phial. After a number of colliſions, on applying a conductor to the phial, I procured a ſmall ſpark.

Two pieces of pyrites, ſufficiently hard to cut glaſs, and extremely brittle, produced an immenſe quantity of light, when ſtruck together, in the atmoſphere; but not the ſlighteſt luminous appearance under water. This body is a good conductor of electric fluid. Do not theſe facts go far to prove that light, when produced by the colliſion of bodies in water, or non-reſpirable air, is electric; and generated by the rapid tranſmiſſion of electrical fluid, excited by colliſion between two non-conducting ſurfaces, to a conducting body? And as iron can be heated, to a degree at which it is capable of decompoſing oxigene gas, in a non-reſpirable air without being luminous; and as pyrites is not luminous under water, is it not probable that light is accidental to, and not neceſſarily produced by, high temperature?

The admiſſion of ſuch inferences would be favourable to my theory of the combinations of light; but facts have occurred to me with regard to the decompoſition of bodies which I have ſuppoſed to contain light, without any luminous appearance. Till I have ſatisfactorily explained theſe facts by new experiments, I beg to be conſidered as a ſceptic with regard to my own particular theory of the combinations of light and theories of light in general. On account of this ſcepticiſm, and for other reaſons, I ſhall in future uſe the common nomenclature; excepting that as my diſcoveries concerning the gaſeous oxide would render it highly

improper to call a principle, which in one of its combinations, is capable of being abſorbed by the venous blood, and of increaſing the powers of life, azote, I ſhall name it, with Dr. Pearſon, Mr. Chaptal, &c. nitrogene, and the gaſeous oxide of azote, the properties and compoſition of which have been miſunderſtood by the chemiſts who gave it that name, nitrous oxide.

Many months ago I made a number of experiments on the compoſition, analyſis, and decompoſition of the ammoniacal ſalts. Theſe experiments have afforded me curious and intereſting reſults; but a wiſh to compleat the inveſtigation relating to the nitrous oxide have prevented me from purſuing them to their full extent. Among theſe reſults, as affording uſeful, practical applications, I ſhall mention the decompoſition of the carbonate and ſulphate of ammonia. Carbonate of ammonia undergoes a change in its compoſition with every change of its temperature; on being heated it gives out carbonic acid, and when cooled abſorbs it again; when paſſed through a tube, heated red, it is decompounded into water, charcoal, nitrogene, and hydro-carbonate. Sulphate of ammonia, the partial decompoſition of which was diſcovered by Mr. Hatchet, when ſent through a tube heated red, is decompounded into ſulphur, water, and nitrogene.

I remain, SIR,

Your's, with much reſpect,

H. DAVY.

X.

Additional Remarks on the Hygrometer and Photometer. By Mr. John Leslie.

To Mr. NICHOLSON.

SIR,

LOOKING over the *notice* of the hygrometer and photometer in your Journal, I perceive a few typographical errors, particularly in the punctuation, but which an attentive reader may eaſily correct. There is a miſtake, however, in the drawing, which it would be proper to rectify: the ſhorter branch of the inſtrument ſhould occupy the centre of the ſocket, in order that the two balls may ſtand in the middle or axis of the cylindrical caſe.

I forgot to remark that, in froſty weather, when the lower ball is enveloped with a congealed cruſt, the ſame degrees of dryneſs in the air will be marked by ſomewhat larger ſpaces on the ſcale of the hygrometer. For in that caſe evaporation has a double office to perform, converting the film of ice ſucceſſively into the liquid and into the aeriform ſtate; the former proceſs expends 75° centigrade of heat, and the latter 524°, and hence the ſize of the correſponding diviſions are to be conceived augmented in the proportion of 75×524, to 524, or that of 8 to 7: that is, to obtain the real dryneſs in time of froſt, one-eighth part muſt be

4 deducted

deducted from the number of degrees indicated on the ſcale. It may not be ſuperfluous to obſerve that, on expoſing the humid coat to freeze, the liquor ſhould be depreſſed to the loweſt point, by touching the upper ball with the hand, elſe, during the act of congelation, the inſtrument is liable to be deranged.

By inſerting theſe additional remarks in your next number, you will much oblige

SIR, your moſt obedient ſervant,

January 6, 1800. JOHN LESLIE.

P.S. An eaſy and expeditious method has ſince occured for obtaining, with remarkable accuracy, the graduation of the ſcale after the compound tube is bent. To the bottom of a ſmall ſaucer, faſten, with ſoft cement, the inſtruments to be graduated, diſpoſed at intervals, the ſtandard one in the centre, in a vertical poſition, with temporary ſcales affixed, and prepared to act as hygrometers. Set this ſaucer on a ſmooth earthen plate: then take a tall glaſs receiver, and pour into it ſome concentrated ſulphuric acid, and keep turning it round and gradually inclining it till the whole of the inſide is completely moiſtened. In this ſtate, invert the receiver over the inſtruments, and let it ſtand on the plate, putting at the ſame time a few drops of the acid about the rim. The air thus included will quickly acquire a dryneſs of 50 or 60 degrees, which will continue without any viſible alteration for a quarter, or perhaps half, an hour. The relative proportions of the different ſcales may therefore be noted with the utmoſt preciſion. With two nice correſpondent thermometers, one of them having its bulb covered with wet bibulous paper, the ſtandard inſtrument could firſt be conſtructed. By attending to thoſe directions, I ſhould preſume that even a common artiſt might qualify himſelf to prepare the inſtruments. I will only add, that for the hygrometer it is preferable to blow both balls of colourleſs glaſs, and cover the lower one with a bit of gold-beater's ſkin.

XI.

An Account of ſome Experiments on the Fecundation of Vegetables. By THOMAS ANDREW KNIGHT, *Eſq.*

(*Concluded from page* 460.)

IN the courſe of the preceding experiments I could never obſerve, that the character either of the male or female in this plant at all preponderated in the offspring; but as this point appeared intereſting, I made a few trials to aſcertain it. And as the foregoing obſervations had occurred in my experiments, made principally to obtain new and improved varieties of the pea for garden culture, I choſe for a ſimilar purpoſe the more hardy varieties, uſually ſown in the fields. By introducing the farina of the largeſt and moſt luxuriant kinds into the bloſſoms of the moſt diminutive, and by reverſing this proceſs, I found

that the powers of the male and female, in their effects on the offspring, are exactly equal. The vigour of the growth, the fize of the feeds produced, and the feafon of maturity were the fame, though the one was a very early, and the other a late variety. I had in this experiment, a ftriking inftance of the ftimulative effects of croffing the breeds; for the fmalleft variety, whofe height rarely exceeded two feet, was increafed to fix feet; whilft the height of the large and luxuriant kind was very little diminifhed. By this procefs it is evident, that any number of new varieties may be obtained; and it is highly probable, that many of thefe will be found better calculated to correct the defects of different foils and fituations, than any we have at prefent; for I imagine that all we now poffefs have in a great manner been the produce of accident; and it will rarely happen in this, or any other cafe, that accident has done all that art will be found able to accomplifh.

The fuccefs of my endeavours to produce improved varieties of the pea, induced me to try fome experiments on wheat; but thefe did not fucceed to my expectations. I readily obtained as many varieties as I wifhed, by merely fowing the different kinds together; for the ftructure of the bloffom of this plant (unlike that of the pea) freely admits the ingrefs of adventitious farina, and is thence very liable to fhoot in varieties. Some of thofe I obtained were excellent; others very bad; and none of them permanent. By feparating the beft varieties, a moft abundant crop was produced; but its quality was not quite equal to the quantity, and all the difcarded varieties again made their appearance. It appeared to me an extraordinary circumftance, that in the years 1795 and 1796, when almoft the whole crop of corn in the ifland was blighted, the varieties thus obtained, and thefe only, efcaped in this neighbourhood, though fown in feveral different foils and fituations.

My fuccefs on the apple (as far as long experience and attention have enabled me to judge, from the cultivated appearance of trees which have not yet borne fruit) has been fully equal to my hopes. But as the improvement of this fruit was the firft object of attention, no probable means of improvement, either from foil or afpect, were neglected. The plants, however, which I obtained from my efforts to unite the good qualities of two kinds of apple, feem to poffefs the greateft health and luxuriance of growth, as well as the moft promifing appearance in other refpects. In fome of thefe the character of the male appears to prevail; in others that of the female; and in others both appear blended, or neither is diftinguifhable. Thefe variations, which were often obfervable in the feeds taken from a fingle apple, evidently arife from the want of permanence in the character of this fruit, when raifed from feed.

The refults of fimilar experiments on another fruit, the grape, were nearly the fame as thofe on the apple, except that by mingling the farina of a black and white grape, juft as the bloffoms of the latter were expanding, I fometimes obtained plants from the fame berry fo diffimilar, that I had good reafon to believe them the produce of fuperfœtation. By taking off the cups, and deftroying the immature fmall parts (as in the pea) I perfectly fucceeded in combining the characters of different varieties of this fruit, as far as the changes of form and autumnal tints in the leaves of the offspring will allow me to judge.

Many experiments of the ſame kind were tried on other plants; but it is ſufficient to ſay, that all tended to evince, that improved varieties of every fruit and eſculent plants may be obtained by this proceſs, and that nature intended that a ſexual intercourſe ſhould take place between neighbouring plants of the ſame ſpecies. The probability of this will, I think, be apparent, when we take a view of the variety of methods which nature has taken to diſperſe the farina, even of thoſe plants in which it has placed the male and female parts within the ſame empalement. It is often ſcattered by an elaſtic exertion of the filaments, which ſupport it on the firſt opening of the bloſſom, and its exceſſive lightneſs renders it capable of being carried to a great diſtance by the wind. Its poſition within the bloſſom is generally well adapted to place it on the bodies of inſects; and the villous coat of the numerous family of bees is not leſs well calculated to carry it. I have frequently obſerved with great pleaſure, the diſperſion of the farina of ſome of the graſſes, when the ſun had juſt riſen in a dewy morning. It ſeemed to be impelled from the plant with conſiderable force; and being blue was eaſily viſible, and very ſtrongly reſembled in appearance the exploſion of a grain of gunpowder. An examination of the ſtructure of the bloſſoms of many plants, will immediately point out, that nature has ſomething more in view than that its own proper males ſhould fecundate each bloſſom; for the means it employs are always thoſe beſt calculated to anſwer the intended purpoſe. But the farina is often ſo placed, that it can never reach the ſummit of the pointal, unleſs by adventitious means; and many trials have convinced me, that it has no action on any other part of it. In promoting this ſexual intercourſe between neighbouring plants of the ſame ſpecies, nature appears to me to have an important purpoſe in view: for independent of its ſtimulative power, this intercourſe certainly tends to confine within more narrow limits, thoſe variations which accidental richneſs, or poverty of ſoil uſually produces. It may be objected, by thoſe who admit the exiſtence of vegetable mules, that under this extenſive intercourſe theſe muſt have been more numerous; but my total want of ſucceſs in many endeavours to produce a ſingle mule plant, makes me much diſpoſed to believe, that hybird plants have been miſtaken for mules; and to doubt (with all the deference I feel for the opinions of Linnæus and his illuſtrious followers) whether nature ever did, or ever will permit the production of ſuch a monſter. The exiſtence of numerous mules in the animal world between kindred ſpecies is allowed; but nature has here guarded againſt their production, by impelling every animal to ſeek its proper mate; and amongſt the feathered tribe, when from perverſion of appetite ſexual intercourſe takes place between thoſe of diſtinct genera*, it has in ſome inſtances, at leaſt, rendered the death of the female the inevitable conſequence. But in the vegetable world there is not any thing to direct the male to its proper female: its farina is carried by winds and inſects to plants of every different genus and ſpecies; and it therefore appears to me (as vegetable mules certainly are not common) that nature has not permitted them to exiſt at all.

* This is ſaid to be the caſe with the drake and the hen.

I cannot

I cannot difmifs this fubject without expreffing my regret that thofe who have made the fcience of botany their ftudy, fhould have confidered the improvement of thofe vegetables which, in their cultivated ftate, afford the largeft portion of fubfiftence to mankind and other animals, as little connected with the object of their purfuit. Hence it has happened that whilft much attention has been paid to the improvement of every fpecies of ufeful animal, the moft valuable efculent plants have been almoft wholly neglected. But when the extent of the benefit which would arife to the agriculture of the country from the poffeffion of varieties of plants which, with the fame extent of foil and labour, would afford even a fmall increafe of produce, is confidered, this fubject appears of no inconfiderable importance. The improvement of animals is attended with much expence, and the improved kinds neceffarily extend themfelves flowly; but a fingle bufhel of improved wheat or peas may, in ten years, be made to afford feed enough to fupply the whole ifland; and a fingle apple, or other fruit tree, may within the fame time be extended to every garden in it. Thefe confiderations have been the caufe of my addreffing the foregoing obfervations to you at this time; for it was much my wifh to have afcertained before I wrote to you, whether in any inftance a fingle plant can be the offspring of two male parents. The decifion of that queftion muft of neceffity have occupied two years, and muft therefore be left to the teft of future experiment.

PHILOSOPHICAL NEWS, &c.

On the Soldering of Glafs.

CIT. Pajot des Charmes, Correfpondent with the Philomathic Society, has laid before this Society, and the Inftitute, fome fmall pieces of glafs compofed of feveral fragments, which he has contrived to join and folder together with fuch firmnefs, that the glafs will rather break on the fide of the joint, than exactly upon it. This operation is equally effectual on fuch pieces as have their fractures either ftrait or winding, fquare or bevel, ftarred, &c. The part where the joint is, is fcarcely vifible, and Cit. Pajot has in fome inftances made it almoft entirely difappear. When it is vifible it merely prefents a fimple thread, which does not break the luminous rays as a crack does.

By means of this procefs, if brought to perfection, a glafs of confiderable value may be obtained, by uniting the fmaller pieces with little expence. As in order to compleat the union it is neceffary to heat the glafs, and to laminate it, thefe operations have the advantage of altering the colour of a glafs of a difagreeable tint, and of caufing a great part of the bubbles which disfigure it to difappear, by making them take an oblong form, which renders

renders them less visible. Lastly, it is possible by this lamination, to increase the size of a glass when it is of sufficient thickness.

Cit. Pajot has not made public the process he has followed in his operations *.

Bulletin des Science, Brumaire, An. 8.

Simple Method of obviating the Resistance of the Air against the Pendulums of Clocks.

The celebrated David Rittenhouse, late president of the American Philosophical Society, has, in the fourth volume of their transactions, described a simple method of removing the effects of the resistance of the air upon the isochronism of pendulums. He remarks, that clocks of this construction have been made to measure time with astonishing accuracy; and if there be still some causes of inequality in their motions, the united efforts of mechanism, philosophy, and mathematics, will probably in time remove them. That the last and least of these causes which perhaps may be worthy of notice, when all others of more importance are removed, is that arising from unequal density of the air, which, by varying the actual weight of the pendulum, must accelerate or retard its motion. The extreme difference he estimates at half a second a day. He observes, that a remedy dependant on the barometer will not be strictly accurate, as the weight of the entire column does not precisely correspond with the density of its base. The simple method he proposes is as follows:

The pendulum consists of an inflexible rod, carrying the ball beneath and continued above the centre of suspension to an equal (or an unequal) distance upwards. At this extremity is fixed another ball of the same dimensions (or greater or less, according as the continuation is shorter or longer) but made as light as possible. The oscillations of this upper ball will be accelerated by its buoyancy by the same quantity as those of the lower would be retarded, and thus, by a proper adjustment, the two effects might be made to balance and correct each other.

Mr. Rittenhouse, with his usual candour, observes, that he has no doubt but that, notwithstanding the plausibility of this theory, there will be some difficulties in the practice. But he is persuaded they will not prove insuperable

* Cit. Swediaur has informed the society that a citizen, of the name of Hollenwegar, had made about 12 or 14 years before, in the presence of Lavoisier, Meunier, and himself, some experiments, by means of which he soldered together, in a firm and almost imperceptible manner, some fragments of cast glass.

Cit. Chaptal also observes, that more than 15 years ago, a decanter of crystal glass was presented to him, the stopper of which was so perfectly united with the neck, that it might be cut into plates, without the line or circle of union being sensibly perceived. The fluid it contained was the *liquor silicum*, and the decanter had remained reversed for a considerable time.

From this fact he conceived the possibility of soldering together two plates of glass, and he offered my ideas on this subject, and shewed the decanter to the audience in a public lecture, and I think by gradually abstracting the dissolving alkali, it might perhaps be possible to unite plates of glass by the precipitated silex. NOTE OF THE EDITOR.

I have lately seen some pieces of glass, of different colours, connected or soldered together in plates by an artist in London. The junctures were excessively neat, some of them in right lines, and some waved; from which last fact I conclude that the cement was capable of supporting ignition and bending of the glass.---W.N.

The

The only experiment he made on the subject, was merely to shew that a pendulum can be made in this manner, which shall vibrate quicker in a dense medium, than in one more rare, contrary to what takes place with common pendulums.

He made a compound pendulum on the above principles, of about one foot in its whole length. This pendulum, on many trials, made in the air 57 vibrations in a minute. On immersing the whole in water, it made 59 vibrations in the same time; shewing evidently, that its returns were quicker in so dense a medium as water, than in the air. When the lower bob or pendulum only was plunged in water, it made no more than 44 vibrations in a minute.

Distillation by the Application of the cooling Mixture.

Mr. C. Wistar, who in the third volume of the American Transactions described the production of a visible vapor from ice, at the melting point, suspended in air reduced to the temperature of 0 Fahrenheit; which he considered as the consequence of a general law of nature, has made some more experiments in illustration of the same law *.

The inference, or law which he drew from those facts was, that the non-elastic vapor does not depend upon any positive quantity or degree of sensible heat in the evaporating body, but upon a relative degree, exceeding that of the atmosphere to which it is exposed; and that it is produced by the passage of heat from the moist body into the contiguous air. If this theory be true, he states as a consequence, that a slow distillation may be performed with the common apparatus by applying cold to the receiver or refrigeratory, without increasing the heat of the retort, or substance to be distilled, as there will be a continual passage of heat from the body, to be evaporated or distilled into the air of the receiver.

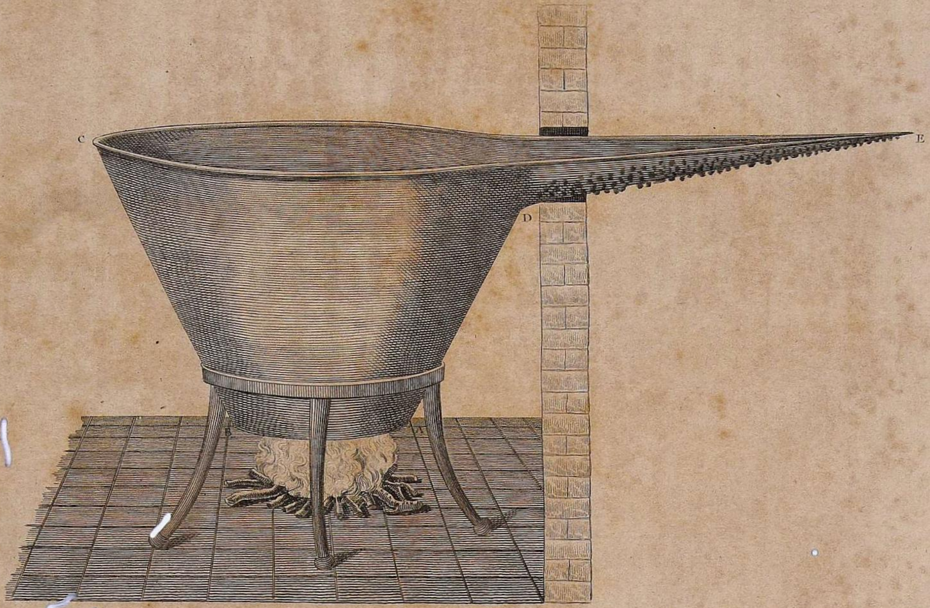
He submitted this conclusion to the test of experiment, because it had been suggested, that the vapor which appeared to arise from ice, might have arisen from the mixture of different portions of air of different temperatures; whereas by distilling and evaporating in a luted retort and receiver, there can be no mixture of warm and cold air; and by using a substance which is not contained in the atmosphere, all suspicion must be removed of the vapor itself having arisen from the included air.

With views of this kind he poured an ounce and half of sulphuric ether into a retort, and luted it to a receiver with a long neck, which was placed in a mixture of salt and snow, while the retort itself was surrounded by the common air at the temperature of 50° Fahrenheit. The frigorific mixture was seldom lower than 10°, so that the difference between the temperatures of the two vessels did not exceed 40°. When the apparatus had been thirty hours in this situation, the frigorific mixture was removed, and one third of the ether was found distilled into the receiver. Another perfectly similar apparatus and charge, was exposed to the air at 50° for thirty hours, but without any application of the freezing mixture. No distillation took place.

The former experiment was varied, by substituting camphor in the place of the ether. The retort stood in air at 50°, and the receiver in the freezing mixture. At the end of thirty hours, some of the camphor was found to be sublimed in the same arborescent form as it usually has when produced by heat.

* American Philos. Trans. IV. 72.

Apparatus for boiling inflammable Fluids.



Mutlow sc.

Resistance of Fluids.

Fig. 1.

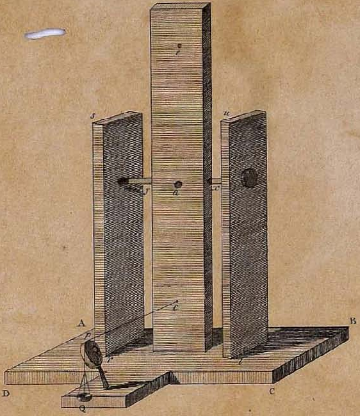
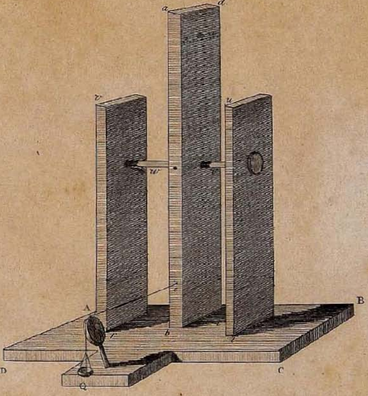


Fig. 2.



Mutlow sc.

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THE ARTS.

MARCH 1800.

ARTICLE I.

Directions for making the best Composition for the Metals of Reflecting Telescopes, and the Method of Casting, Grinding, Polishing, and giving the great Speculum the true parabolic Figure. By the Rev. JOHN EDWARDS, *B. A.*

(Concluded from page 497.*)*

Of polishing the Metal, and giving it the true parabolic Figure.

THE rough grinder, of an *elliptical* form, is now to be covered with common pitch. I generally make my own pitch by boiling tar in a ladle or crucible over a very slow fire, till it becomes of the consistence I require; for a greater nicety is required in the degree of the hardness of the pitch. The harder the pitch is the better figure it will give to the metal, as it does not alter its figure in working as soft pitch does; besides the metal will acquire a lustre upon a polisher moderately hard, so as to shew objects reflected upon it as vivid and as near their natural colour as possible; but if the pitch is too soft, some of its finest particles will always adhere to the face of the metal, and form a very fine and thin cuticle or covering upon its surface. This circumstance is rendered very evident by viewing any white object in the metal (a sheet of white paper for example) when that fine cuticle or thin surface of the pitch upon the speculum will cause it to shew the object of a dingy brown colour,

and not of its genuine whitenefs. Pitch may be eafily made harder by adding a proper quantity of rofin. I often ufe equal quantities of pitch and rofin, fo as to make the mixture juft fo hard, when cold, as to receive an impreffion from a moderate preffure of my nail. A polifher made with pitch and rofin has this advantage, viz. though it is hard, yet it is not fo brittle as when pitch only is ufed, and made hard by boiling it, and confequently not fo liable to break or chip off at the edges, and thereby fcratch the metal. Pour the melted pitch and rofin when pretty cool from the crucible upon the elliptic tool,* fo as to cover it every where when fpread upon it with an iron fpatula, about the thicknefs of a half-crown piece. If the covering is too thin, it will continually alter its figure by the heat it acquires in working the metal upon it, and thereby give a bad figure alfo to the fpeculum. When it is fomewhat cool, lay a piece of writing paper upon the furface of the pitch, prefs the face of the mirror upon it, and you will find the polifher will be nearly figured to the form of the fpeculum. If it has not taken an exact figure every where, which would appear by the fine marks of the grain of the paper upon the pitch, gently warm the furface of the pitch, and repeat the operation as before, until you have formed it of the exact † figure of the metal. With a penknife take away now all the fuperfluous pitch from the edge of the polifher, and with a conical piece of wood form the hole in the middle accurately round: in other words, let the pitchy furface be every where of the exact fize and fhape as the lead tool, which is under it. It may be neceffary to mention, that the hole in the middle of the polifher fhould go quite through the tool, (for a particular reafon) and fhould be made of the fame fize, or fomewhat lefs, than the hole in the middle of the fpeculum. This is a neceffary caution, and indeed I have always found that fmall mirrors, without any hole in the middle, will polifh much better, and the figure will be more correct, if the polifher has a hole in the middle of it. The powder I prefer above all others, to give a moft exquifite luftre, is *colcothar of vitriol*, and not *putty*. *Putty* gives metals a white luftre, or as workmen call it, a filver hue; but good colcothar of vitriol will polifh with a very fine and high black luftre, fo as to give the metal finifhed with it the complexion of polifhed fteel. To know if the colcothar of vitriol is good, put fome of it into your mouth, and if you find it diffolves away it is good; but if you find it hard, and crunch between your teeth, then it is bad, and not well burned. Good colcothar of vitriol is of a deep red, or a deep purple colour, and is foft and oily when rubbed between the fingers; bad colcothar of vitriol is of a light red colour, and feels harfh and gritty. The colcothar of vitriol fhould be levigated between two furfaces of polifhed fteel, and wrought with a little water; when it is worked dry,

* The elliptic tool muft be made pretty warm, or the pitch will not adhere to it.

† When the polifher is brought to its true figure, gently warm it at the fire, and with the edge of a knife divide it into feveral fquares, by preffing the edge of the knife gently upon the pitch; thefe fquares, by receiving the fmall portion of the metal that works off it in polifhing, will caufe the figure of the fpeculum to be more correct, than if no fuch fquares had been made. The polifher may alfo be formed without the writing paper, by dipping the mirror into cold water, and afterwards preffing it upon the furface of the pitch, (when it is fomewhat cool) and by repeating this operation till it has taken the exact figure of the metal.

you

you may add a little more water, to carry it lower down to what degree you pleafe. When the colcothar of vitriol has been wrought dry three or four times, it will acquire a black colour, and will be low enough, or fufficiently fine, to give an exquifite luftre. This levigated colcothar of vitriol I put in a fmall phial, and pour fome water upon it, and afterwards I ufe it for polifhing the metals, in the fame manner that wafhed putty is always directed to be made ufe of for that purpofe. I always put on a large quantity of wafhed colcothar of vitriol at once, fo as to faturate the pitch, and form a fine coating of the colcothar, and very rarely make ufe of a fecond application. If a fecond or third application of colcothar fhould be found neceffary to bring the metal to a high luftre, or to take out any fcratches upon its face, ufe it very fparingly, or you will deftroy the polifh you have already attained. When the metal is nearly polifhed, it will always generate fome black mud upon the furface of the mirror, and alfo upon the tool. Wipe it now away from the face of the metal with fome very foft wafh leather; though if too much of this mud be taken away, it will not polifh fo well. Indeed, a little experience in thefe matters will better fuffice, than a volume written upon the fubject.

In regard to the *parabolic figure* to be given to the metal, no particular caution is required in the polifhing; the *elliptical tool* will always caufe the fpeculum to work into an accurate parabolical figure, fuppofing the tranfverfe and conjugate diameters bear the true proportion to each other, and the metal is not too thick to prevent it always from adhering firmly and uniformly to the polifher. Should the pitch prove too foft, it will give way and alter the figure a little. This circumftance will render the figure of the mirror fometimes a fmall degree fhort of the parabola, and fometimes a very little beyond it; but by a little perfeverance the correct figure is very eafily acquired. I could very eafily give the reader the reafon, why an *elliptical tool* of a proper proportion will always give a parabolical figure; and if the tranfverfe diameter is increafed, it will then always give an hyperbolical figure; but as I am writing upon the practical part of making reflecting telefcopes, and not the theory, I will not offend his patience. To convince any one of the certainty of my affertions, let him polifh a metal of $2\frac{1}{2}$ inches diameter, and $9\frac{1}{2}$ inches focus, upon an *elliptical tool*, whofe diameters are $2\frac{1}{2}$ and 3 inches; and I can affert, he will always find the metal, when polifhed, (if it is not too thick) beyond the parabola, or it will always prove hyperbolical. If he polifhes it upon a *circular* tool in the common way, with crofs ftrokes in every direction poffible, ufing firft a few round ftrokes every time he changes his pofition, he will find it will always prove fpherical, and confequently *fhort* of the parabola. A very little experience in thefe matters, will convince any one of the eafe and certainty of giving the great fpeculum a parabolic figure, by polifhing it in a common manner only with crofs ftrokes in every poffible direction, upon an *elliptical tool* of the proper dimenfions, in which for common foci and apertures, viz. $2\frac{1}{2}$ to $9\frac{1}{2}$ focus, or 3,8 inches in diameter to 18 inches focus, the diameters fhould be 10 to 9. The fhorteft diameter of the ellipfe being accurately the fame as the diameter of the metal, and the longeft diameter of the ellipfe to the fhorteft diameter, as 10 to 9.

Ludlow, July 19, 1781.

JOHN EDWARDS.

APPENDIX.

APPENDIX.

An Account of several Compositions of Metals and Semi-Metals, on which Trials were made to find out the most proper Mixture for the Specula of Reflecting Telescopes. By the Rev. JOHN EDWARDS, B. A.

1. COPPER and tin * equal parts; very bad, soft, and of a blue color.
2. Copper with arsenic $\frac{1}{8}$; but little different from the first.
3. Tin 2, copper 1; much worse than the preceding ones.
4. Copper 32, tin 16, arsenic 4; † black and brittle.
5. Copper 6, tin $1\frac{3}{4}$, arsenic 1; very indifferent.
6. Copper 32, tin 14, arsenic 2; a very good metal.
7. Copper 32, tin $13\frac{1}{2}$, arsenic 1; not quite so good as the sixth.
8. Copper 32, tin $13\frac{1}{2}$, arsenic $1\frac{1}{2}$; a good metal.
9. Copper 32, tin 15, arsenic 2; much better than any of the above.
10. Copper 6, tin 2, arsenic 1; compact, but very yellow when polished.
11. Copper 3, tin $1\frac{1}{4}$; compact, and whiter than the tenth.
12. Copper 32, tin $14\frac{1}{2}$; a pretty good metal, but polishes too yellow.
13. Copper 32, tin 15, arsenic 2, flint glass ‡ in powder 3; very bright, but rotten.
14. Brass 6, tin 1; compact, but too yellow.
15. Two parts of 11th composition, and 1 part of 14 composition; compact, but much too yellow when polished.
16. Brass 5, tin 1; somewhat whiter than the 14th.
17. Brass 4, tin 1; a good metal, but rather yellow.
18. Brass 4, tin 1, with arsenic $\frac{1}{10}$; whiter than 17.
19. Brass 3, tin 1 §; will not polish well.
20. Brass 2, tin 1; of a sparry nature.
21. Tin 3, brass 1; too soft, being only a kind of hard pewter.
22. Brass and arsenic equal parts; a dirty white colour.
23. Brass, copper, and arsenic equal parts; a dingy white.

* By tin, I always mean grain tin.

† The nitre was added to fix the arsenic.

‡ Flint glass was added as a flux. See Shaw's Chemistry, p. 255. Note, the 10th is the composition of Sir Isaac Newton. See Appendix to Gregory's Optics, p. 221. The 11th, 14th, and 15th are the compositions of Mr. Molyneux. See Smith's Optics, Vol. II. p. 304. And the 12th is the composition of Mr. Mudge. See Philosophical Transactions, Vol. LXVII. p. 298.

§ These compositions are mentioned by Neri and Kunckell in Neri's art of glass making. Surely they never tried those compositions themselves, but took them upon the report of other authors; as the 19 will not take a good lustre, and the 21 is very soft, like hard pewter, therefore highly improper for specula, which should be as hard as possible.

24. Brafs and platina, equal parts; very difficult to fufe and mix well together, is then malleable, and of a dingy white colour like 22, compofition.

25. Copper 32, tin 14, crude antimony 4; black and rotten.

26. Copper 32, tin 14, crude antimony 1; blueifh and rough grained.

27. Copper 32, tin 15, arfenic 4, bifmuth 2; much too rotten.

28. Copper 32, tin 15, arfenic 3, bifmuth 1; much too yellow when polifhed, and appears alfo porous.

29. Copper 2, zinc 1; a pale malleable metal.

30. Copper and zinc equal parts; ftill malleable and rough grained.

31. Copper 32, tin 15, arfenic 4, zinc 4; a good metal, but does not take a high luftre.

32. The 31ft compofition fluxed with corrofive fublimate; a compact and hard metal, but rather yellow when polifhed.

33. Copper* 32, tin 16; a moft beautiful, brilliant compofition, but much too brittle and rotten.

34. Copper 32, tin 17; blueifh and rough grained.

35. Copper 32, tin 18; blueifh and rough grained.

36. Brafs 2, zinc 1; nearly of a gold colour.

37. Brafs and zinc equal parts; a pale gold color, and rough grained.

38. Spelter 4, tin 1; very rotten.

39. Copper and crude antimony equal parts; of a fparry nature.

II.

Experiments and Obfervations on Shell and Bone. By CHARLES HATCHETT, *Efq.* F. R. S.

(Concluded from page 506.)

THE bones of fifh, fuch as thofe of the falmon, mackarel, brill, and fkate, afforded phofphate of lime; and the only difference was, that the bones of thefe fifh appeared in general to contain more of the cartilaginous fubftance relative to the phofphate of lime, than is commonly found in the bones of quadrupeds, &c.

The different bones alfo of the fame fifh were various in this refpect; and the bones about the head of the fkate only differed from cartilage by containing a moderate proportion of phofphate of lime.

It is at prefent believed, that phofphate, with fome fulphate of lime, conftitutes the whole of the offifying fubftance; and perhaps the formation of bone from cartilage depends only on the phofphate of lime; but whether this is the cafe or not, it is fit that I

* Unlefs the copper is very pure, this compofition will be of a dark blue colour, as 15 ounces of grain tin will generally faturate two pounds weight of copper.

fhould

ſhould notice a third ſubſtance, which conſtantly occurred in the courſe of my experiments.

When human bones or teeth, as well as thoſe of quadrupeds andfiſh, whether recent or calcined, were expoſed to the action of acids, an effervefcence, although at times but feeble, was produced. This circumſtance at firſt I did not particularly notice; but the following experiments excited my attention:

After the phoſphate of lime had been precipitated from the ſolutions of various teeth and bones by pure ammoniac, I obſerved that a ſecond precipitate, much ſmaller in quantity, was obtained by the addition of carbonate of ammoniac. This ſecond precipitate diſſolved in acids with much effervefcence, during which carbonic acid was diſengaged, and ſelenite was formed by adding ſulphuric acid. Moreover the ſolution of this precipitate did not contain any phoſphoric acid; nor did the liquor, from which the precipitate had been ſeparated, afford any trace of it.

This precipitate was, therefore, carbonate of lime; but I ſtill was not certain that it exiſted as ſuch in the teeth and bones.

Although regular and comparative analyſes of the bones of different animals have not hitherto been made, yet by the experiments of Meſſrs. Gahn, Scheele, Macquer, Fourcroy, Berniard, and the Marquis de Bullion, it has been proved that phoſphate of lime is the principal oſſifying ſubſtance of bones in general, and that this is accompanied by a ſmall proportion of ſaline ſubſtances, and by ſulphate of lime.

I was, therefore, deſirous to aſcertain whether the carbonate of lime, which I had obtained by the above mentioned experiments, had been produced from the ſulphate of lime, decompoſed by the alkaline precipitant, or whether the greater part had not exiſted in the bones in the ſtate of carbonate.

Each of the ſolutions in nitric acid afforded a precipitate with nitrate of barytes; but the quantity of ſulphuric acid thus ſeparated appeared by far too ſmall to be capable of ſaturating the whole of the carbonate of lime obtained from an equal quantity of the ſolution. To prove, therefore, the preſence of the carbonic acid, and the conſequent formation of carbonate of lime, portions of the various teeth and bones were immerſed at ſeparate times in muriatic acid; and the gas produced was received in lime water, by which it was ſpeedily abſorbed, and a proportionate quantity of carbonate of lime was obtained.

Although it appears that the principal effects during oſſification are produced by the phoſphate of lime, yet we here ſee that not only ſome ſulphate, but alſo ſome carbonate of lime enters the compoſition of bones; and it is not a little curious to obſerve, that as the carbonate of lime exceeds in quantity the phoſphate of lime in cruſtaceous marine animals and in the egg-ſhells of birds, ſo in bones it is *vice verſa*. It is poſſible, when many accurate comparative analyſes of bones have been made, that ſome may be found compoſed only of phoſphate of lime; and that thus ſhells containing only carbonate of lime, and bones containing only phoſphate of lime, will form the two extremities of the chain.

I ſhall

I ſhall now make a few remarks on the enamel of teeth.

When a tooth, coated with enamel, is immerſed in diluted nitric or muriatic acid, a feeble effervefcence takes place, and the enamel is completely diſſolved; ſo alſo is the bony part; but the cartilage of that part is left, retaining the ſhape of the tooth. Or if a tooth, in which the enamel is intermixed with the bony ſubſtance, is plunged in the acid, the enamel and the bony part are diſſolved in the ſame manner as before; that is to ſay, the enamel is completely taken up by the acid, while the tooth, like other bones, remains in a pulpy or cartilaginous ſtate, having been deprived of the oſſifying ſubſtance. Conſequently, thoſe parts which were coated or penetrated by lines of enamel are diminiſhed in proportion to the thickneſs of the enamel which has been thus diſſolved; but little or no diminution is obſerved in the tooth*.

Mr. Hunter has noticed this; and ſpeaking of enamel ſays, "when ſoaked in a gentle "acid, there appears no griſly or fleſhy part with which the earthy part had been in-"corporated †."

Now when the difference, which has been lately ſtated between porcellaneous ſhell and mother of pearl, is conſidered, it is not poſſible to avoid the comparing of theſe to enamel and tooth.

When porcellaneous ſhell, whole, or in powder, is expoſed to the action of acids, it is compleatly diſſolved without leaving any reſiduum.

Enamel is alſo completely diſſolved in the like manner.

Porcellaneous ſhell and enamel when burned, emit little or no ſmoke, nor ſcarcely any ſmell of burned horn or cartilage.

Their figure, after having been expoſed to fire, is not materially changed, except by cracking in ſome parts; their external gloſs partly remains, and their colour at moſt becomes grey, very different from what happens to mother of pearl, or tooth. In their fracture they have a fibrous texture; and in ſhort, the only eſſential difference between them appears to be, that porcellaneous ſhell conſiſts of carbonate of lime, and enamel of phoſphate of lime, each being cemented by a ſmall portion of gluten.

In like manner, if the effects produced by fire and acid menſtrua, on ſhells compoſed of mother of pearl, and on the ſubſtance of teeth and bone, are compared, a great ſimilarity will be found; for when expoſed to a red heat,

1ſt. They ſmoke much, and emit a ſmell of burned cartilage or horn.

2ndly. They become of a dark grey, or black colour.

3dly. The animal coal thus formed is of different incineration.

4thly. They retain much of their original figure; but the membranaceous ſhells are ſubject to exfoliate ‡.

* I have alſo obſerved, that when raſpings of enamel are put into diluted nitric, or muriatic acid, they are diſſolved without any apparent reſiduum; but when raſpings of tooth or bone are thus treated, portions of membrane or cartilage remain correſponding to the ſize of the raſpings.

† Natural Hiſtory of the Human Teeth, p. 35.

‡ This is a natural conſequence ariſing from their ſtructure.

5thly.

5thly. Theſe ſubſtances (pearl, mother of pearl, tooth and bone) when immerſed in certain acids, part with their hardening or oſſifying ſubſtances, and then remain in the ſtate of membrane or cartilage.

6thly. When previouſly burned, and afterwards diſſolved in acids, a quantity of animal coal is ſeparated, according to the proportion of the gelatinous, membranaceous, or cartilaginous ſubſtance, and according to the duration of the red heat.

And laſtly, the acid ſolutions of theſe ſubſtances by proper precipitants, afford carbonate of lime in the one caſe, and phoſphate of lime principally in the other, in a proportion relative to the membrane or cartilage with which, or on which, the one or the other had been mixed or depoſited.

As porcellaneous ſhell principally differs from mother of pearl, only by a relative proportion between the carbonate of lime and the gluten, or membrane; in like manner the enamel appears only to be different from tooth or bone, by being deſtitute of cartilage, and by being principally formed of phoſphate of lime cemented by gluten.

The difference in the latter caſe ſeems to explain, why the bones and teeth of animals fed on madder, become red, when at the ſame time the like colour is not communicated to the enamel; for it appears probable that the cartilages, which form the original ſtructure of the teeth and bones, become the channel by which the tinging principle is communicated and diffuſed.

Theſe comparative experiments prove, that there is a great approximation in the nature of porcellaneous ſhell and the enamel of teeth, and alſo in that of mother of pearl and bone; and if a ſhell ſhould be found compoſed of mother of pearl, coated by the porcellaneous ſubſtance, it will reſemble a tooth coated by the enamel, with the difference of carbonate being ſubſtituted for phoſphate of lime.

Some experiments on cartilaginous ſubſtances (which I intended to have inſerted in this paper, but which I am prevented from doing, as they are not as yet ſufficiently advanced) have in a great meaſure convinced me, that membranes and cartilages (whether deſtined to become bones by a natural proceſs, as in young animals, or whether they become ſuch by morbid oſſification, as often happens in thoſe which are aged) do not contain the oſſifying ſubſtance, or phoſphate of lime, as a conſtituent principle. I mean by this, that I believe the portion of phoſphate of lime found in cartilaginous and horny ſubſtances, to be ſimply mixed as an extraneous matter; and that when it is abſent, membrane, cartilage, and horn are moſt perfect and complete.

The frequent preſence of phoſphate of lime in cartilaginous ſubſtances, is not a proof of its being one of their conſtituent principles, but only that it has become depoſited and mixed with them, in proportion to the tendency they may have to form modifications of bone; or according to their vicinity with ſuch membranes or cartilages, as are liable to ſuch a change. If horns are examined, few I believe will be found to contain phoſphate of lime in ſuch a proportion, as to be conſidered an eſſential ingredient. I would not be underſtood to ſpeak here of ſuch as ſtag, or buck horn, for that has every chemical character

of

of bone, with fome excefs of cartilage; but I allude to thofe in which the fubftance of the horn is diftinctly feparate from the bone; and which, like a fheath, covers a bony protuberance, which iffues from the os frontis of certain animals *.

Horns of this nature, fuch as thofe of the ox, the ram, and the chamois, alfo tortoife-fhell, afford after diftillation and incineration, fo very fmall a refiduum, of which only a fmall part is phofphate of lime, that this latter can fcarcely be regarded as a neceffary ingredient.

By fome experiments made on 500 grains of the horn of the ox, I obtained, after a long continued heat, only 1,50 grains of refiduum; and of this lefs than half proved to be phofphate of lime.

78 grains of the horn of the chamois afforded only 0,50 of refiduum; and 500 grains of tortoife-fhell yielded not more than 0,25 of a grain, of which lefs than half was phofphate of lime.

Now it muft be evident, that fo very fmall a quantity cannot influence the nature of the fubftances which afforded it; and the fame may be faid of fynovia, 480 grains of which did not yield more than one grain of phofphate of lime.

This fubftance is undoubtedly various in its proportions, in all thefe and other animal fubftances, arifing probably from the age and habit of the animal which has produced them; but I believe that I may at leaft venture to place fome confidence in the foregoing experiments, as feveral others made fince the above was written, have tended to confirm them †.

In the courfe of making the experiments which have been related, I examined the foffil bones of Gibraltar, as well as fome gloffopetræ, or fhark's teeth. The latter afforded phofphate and carbonate of lime; but the carbonate of lime was vifibly owing principally to the matter of the calcareous ftrata which had inclofed thefe teeth, and which had infinuated itfelf into thefe cavities, left by the decompofition of the original cartilaginous fubftance.

The bones of the Gibraltar rock alfo confift principally of phofphate of lime; and the cavities have been partly filled by the carbonate of lime which cements them together.

Foffil bones refemble bones, which by combuftion have been deprived of their cartilaginous part; for they retain the figure of the original bone, without being bone in reality, as one of the moft effential parts has been taken away. Now fuch foffil, or burned bones,

* Nature feems here to have made an analyfis, or feparation of horn from bone.

† Thefe experiments were repeated on bladders, which I chofe in preference to any other membrane, as not being liable to offification, and therefore likely to contain very little, or no phofphate of lime. 250 grains of dry hog's bladder after incineration left a refiduum, the weight of which did not exceed $\frac{1}{50}$ of a grain. This was diffolved in diluted nitric acid; and upon adding pure ammoniac, fome faint traces of phofphate of lime were obferved. Now as 250 grains of bladder did not afford more than $\frac{1}{50}$ of a grain of refiduum, of which only a part confifted of phofphate of lime; there is much reafon to regard this experiment as an additional proof, that the phofphate of lime is not an effential ingredient of membrane.

can no more be regarded as bone, than charcoal can be considered as the vegetable, of which it retains the figure and fibrous structure.

Bones which keep their figure after combustion, resemble charcoal made from vegetables, replete with fibre; and cartilaginous bones which lose their shape by the same cause, may be compared to succulent plants, which are reduced in bulk and shape in a similar manner.

From these last experiments, I much question if bodies, consisting of phosphate of lime like bones, have concurred materially to form strata of limestone or chalk; for it appears to be improbable, that phosphate is converted into carbonate of lime, after these bodies have become extraneous fossils.

The destruction or decomposition of the cartilaginous parts of teeth and bones in a fossil state, must have been the work of a very long period of time, unless accelerated by the action of some mineral principle; for after having in the usual manner steeped in muriatic acid the os humeri of a man brought from Hythe in Kent, and said to have been taken from a Saxon tomb, I found the remaining cartilage nearly as compleat as that of a recent bone. The difficult destructibility of substances of a somewhat similar nature, appears also from the piercing implements formed of horn, which are not unfrequently found in excavations of high antiquity.

III.

Some Account of the Elastic Gum Vine of Prince of Wales's Island, and of Experiments made on the milky Juice which it produces: with Hints respecting the useful Purposes to which it may be applied. By JAMES HOWISON, *Esq.**

OUR first knowledge of the plant being a native of our *island* arose from the following accident. In our excursions into the forests, it was found necessary to carry cutlasses, for the purpose of clearing our way through the underwood. In one of those an elastic gum vine had been divided, the milk of which drying upon the blade, we were much surprized in finding it possess all the properties of the *American Caout-chouc*. The vine which produces this milk is generally about the thickness of the arm, and almost round, with a strong ash-coloured bark, much cracked, and divided longitudinally; has joints at a small distance from each other, which often send out roots, but seldom branches; runs upon the ground to a great length; at last rises upon the highest trees into the open air. It is found in the greatest plenty at the foot of the mountains, upon a red clay mixed with sand, in situations completely shaded, and where the mercury in the thermometer will seldom exceed summer heat.

* Asiatic Researches, V. 157.

In

In my numerous attempts to trace this vine to its top, I never succeeded; for, after following it in its different windings, sometimes to a distance of two hundred paces, I lost it, from its ascending among the branches of trees that were inaccessible either from their size or height. On the west coast of *Sumatra* I understand they have been more successful; *Dr. Roxburgh* having procured from thence a specimen of the vine in flowers, from which he has classed it; but whose description I have not yet seen.

With us the *Malays* have found tasting of the milk the best mode of discriminating between the elastic gum vine, and those which resemble it in giving out a milky juice, of which we have a great variety; the liquid from the former being much less pungent or corrosive than that obtained from the latter.

The usual method of drawing off the milk is by wounding the bark deeply in different places, from which it runs but slowly, it being full employment for one person to collect a quart in the course of two days. A much more expeditious mode, but ruinous to the vine, is cutting it in lengths of two feet, and placing under both ends vessels to receive the milk. The best is always procured from the oldest vines. From them it is often obtained in a consistence equal to thick cream, and which will yield two thirds of its own weight in gum.

The chemical properties of this vegetable milk, so far as I have had an opportunity of examining, surprisingly resemble those of animal milk. From its decomposition in consequence of spontaneous fermentation, or by the addition of acids, a separation takes place between its *caseous* and serous parts, both of which are very similar to those produced by the same processes from animal milk. An oily or butyrous matter is also one of its component parts, which appears upon the surface of the gum so soon as the latter has attained its solid form. The presence of this considerably impeded the progress of my experiments, as will be seen hereafter.

I was at some trouble in endeavouring to form an extract of this milk so as to approach to the consistence of new butter, by which I hoped to retard its fermentative stage, without depriving it of its useful qualities; but as I had no apparatus for distilling, the surface of the milk, that was exposed to the air, instantly formed into a solid coat, by which the evaporation was in a great degree prevented. I, however, learned, by collecting the thickened milk from the inside of the coats, and depositing it in a jelly pot, that, if excluded from the air, it might be preserved in this state for a considerable length of time.

I have kept it in bottles, without any preparation, tolerably good, upwards of one year; for, notwithstanding the fermentation soon takes place, the decomposition in consequence is only partial, and what remains fluid, still retains its original properties, although considerably diminished.

Not having seen *M. Fourcroy's* memoir on *Caout-chouc*, I could not make trials of the methods proposed by him for preserving the milk unaltered.

In making boots, gloves, and bottles, of the elastic gum, I found the following method the best: I first made moulds of wax, as nearly of the size and shape of what they repre-

ſented as poſſible: theſe I hung ſeparately upon pins, about a foot from the ground, by pieces of cord wrought into the wax; I then placed under each a ſoup plate, into which I poured as much of the milk as I thought would be ſufficient for one coat. Having dipped my fingers in this, I completely covered the moulds one after another, and what dropped into the plates was uſed as part of the next coat: the firſt I generally found ſufficiently dry in the ſpace of ten minutes, when expoſed to the ſun, to admit of a ſecond being applied: however, after every ſecond coat, the oily matter before mentioned was in ſuch quantity upon the ſurface, that, until waſhed off with ſoap and water, I found it impoſſible to apply any more milk with effect; for, if laid on, it kept running and dividing like water upon wax.

Thirty coats I in common found ſufficient to give a covering of the thickneſs of the bottles which come from *America*. This circumſtance may, however, at any time be aſcertained, by introducing the finger between the mould and gum, the one very readily ſeparating from the other.

I found the fingers preferable to a bruſh, or any inſtrument whatever, for laying on the milk; for the moment a bruſh was wet with that fluid, the hair became united as one maſs. A mode which at firſt view would appear to have the advantage of all others for eaſe and expedition in covering clay and wax moulds with the gum, *viz.* immerſing them in the milk, did not at all anſwer upon trial; that fluid running almoſt entirely off, although none of the oily matter was preſent; a certain degree of force ſeeming neceſſary to incorporate by friction the milk with the new formed gum.

When, upon examination, I found that the boots and gloves were of the thickneſs wanted, I turned them over at the top, and drew them off, as if from the leg or hand, by which I ſaved the trouble of forming new moulds. Thoſe of the bottles being ſmalleſt at the neck, I was under the neceſſity of diſſolving in hot water.

The inſide of the boots and gloves which had been in contact with the wax being by far the ſmootheſt, I made the outſide. The gloves were now finiſhed, unleſs cutting their tops even, which was beſt done with ſciſſars. The boots, however, in their preſent ſtate, more reſembled ſtockings, having as yet no ſoles. To ſupply them with theſe, I poured upon a piece of gunny a proper quantity of milk, to give it a thick coat of gum. From this, when dry, I cut pieces ſufficiently large to cover the ſole of the foot, which, having wet with the milk, I applied; firſt replacing the boot upon the mould to keep it properly extended. By this mode the ſoles were ſo firmly joined, that no force could afterwards ſeparate them. In the ſame manner I added heels and ſtraps, when the boots had a very neat appearance. To ſatisfy myſelf as to their impermeability to water, I ſtood in a pond up to their tops for the ſpace of fifteen minutes, when, upon pulling them off, I did not find my ſtockings in the leaſt damp. Indeed, from the nature of the gum, had it been for a period of as many months, the ſame reſult was to have been expected.

After being thus far ſucceſsful, I was greatly diſappointed in my expectations with regard to their retaining their original ſhape; for, on wearing them but a few times, they loſt

loſt much of their firſt neatneſs, the contractions of the gum being only equal to about [illegible] eighths of its extenſion.

A ſecond diſadvantage aroſe from a circumſtance difficult to guard againſt, which was, that if, by any accident, the gum ſhould be in the ſmalleſt degree weaker in one place than another, the effect of extenſion fell almoſt entirely on that part, and the conſequence was, that it ſoon gave way.

From what I had obſerved of the advantage gained in ſubſtance and uniformity of ſtrength, by making uſe of gunny as a baſis for the ſoles, I was led to ſuppoſe, that if an elaſtic cloth, in ſome degree correſpondent to the elaſticity of the gum, were uſed for boots, ſtockings, gloves, and other articles, where that property was neceſſary, that the defects above mentioned might in a great meaſure be remedied. I accordingly made my firſt experiment with *Coſſimbazar* ſtockings and gloves.

Having drawn them upon the wax moulds, I plunged them into veſſels containing the milk, which the cloth greedily abſorbed. When taken out, they were ſo completely diſtended with the gum in ſolution, that, upon becoming dry by expoſure to the air, not only every thread, but every fibre of the cotton had its own diſtinct envelope, and in conſequence was equally capable of reſiſting the action of foreign bodies as if of ſolid gum.

The firſt coat by this method was of ſuch thickneſs, that for ſtockings or gloves nothing farther was neceſſary. What were intended for boots required a few more applications of milk with the fingers, and were finiſhed as thoſe made with the gum only.

This mode of giving cloth as a baſis I found to be a very great improvement: for, beſides the addition of ſtrength received by the gum, the operation was much ſhortened.

Woven ſubſtances, that are to be covered with the gum, as alſo the moulds on which they are to be placed, ought to be conſiderably larger than the bodies they are afterwards intended to fit; for, being much contracted from the abſorption of the milk, little alteration takes place in this diminution in ſize, even when dry, as about one third only of the fluid evaporates before the gum acquires its ſolid form.

Great attention muſt be paid to prevent one part of the gum coming in contact with another while wet with the milk or its whey; for the inſtant that takes place, they become inſeparably united. But ſhould we ever ſucceed in having large plantations of our own vine, or in transferring the *American* tree (which is perhaps more productive) to our poſſeſſions, ſo that milk could be procured in ſufficient quantity for the covering various cloths, which ſhould be done on the ſpot, and afterwards exported to *Europe*, then the advantages attending this ſingular property of the milk would for ever balance its diſadvantages: cloths, and coverings of different deſcriptions, might then be made from this gum cloth, with an expedition ſo much greater than by the needle, that would at firſt appear very ſurprizing: the edges of the ſeparate pieces only requiring to be wet with the milk, or its whey, and brought into contact, when the article would be finiſhed, and fit for uſe. Should both milk and whey be wanting, a ſolution of the gum in ether can always be obtained, by which the ſame end would be accompliſhed.

Of

Of all the cloths upon which I made experiments, nankeen, from the ſtrength and quality of its fabric, appeared the beſt calculated for coating with the gum. The method I followed in performing this, was, to lay the cloth ſmooth upon a table, pour the milk upon it, and with a ruler to ſpread it equally. But ſhould this ever be attempted on a larger ſcale, I would recommend the following plan: to have a ciſtern for holding the milk a little broader than the cloth, to be covered with a croſs bar in the centre, which muſt reach under the ſurface of the milk, and two rollers at one end. Having filled the ciſtern, one end of the piece of cloth is to be paſſed under the bar, and through between the rollers; the former keeping the cloth immerſed in the milk, the latter in preſſing out what is ſuperfluous, ſo that none may be loſt. The cloth can be hung up at full length to dry; and the operation repeated until of whatever thickneſs wanted. For the reaſons above-mentioned, care muſt be taken that one fold does not come in contact with another while wet.

Having obſerved that moſt of the patent catheters and bougies made with a ſolution of the elaſtic gum, whether in ether or in the eſſential oils, had either a diſagreeable ſtickineſs, or were too hard to admit of any advantage being derived from the elaſticity of the gum, I was induced to make ſome experiments with the milk towards removing theſe objections.

From that fluid, by evaporation, I made ſeveral large ſized bougies of pure gum, which, from their over-flexibility, were totally uſeleſs. I then took ſome ſlips of fine cloth covered with the gum, which I rolled up until of a proper ſize, and which I rendered ſolid by ſoaking them in the milk, and then drying them. Theſe poſſeſſed more firmneſs than the former, but in no degree ſufficient for the purpoſe intended. Pieces of ſtrong catgut, coated with the gum, I found to anſwer better than either.

Beſides an effectual cloathing for manufacturers employed with the mineral acids, which had been long a deſideratum, this ſubſtance, under different modifications, might be applied to a number of other uſeful purpoſes in life; ſuch as making hats, great coats, boots, &c. for ſailors, ſoldiers, fiſhermen, and every other deſcription of perſons who, from their purſuits, are expoſed to wet ſtockings; for invalids, who ſuffer from damps; bathing caps, tents, coverings for carriages of all kinds, for roofs of houſes, trunks, buoys, &c.

This extraordinary vegetable production, in place of being injured by water, at its uſual temperature * is preſerved by it. For a knowledge of this circumſtance I am indebted to the *Chineſe*. Having ſome years ago commiſſioned articles made of the elaſtic gum from *China*, I received them in a ſmall jar filled up with water, in which ſtate I have ſince kept them without obſerving any ſigns of decay.

Should it ever be deemed an object to attempt plantations of the elaſtic gum vine in *Bengal*, I would recommend the foot of the *Chittagong*, *Rajmahal* and *Bauglipore* hills, as

* From an account of experiments made with the elaſtic gum by M. Groſſart, inſerted in the *Annals de Chimie* for 1792, it appears, that water, when boiling, has a power of partially diſſolving the gum ſo as to render one part capable of being finally joined to another by preſſure only.

ſituations where there is every probability of ſucceeding, being very ſimilar in ſoil and [illegible]limate to the places of its growth on *Prince of Wales's Iſland.* It would, however, be ad- [illegible] make the firſt trial at this ſettlement, to learn in what way the propagation of [illegible] be moſt ſucceſsfully conducted. A further experience may alſo be neceſ- ſary, to a[illegible]tain the ſeaſon when the milk can be procured of the beſt quality, and in the greateſt quantity, with the leaſt detriment to the vine.

IV.

Miſcellaneous Obſervations relative to the Weſtern Parts of Pennſylvania, particularly thoſe in the Neighbourhood of Lake Erie. By ANDREW ELLICOTT *.

DEAR SIR,

I TAKE the liberty of tranſmitting to you the following miſcellaneous obſervations, collected from my notes, relative to Lake Erie, and the Weſtern Country, the peruſal of which I flatter myſelf will not be unſatisfactory or unintereſting.

The ſituation of this lake is already well known, and therefore a particular topographical deſcription will here be unneceſſary; but a variety of phenomena which attend it, merit a more minute conſideration, and cannot fail to engage the attention of the philoſopher; phenomena which in all probability are common to all large lakes of freſh water.

In the ſummer ſeaſon fogs are ſeldom obſerved on the margin of the lake. The three ſummer months that I reſided at Preſqu' Iſle, no fogs were ſeen during the whole time. The horizon was generally clear, and the ſtars ſhone with remarkable luſtre. The moſt common winds here generally reſemble the ſea and land breezes, in the Weſt Indies. From the end of ſpring till the beginning of autumn, they blow, except at the time of ſtorms, from the lake upon the land during great part of the day, and from the land upon the lake during the night: the change generally takes place between the hours of ſeven and ten in the morning, and about the ſetting of the ſun in the evening. Theſe breezes, alternately blowing in oppoſite directions, render thoſe ſituations contiguous to the lake extremely pleaſant during the heat of the ſummer months, and have moſt probably a very ſalutary influence upon the atmoſphere.

A ſtrong eaſterly wind will occaſion a conſiderable depreſſion, and a ſtrong weſterly wind a conſiderable ſwell of the waters in Preſqu' Iſle Bay. In the former caſe, a portion of the water is driven towards the upper end, and in the latter, towards the lower end of the lake. To theſe cauſes we are to attribute thoſe ebbings, and flowings, which have ſo frequently been miſtaken for regular tides: for a little reflection will convince one, that the moon can have no ſenſible effect upon the waters of the lakes. When the wind ceaſes the waters re-

* Addreſſed to Mr. Robert Patterſon, Vice-Preſident of the American Philoſophical Society, and inſerted in their Tranſactions, Vol. IV. lately publiſhed.

turn

turn to reſtore the equilibrium, and an undulation will be viſible for ſeveral days after thoſe ſtorms, and appears to be but ſlightly affected by the alternate breezes already mentioned.

In the weſtern country, and eſpecially in the neighbourhood of the lakes, dew [illegible] heavy. On the Ohio and Allegany rivers, and their numerous branches, [illegible] common, and of remarkable denſity; they do not however appear to contain any portion of thoſe noxious miaſmata, which are ſo frequently combined with the fogs on the eaſtern ſide of the mountains; nay the inhabitants of Pittſburgh conſider them as poſſeſſed of ſalubrious qualities. From a variety of obſervations I am convinced that the atmoſphere in the weſtern country, and particularly in the vicinity of the lakes, contains a greater quantity of moiſture than in the middle Atlantic ſtates. The wooden works which contained my inſtruments were always uncommonly ſwelled, and frequently very much injured in that country, though conſtantly defended from the rain, and occaſionally expoſed to the ſun. The ivory and wood of my ſectors with braſs joints, always expanded above the metal; this expanſion was not ſudden, but effected by ſlow degrees. Whether this exceſs of moiſture ariſes from the extenſive foreſts which conſtantly preſerve the earth in a ſtate of humidity or from more permanent cauſes, future obſervations muſt determine.

Iron is here more ſuſceptible of ruſt, and braſs ſooner tarniſhed than in the Atlantic ſtates; but this ſuſceptibility of ruſt I obſerved to be greater in the foreſts than in thoſe parts of the country that had been cleared for cultivation, and from theſe circumſtances the probable cauſe is aſcertained.

The ſouthern ſhores of Lake Erie are generally high; in many places they are perpendicular, and various ſtrata of ſtone are conſiderably elevated above the ſurface of the water. The ſtreams which diſcharge themſelves into the lake over theſe ſtrata form a great variety of caſcades of a romantic appearance, which increaſe the beauty of the country, and muſt at ſome future period enhance the value of the lands.

At the lower end of the lake, and for ſome diſtance up it, theſe ſtrata conſiſt of limeſtone intermixed with flint and marine petrifactions, but the other ſtrata are generally ſlate and excellent freeſtone. About Preſqu' Iſle there is but little lime-ſtone to be ſeen, it lies in detached pieces, and is likewiſe interſperſed with flint and marine petrifactions.

In a large extent of country on the weſtern ſide of the Allegany Mountain, the ſtrata of ſtone are horizontally diſpoſed, except in ſome places where that poſition has been changed by the undermining of creeks and rivers. In theſe places where the ſtrata have been deprived of their ſupport, they have fallen from their original poſitions, and therefore deviate from the general rule. This law of nature is eſtabliſhed on the ſouth ſide of Lake Erie, but how far weſt of the mountains the ſame obtains, has never yet been aſcertained. The horizontal poſition of the ſtrata on that lake has a pleaſing effect; the ſofter lamina are worn away by the beating of the waves, the harder remain projected, and at a diſtance reſemble wainſcotting or mouldings.

From the horizontal diſpoſition of theſe ſtrata the following concluſions may be deduced; firſt, that the country has never been diſturbed by thoſe terrible convulſions which a great

part

part of this globe must have experienced at some remote period of antiquity; and secondly, [illegible]at those naturalists are deceived, who suppose that the strata were originally parallel to [illegible] the earth.

[illegible]clude my observations on this subject, I shall take the liberty of adding an account o[illegible] falls of Niagara, which are in some measure connected with the horizontal disposition of the strata in the Western and North Western Country.

This stupendous cataract of water infinitely excels all other natural curiosities of the country, and exhibits a spectacle scarce equalled in grandeur by any object in the physical world. Lake Erie is situated upon one of those horizontal strata in a region elevated about three hundred feet above the country which contains Lake Ontario. The descent which separates the two countries, is in some places almost perpendicular, and the immense declivity formed by these strata occasions both the cataract of Niagara and the great falls of Chenesco. This remarkable precipice generally runs in a south-western direction from a place near the Bay of Toronto on the northern side of Ontario, round the western angle of the lake; from thence it continues its course generally in an eastern direction, crossing the strait of Niagara and the Chenesco river, till it is lost in the country towards the Seneca Lake.

The waters of this cataract formerly fell from the northern side of the slope, near the landing place; but the action of such a tremendous column of water falling from such an eminence, through a long succession of ages, has worn away the solid stone for the distance of seven miles, and formed an immense chasm which cannot be approached without horror. In ascending the road from the landing to Fort Slauser the eye is continually engaged in the contemplation of the awful, and romantic scenes which present themselves, till the transcendent magnificence of the falls is displayed to view, the imagination is then forcibly arrested, and the spectator is lost in silent admiration! down this awful chasm, the waters are precipitated with amazing velocity after they make the great pitch, and such a vast torrent of falling water communicates a tremulous motion to the earth, which is sensibly felt for some poles round, and produces a sound which is frequently heard at the distance of twenty miles. Many wild beasts that attempt to cross the rapids above this great cataract, are destroyed; and if geese or ducks inadvertently alight in these rapids, they are incapable of rising upon the wing again, and are hurried on to inevitable destruction.

The great height of the banks renders the descent into the chasm extremely difficult; but a person after having descended may easily proceed to the base of the falls, and a number of persons may walk in perfect safety a considerable distance between the precipice and the descending torrent, where conversation is not much interrupted by the noise, which is not so great here as at some distance. A vapour or spray of considerable density, resembling a cloud, continually ascends, in which a rainbow is always seen when the sun shines, and the position of the spectator is favourable. In the winter this spray attaches itself to the trees, where it is congealed in such quantities as to divest them of their smaller branches, and

produces a moſt beautiful chryſtalline appearance; a circumſtance which attends the falls of Cheneſeco, as well as thoſe of Niagara.

A ſingular appearance is obſerved at theſe falls, which has never perhaps bee[illegible] by any writer. Immediately below the great pitch a commixture of foa[illegible] puffed up in ſpherical figures, about the ſize of a common haycock. The[illegible] the top, and diſcharge a column of ſpray to a prodigious height; they th[illegible] ſubſide, and are ſucceeded by others, which exhibit the ſame appearances. Theſe ſpherical forms are moſt conſpicuous about midway between the weſt ſide of the ſtreight, and the iſland which divides the falls, and where the largeſt column of water deſcends. This appearance is produced by the aſcenſion of the air, which is carried down by the column of falling water in great quantities to the bed of the river.

The river at the falls is about ſeven hundred and forty three yards wide, and the perpendicular pitch is one hundred and fifty feet in height. In the laſt half mile immediately above the falls the deſcent of the water is fifty-eight feet; but the difficulty which would attend the buſineſs, prevented me from attempting to level the rapids in the chaſm below; though from conjecture, I concluded that the waters muſt deſcend at leaſt ſixty-five feet; and from theſe reſults it appears that the water falls about two hundred and ſeventy-three feet, in the diſtance of about ſeven miles and an half.

I am, Sir, with reſpect,

Your friend,

ANDREW ELLICOTT.

To Robert Patterſon.

V.

An Account of the Pearl Fiſhery in the Gulph of Manar, in March and April 1797. By HENRY J. LE BECK, *Eſq.**

FROM the accounts of the former pearl fiſheries at *Ceylon*, it will be found, that none have ever been ſo productive as this year's. It was generally ſuppoſed that the renter would be infallibly ruined, as the ſum he paid for the preſent fiſhery was thought exorbitant when compared with what had been formerly given; but this conjecture in the event appeared ill founded, as it proved extremely profitable and lucrative.

The farmer this time was a *Tamul* merchant, who for the privilege of fiſhing with more than the uſual number of donies or boats, paid between two and three hundred thouſand *Porto-novo* pagodas, a ſum nearly double the uſual rent.

* Aſiatic Reſearches, IV. 493.

Theſe

Theſe boats he farmed out again to individuals in the beſt manner he could, but for [illegible] ſufficient number of divers ſome of them could not be employed.

[illegible] which commonly began about the middle of *February*, if wind and weather [illegible] this year, for various reaſons, delayed till the end of the month; yet ſo favourable was the weather, that the renter was able to take advantage of the permiſſion granted by the agreement, to fiſh a little longer than the uſual period of thirty days.

The fiſhery cannot well be continued after the ſetting in of the ſouthern monſoon, which uſually happens about the 15th of *April*, as, after that time, the boats would not be able to reach the pearl banks, and the water being then ſo troubled by heavy ſeas, diving would be impracticable; in addition to which, the ſea-weed, a ſpecies of *fucus*, driven in by the ſoutherly wind, and which ſpreads to a conſiderable diſtance from the ſhore, would be an impediment.

Many of the divers, being Roman Catholics, leave the fiſhery on *Sundays* to attend divine ſervice in their church at *Aripoo;* but if either a *Mahomedan* or *Hindoo* feſtival happens during the fiſhing days, or if it is interrupted by ſtormy weather, or any other accident, this loſt time is made up by obliging the Catholics to work on *Sundays*.

The fear of ſharks, as we ſhall ſee hereafter, is alſo another cauſe of interruption. Theſe, amongſt ſome others, are the reaſons that, out of two months, (from February till April) ſeldom more than thirty days can be employed in the fiſhery.

As this time would be inſufficient to fiſh all the banks, (each of which has its appropriate name, both in Dutch and Tamul,) it is carried on for three or four ſucceſſive years, and a new contract annually made till the whole banks have been fiſhed, after which they are left to recover.

The length of time required for this purpoſe, or from one general fiſhing to another, has not yet been exactly determined; it was, therefore, a practice to depute ſome perſons to viſit the banks annually, and to give their opinion, whether a fiſhery might be undertaken with any degree of ſucceſs *?

From various accounts, which I have collected from good authority, and the experience of thoſe who aſſiſted at ſuch examinations, I conjecture, that every ſeven years ſuch a general fiſhery could be attempted with advantage, as this interval ſeems ſufficient for the pearl ſhells to attain their growth: I am alſo confirmed in this opinion, by a report made by a Dutch governor at *Jafnas* of all the fiſheries that have been undertaken at *Ceylon* ſince 1722; a tranſlation of which is to be found in Wolfe's Travels into *Ceylon*. But the ruinous condition in which the divers leave the pearl banks at each fiſhery, by attending only to the profit of individuals, and not to that of the public, is one great cauſe, that it requires twice the above mentioned ſpace of time, and ſometimes longer, for rendering the fiſhing productive. They do not pay the leaſt attention, to ſpare the young and immature ſhells that contain no pearl; heaps of them are ſeen thrown out of the boats as uſeleſs,

* A gentleman, who aſſiſted at one of the laſt viſits, being an engineer, drew a chart of the banks, by which their ſituation and ſize are now better known than formerly.

on the beach between *Manâr** and *Aripoo*; if theſe had been ſuffered to rem[illegible]in in their native beds, they would, no doubt, have produced many fine pearls. It might, [illegible], be adviſeable, to oblige the boat people to throw them into the ſea again, [illegible] leave the bank. If this circumſpection, in ſparing the ſmall pearl ſhells, to p[illegible] breed was always obſerved, ſucceeding fiſheries might be expected ſooner, [illegible]d with ſtill greater ſucceſs: but the neglect of this ſimple precaution will, I f[illegible]ar, be attended with ſimilar fatal conſequences here, as have already happened to the p[illegible]arl banks on the coaſt of *Perſia*, *South America*, and *Sweden*, where the fiſheries are by no means ſo profitable at preſent as they were formerly.

Another cauſe of the deſtruction of numbers of both old and young pearl ſhells, is the anchoring of ſo many boats on the banks, almoſt all of them uſed differently formed, clumſy, heavy, wooden anchors, large ſtones, &c. &c. If this evil cannot be entirely prevented, it might, at leaſt, be greatly leſſened, by obliging them all to uſe anchors of a particular ſort, and leſs deſtructive.

This ſeaſon the *Seewel* bank only was fiſhed, which lies above twenty miles to the weſtward of *Aripoo*, oppoſite to the freſh water rivers of *Mooſalee Modragam* and *Pomparipoo*. It has been obſerved, that the pearls on the north-weſt part of this bank, which conſiſts of rock, are of a clearer water than thoſe found on the ſouth-eaſt, neareſt the ſhore, growing on corals and ſand.

Condatchey is ſituated in a bay, forming nearly a half moon, and is a waſte, ſandy diſtrict, with ſome miſerable huts built on it. The water is bad and brackiſh, and the ſoil produces only a few, widely ſcattered, ſtunted trees and buſhes. Thoſe perſons who remain here during the fiſhery are obliged to get their water for drinking from *Aripoo*, a village with a ſmall old fort, lying about four miles to the ſouthward. Tigers, porcupines, wild hogs, pangolines, or the *Ceylon armadillos*, are, amongſt other quadrupeds, here common. Of amphibia, there are tortoiſes, eſpecially the *teſtudo geometrica*, and various kinds of ſnakes. A conchologiſt meets here with a large field for his enquiries. The preſents which I made to the people employed in the fiſhery, to encourage them to collect all ſorts of ſhells which the divers bring on ſhore, produced but little effect; as they were too much taken up in ſearching after the mother of pearl ſhells to pay attention to any other object. However, my endeavours were not entirely uſeleſs; I will ſpecify here a few of the number I collected during my ſtay: different kinds of *pectines* †, *palium porphyreum*, *ſolen radiatus* ‡, *Venus caſtrenſis*, *Linn.* § *aſtrea hyotis* ‖, *oſtr. Forſkolii*, *oſtr. Malleus* **, *mytilus hirundo Linn.* ††, *ſpondilus crocius*, *pholas puſillus*, *Linn.* ‡‡, *mitra epiſcopalis*, *Linn.*

* *Manara*, properly *Manar*, is a *Tamul* word, and ſignifies a ſandy river, from the ſhallowneſs of the ſea at that place.

† Scallops. ‡ Radiated razor ſhell. § Alpha cockle. ‖ Double cocks-comb.

** Hammer oyſter; theſe were pretty large, but many broken and ſome covered by a calcarious cruſt. It is very probable that, among thoſe, there may be ſome precious *white* ones.

†† Swallow muſcle. ‡‡ The wood piercer.

 lepas

lepas ſtriata Pennanti, (*vide Zool. Brit.*) , *patella tricarinata*, *Linn. bulla perfecta maculata* *, *harpa n[illegible]bilis*, *porcellana ſalita*, *Rumph.* †, *ſtrombus ſcorpio*, and other of inferior kinds. [illegible] the *zoophytes*, many valuable ſpecies of *ſpongiæ*, *corallinæ*, *ſatulariæ*, &c. a great [illegible] ſtars, and other marine productions, that cannot be preſerved in ſpirits, but ſhould be [illegible]ſcribed on the ſpot. Theſe, as well as the deſcription of the different animals inhabiting the ſhells, are the more worthy of our attention, and deſerve farther inveſtigation, as we are yet very deficient in this branch of natural hiſtory.

During the fiſhing ſeaſon, the deſert, barren place, *Condatchey*, offers to our view a ſcene equally novel and aſtoniſhing. A heterogeneous mixture of thouſands of people of different colours, countries, caſts, and occupations, the number of tents and huts, erected on the ſea ſhore, with their ſhops or bazars before each of them; and the many boats returning on ſhore in the afternoon, generally richly laden; all together form a ſpectacle entirely new to an *European* eye. Each owner runs to his reſpective boat as ſoon as it reaches the ſhore, in hopes of finding it fraught with immenſe treaſure, which is often much greater in imagination than in the ſhell; and though he is diſappointed one day, he relies with greater certainty on the next, looking forward to the fortune promiſed him by his ſtars, as he thinks it impoſſible for the aſtrological predictions of his *Brâhmen* to err.

To prevent riot and diſorder, an officer with a party of *Malays* is ſtationed here. They occupy a large ſquare, where they have a field piece and a flag ſtaff for ſignals.

Here and there you meet with brokers, jewellers, and merchants of all deſcriptions; alſo, ſuttlers offering proviſions and other articles to gratify the ſenſual appetite and luxury. But by far the greater number are occupied with the pearls. Some are baſely employed in aſſorting them; for which purpoſe they make uſe of ſmall braſs plates perforated with holes of different ſizes; others are weighing and offering them to the purchaſer; while others are drilling or boring them; which they perform for a trifle.

The inſtrument, theſe people carry about with them for this purpoſe, is of a very ſimple conſtruction, but requires much ſkill and exerciſe to uſe it; it is made in the following manner: the principal part conſiſts of a piece of ſoft wood, of an obtuſe, inverted, conical ſhape, about ſix inches high and four in diameter in its plain ſurface; this is ſupported by three wooden feet, each of which is more than a foot in length. Upon the upper flat part of this machine are holes, or pits, for the larger pearls, and the ſmaller ones are beat in with a wooden hammer. On the right ſide of this ſtool, half a cocoa nut ſhell is faſtened, which is filled with water. The drilling inſtruments are iron ſpindles, of various ſizes, adapted to the different dimenſions of the pearls, which are turned round in a wooden head by a bow. The pearls being placed on the flat ſurface of the inverted cone, as already mentioned, the operator ſitting on a mat, preſſes on the wooden head of his inſtrument with the left hand, while, with his right, he moves the bow which turns round the moveable part of the drill; at the ſame time, he moiſtens the pearl, occaſionally dipping the little

* Diving ſnail, (Grew, Muſ.) † Salt-coury, Kl.

finger

finger of the ſame hand into the water of the cocoa nut ſhell, with a dexterity that can only be attained by conſtant practice.

Amongſt the crowd are found vagabonds of every deſcription, ſuch as *Pandara*[illegible] or *Hindu* monks, fakirs, beggars, and the like, who are impertinently troub[illegible] o of theſe wretches particularly attracted the attention of the mob, though their [illegible]perſtitious penance muſt have diſguſted a man of the leaſt reflection: one had a gridiron, of one and a half foot long, and the ſame in breadth, faſtened round his neck, with which he always walked about, nor did he take it off either when eating or ſleeping; the other had faſtened round that member, which decency forbids me to mention, a braſs ring, and fixed to it was a chain, of a fathom in length, trailing on the ground; the links of this chain were as thick as a man's finger, and the whole was exhibited in a moſt ſcandalous manner.

The peſtilential ſmell occaſioned by the numbers of putrifying pearl fiſhes, renders the atmoſphere of *Condatchey* ſo inſufferably offenſive when the ſouth-weſt wind blows, that it ſenſibly affects the olfactory nerves of any one unaccuſtomed to ſuch cadaverous ſmells. This putrefaction generates immenſe numbers of worms, flies, muſkitoes, and other vermin; all together forming a ſcene ſtrongly diſpleaſing to the ſenſes.

Thoſe who are not provided with a ſufficient ſtock of money ſuffer great hardſhips, as not only all kinds of proviſions are very dear, but even every drop of good water muſt be paid for. Thoſe who drink the brackiſh water of this place are often attacked by ſickneſs. It may eaſily be conceived what an effect the extreme heat of the day, the cold of the night, the heavy dews, and the putrid ſmell, muſt have on weak conſtitutions. It is, therefore, no wonder that of thoſe who fall ſick many die, and many more return home with fevers, fluxes, or other equally fatal diſorders.

The many diſappointments, uſually experienced by the lower claſſes of men in particular, make them often repent of their coming here. They are often ruined, as they riſk all they are worth to purchaſe pearl ſhells; however, there are many inſtances of their making a fortune beyond all expectation. A particular circumſtance of this kind fell within my own obſervation: a day labourer bought three oyſters * for a copper fanam (about the value of two-pence) and was ſo fortunate as to find one of the largeſt pearls which the fiſhery produced this ſeaſon.

The donies appointed for the fiſhery are not all procured at *Ceylon*; many came from the coaſts of *Coromandel* and *Malabar*, each of which has its diſtinguiſhing number. About ten o'clock at night a gun is fired as a ſignal, when they ſail from *Condatchey* with an eaſterly or land wind, under the direction of a pilot, If the wind continues fair, they reach the bank before day, and begin diving at ſun riſe, which they continue till the weſt or ſea breeze ſets in, with which they return. The moment they appear in ſight, the colours are

* The *Eaſt India* pearl ſhell, is well known to be the *matrix perlarum* (mother of pearl) of Rumphius, or the *Mytilus margaritiferus* of Linnæus; conſequently the general term pearl oyſter muſt be erroneous; however, as it has long been in common uſe, I hope to be excuſed for continuing it.

hoiſted

hoiſted at the flag ſtaff, and in the afternoon they come to an anchor, ſo that the owners of the boats are thereby enabled to get their cargoes out before night, which may amount to 30,000 oyſters, if the divers have been active and ſucceſsful.

Each boat carries twenty-one men and five heavy diving ſtones for the uſe of ten divers, who are called in *Tamul, kooly kârer*, the reſt of the crew conſiſts of a tandel, or head boatman, and ten rowers, who aſſiſt in lifting up the divers and their ſhells.

(To be continued.)

VI.

Deſcription of the Air Furnace of Cit. Lecour.

THE air furnace, of which a ſection is given in Plate XXIII. Fig. 2, was uſed in the experiments deſcribed page 134 of our Journal, and is taken from the ſame report of Guyton. It is conſtructed of brick; its fire-place being 25 centimeters ſquare ($9\frac{3}{4}$ inches) within, and 45 ($17\frac{1}{2}$ inches) in height. It is cloſed on one ſide by an iron cover, inclined about 25 degrees. The chimney, which is alſo of brick, is 25 centimeters ſquare below, and one-fifth leſs at top, namely at an elevation of 13 decimeters (about 4 feet 3 inches) where it enters into a large chimney, about 15 metres (or near 5 feet) high, the exceſs of which is cloſed by a kind of door.

In this furnace the heat was raiſed to about 150 pyrometree degrees of Wedgwood.

VII.

On the Lamp for Tallow, and the Combuſtion of that Material. By Mr. William Close.

To Mr. NICHOLSON.

SIR,

SOON after the deſcription of the lamp for burning tallow was ſent to you, I made a little addition to the apparatus, which makes it more convenient.

For an appendix to the preceding detail, I ſend you an account of the new arrangement,

And am, Sir, your humble ſervant,

WILLIAM CLOSE.

Dalton, Jan. 25*th*, 1800.

The

The cup C, in Plate XVI. is made without the tube *g* in the inſide, and the thumb ſcrew is fixed near its bottom.

Another cup, about three quarters of an inch deep, is made to ſlide eaſily into the cup C: the brim of it is ſurrounded by a ring which projects outwardly, and to the bottom is ſoldered the tube *f*.

The cup B is ſoldered to the tube *f*, as before, and when detached from the reſt of the apparatus, is accompanied by the additional cup, which ſerves it for a bottom or ſtand, and for the receptacle of the ſuperfluous tallow.

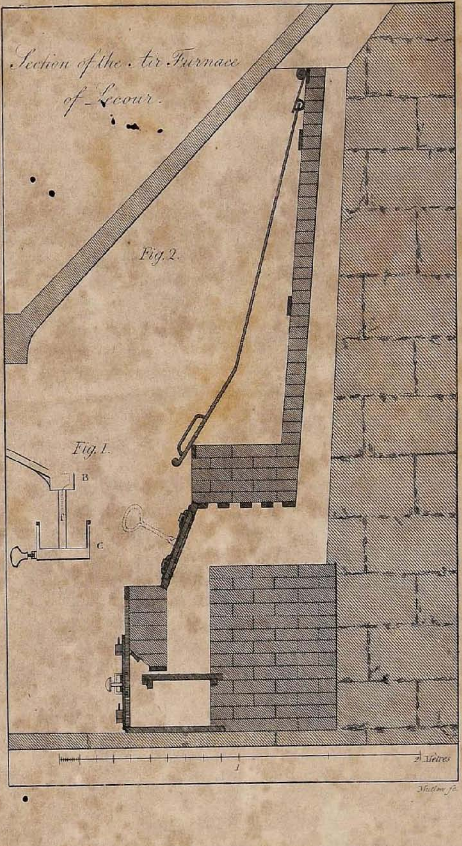
Laſtly, when B is put into its proper place, the ring which ſurrounds the interior cup reſts upon the brim of the cup C, and prevents the bottom from touching the tallow which drops into C when B is removed.

Section of the cups B, C, Fig. 1, Plate XXIII. and the interior cup, intended only to illuſtrate the deſcription. I have not yet ſeen your laſt Number: I have bought rendered hogs' lard at 6d. a pound lately at this place. It is now about 7d. Sheep's ſuet at 7d. Tallow ſold laſt year at about 7s. 6d. a ſtone. Train oil is about 5d. a pint.

Farmers and country people conſume a great quantity of impure tallow in *ruſh candles*. Tallow is alſo burnt in a common cylindrical drinking glaſs, in ſome places the wick is ſupported upon a piece of ſplit ſtick, the lower end of which is thruſt into ſome ſoft ſubſtance put into the bottom of the glaſs; a quantity of water is then poured in, and the remainder filled with melted tallow.

This is but an inconvenient method of burning tallow: the light is much dimmed by the ſides of the glaſs covered with oil; it requires ſnuffing, which is not eaſy to effect; and the light is very unſteady from currents of air paſſing over the top of the glaſs.

END OF THE THIRD VOLUME.



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TO THE

THIRD VOLUME.

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